Decision-Making in Higher Education

The Impact of Information on Study Program Decisions and How It Relates to Dropout

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Chapter 1: Introduction

Educational decisions severely influence life trajectories. They create and restrict opportunities by opening and closing doors along the educational and subsequent occupational pathway. Especially the decision for a specific higher education study program and whether to successfully complete the study program can have a profound impact on students' later life outcomes. For this reason, these decisions need to be thoroughly understood.

People invest time, effort and money into a university degree program, expecting a return on their investment in the future (Becker, 1962; Mincer, 1958). An expectation driven by education's lure that those who pursue it may gain high status, in form of prestigious jobs and large financial returns (Ammermueller & Weber, 2005; Boarini & Strauss, 2010; Oreopoulos & Petronijevic, 2013). Aside from monetary returns, more education is also an opportunity for individuals to deepen their passions, focus their drive (Oreopoulos & Salvanes, 2011) and develop healthy habits (Brunello et al., 2016). It comes as no surprise then that university enrollment rates in Germany are steadily on the rise (Destatis, 2020, page 8). However, most benefits higher education provides can only be reaped after successfully completing a study program. The most recent dropout rate in Germany for Bachelor programs for all higher education institution is estimated to be 27%. The dropout rate even reaches 32% for Bachelor programs offered by universities (Heublein et al., 2020). That is to say about one in three students who enrolls into university does not gain a diploma and the benefits it was supposed to provide.

Not completing a study program by dropping out or changing the degree program can have negative consequences for the individual and society as a whole (Höschler & Backes-Gellner, 2017; Neugebauer et al., 2019; Schneider & Yin, 2011). Since higher education is a self-investment that costs time, effort and money, students who leave tertiary education without a diploma will not have the same returns on these investment as their peers who completed their studies. Students who drop out of higher education may also experience disadvantages in the labor market, such as less prestigious jobs and lower income (Matkovic & Kogan, 2012; Neugebauer et al., 2019; Scholten & Tieben, 2017). From a societal standpoint, people who start a university degree participate in the labor market later and as such pay taxes later. This is a worthwhile time delay in taxation for the state, if people successfully complete their studies and fill a specialized niche or a qualified position later on. But if the investment does not yield a return, the state and industries reliant on people who choose a different path than higher education, such as vocational training, will have forgone potential gains (Schneider & Yin, 2011).

Research shows that one way for students to achieve the outcomes they seek is by assuring a good fit between students' skills and passions and the content of a study program (Brown & Lent, 2016; Hazari et al., 2010; Holland, 1959; Rocconi et al., 2020). However, the choice of study program is everything but easy. In Germany, the number of bachelor programs students can choose from steadily increased from 4108 in the winter semester of 2007/2008 to an astonishing 9168 in the winter semester 2020/2021 (HRK, 2020). How can students make informed, or better yet approximately optimal, study program decisions when there are so many study programs to choose from? Within this sea of possibilities, the only way not to drown is to use heuristics and reduce the mental costs of information gathering (Chaiken & Trope, 1999; Logan, 1980; Simon, 1955; Tversky & Kahneman, 1973), ultimately increasing the likelihood that students make choices based on information deficits (Barone et al., 2017) and potentially wrong beliefs (Morgan, Leenman, et al., 2013). When making study program decisions based on convenient and easily accessible information, the choice set of study programs will be small and unanticipated consequences follow. For example, seeing that many students focus predominantly on a small set of well-known (Destatis, 2020; OECD, 2020) or gender typical study programs (Hägglund & Lörz, 2020; Jonsson, 1999; Morgan, Gelbgiser, & Weeden, 2013), the question arises whether information deficits are partly to blame for this phenomenon. Similarly, could the empirically established trend towards educational outcome homogeneity in networks (e.g. Bifulco et al., 2014; Fletcher, 2015; Kretschmer et al., 2018; Poldin et al., 2015), such as students studying something similar as their friends (Poldin et al., 2015), be partly explained by information diffusion processes in small and dense networks?

If the answer to these questions really is related to information deficits, there is a straightforward answer: get rid of information deficits by providing high quality and personally tailored information to students! Individuals can only make decision based on the information they have available. If students are not aware of a study program that fits their interest and skills well, then they will simply not consider and choose it. Similarly, if students have inaccurate or wrong information on a study program, they may prematurely exclude that study program, albeit it may be skill and interest congruent. The argument thus is that providing students with accurate, relevant and novel information should enable them to make better study program decisions. Yet, an informed and well thought out decision for a study program does not by itself guarantee that the chosen study program is completed successfully. While studying a lot can change and even an approximately optimal decision does not keep students from dropping out. The factors that affect dropout *while* enrolled in higher education, such as integration into the social and academic aspects of the higher education system (Spady, 1971; Tinto, 1975), need to be understood as well. In order to develop effective measures to help students make suitable educational decisions, causal research on educational decision making is pertinent. Consequently, trying to assess the impact of information on study program decision or the factors influencing dropout decisions with more or less sophisticated correlation studies using secondary data will not stand up to the high standard of causality. Following the famous motto "… no causation without manipulation" (D. B. Rubin, 1975, p. 238), what I present within my dissertation to ascertain the validity of these connections is experimental evidence.

The dissertation is set up as follows. In the first chapter of my dissertation, I set the theoretical stage for all three of my papers by describing educational decision making according to rational choice theory. Examining the study program and dropout decision through the lens of rational choice theory unveils the connection between them and shows how understanding the study program decision can help us understand the dropout decision. After the stage has been set, I first introduce the data set for the papers, followed by extended summaries of all my papers where I specifically focus on how the explanandum of each paper can be framed within rational choice theory and the benefits and insights such an integration provides. Chapter 2-4 are the papers of my dissertation. The first two papers of my dissertation present evidence from a randomized controlled trial of a student counseling workshop and its impact on students' field of study intentions and decisions. More specifically, the first paper (Chapter 2) of my dissertation focuses on the impact of an information intervention on field of study *intentions* of well-known and gender typical study programs, while the second paper (Chapter 3) focusses on the effect the information intervention exerts on *intended* field of study homogeneity and *chosen* field of study homogeneity in close friend networks. Both studies yield important insights for educational decision-making in general and the impact of information on study programs in particular. Furthermore, they provide causal evidence on the efficacy of student guidance counseling, deepen our understanding on homogeneity formation processes in field of study intentions and choices, and can help explain why students tend to concentrate on well-known and gender typical study programs. The third paper (Chapter 4) of my dissertation provides evidence from a vignette experiment, on the circumstances under which students drop out of a study program based on Tinto's (1975) student dropout model. This paper focusses on the factors *within* higher education that can increase dropout propensity and provides an important extension to the first two papers of my dissertation which focus on the decision *before* enrollment into higher education. This type of research is pivotal for devising policies aiming to reduce student dropout by uncovering which factors increase dropout propensity. The penultimate Chapter of my dissertation is a discussion (Chapter 5) where I focus on limitations, potential explanations and the implications the findings have for theory and practice. Finally, the dissertation ends with a short summary and conclusion (Chapter 6).

1.1 Educational Decision-Making – The RC Account

To embed the interplay of study program intention, choice, information and dropout into a wider theoretical framework, educational decision-making needs to be examined in detail. One of the most influential decision-making frameworks of the past decades has been rational choice theory (RC). Although many RC variants have been developed, some with strict definitions and assumptions, others with less strict assumptions (Opp, 1999; Simon, 1955), at its core RC posits that individuals have preferences and beliefs, are subject to different constraints and opportunities and try to choose actions that maximize their utility, i.e., satisfy their preferences as much as possible given their opportunities and constraints (Kroneberg & Kalter, 2012; Opp, 1999). Within educational decision making, RC says that given the set of all possible courses of action (Ω), students should choose the behavioral alternative (i), such as a college major, that provides them with the greatest utility (U) given costs (C), benefits (B) and probability of success (P). Formally, a simple utility function

EQ 1. $$U_i = P_i \cdot B_i - C_i$$

may be developed for each (i), whereby the behavioral function with the highest utility ($\max_{i \in \Omega} U_i$) should be chosen in the end.

This RC application of educational decision making was pioneered by Erikson and Jonsson (1996) as well as Breen and Goldthorpe (1997), who proposed an RC based educational decision-making model to explain class differentials and educational inequality. Although RC in educational decision making has a long tradition (e.g. Becker, 1962; Boudon, 1974; Mincer, 1958), the RC account best applicable to study program and drop out decisions in Germany is the one described above by Erikson and Jonsson (1996) (for a RC application of college major choice in the US see, for example, Wiswall & Zafar, 2015) . Erikson and Jonsson (1996; Jonsson & Erikson, 2000) posit that the set of alternatives (i) is further restricted by two conditions, namely risk aversion and feasibility. First, students only contemplate the set of feasible options (F) and discard unfeasible options, such that $i \in F$, whereby $\Omega \supseteq F$. Second, individuals vary in their risk aversion and only chose options where their perceived risk (r_i), whereby $r_i = (1 - P_i) \cdot C_i$, of alternative i is lower than the maximum risk they are willing to take (R), such that $R \geq r_i$. While it is not assumed that people have precise values for P, B and C, they should be able to rank order the U_i of different alternatives, such that the preferred choice is the one where U_i is highest ($\max_{i \in F} U_i$).

Everyone has their own perceived P, B and C, although the magnitude of the parameters are uncertain, drawing close similarities to SEU (subjective expected utility) approaches (e.g. Fishburn, 1981). To illustrate this point and make the educational decisions of my dissertation more transparent, let us consider two high school students, who think about majoring in a STEM (Science, Technology, Engineering and Math) field (U_{stem}) or in the field of humanities (U_{human}). For simplicity, let us assume that the set of feasible alternatives F only contains these two options, such that $F = \{stem, human\}$. The first high school student is taking an advanced math course, has good grades in most natural science topics and a vivid interest in science in general, while the second student is struggling and generally not interested in math and natural sciences but succeeds at his or her language courses. When considering a STEM major, the first student is likely to perceive his or her probability of success (P) in a math heavy STEM major as high, whereas the second student might not. The benefits (B) both students can reap from majoring in STEM are to some extent similar since they should receive the same labor market opportunities after successfully graduating. But more subjective benefits, such as identification with the course content and interest in the topic, may be very different for each. The first student may have a high personal fit with the major, while the second might not. Reversely the second student might flourish in the field of humanities, while the first student may not like it. Although the objective costs (C) of a STEM and humanities major could be the same for both students (for example when considering the same German university) their ease of financing their time in higher education can be quite different, depending on the amount of financial resources at their disposal. Additionally, depending on which university a student intends to enroll in, the costs of moving to and living in the city the university is located in can vary greatly across Germany. Students also have more subjective costs of enrolling in a study program and university in general, such as their perceived cost of leaving friends, significant others and family behind to study in a different city. Given these considerations, the first student may rank the utility of a STEM major higher than the utility of a humanities major ($U_{stem} > U_{human}$), while the second ranks the utilities in the exact opposite order ($U_{stem} < U_{human}$). As such, RC would predict that the first student's choice would be a STEM major (since $\max_{i \in F} U_i = U_{stem}$) and the second student's choice would be a humanity major (since $\max_{i \in F} U_i = U_{human}$).

Attending university and choosing a study program are only the latest set of educational decisions in a long line of educational decisions that came prior to it. Educational attainment, although marked by clear and succinct events, such as receiving a high school or bachelor diploma, is better understood as a cumulative process of educational decisions that paved the way to these events (Boudon, 1974; Hillmert & Jacob, 2010). These decisions are both small, such as the choice of advanced subject, or large, like going to university or starting vocational training. At first, parents play a decisive role in educational decision making, since they enroll their children in kindergarten, primary school and secondary school (Pietsch & Stubbe, 2007; Stocke, 2007). Later, more independent educational decisions are made by students, such as transitioning to upper secondary education, starting a job, vocational training or applying for university (P. N. Blossfeld et al., 2015). Only after deciding for a university education, the more specific study program decision is made and even later, students have to contemplate whether to continue, change or drop out of the chosen degree program (Hillmert & Jacob, 2010). As such, each educational decision at any time D_t is enabled and restricted by all previous educational decisions ($D_{t-n} \dots D_t$) (see also Breen & Jonsson, 2000). To put this idea into the context of the utility function (*EQ1*), we can say that the parameters P, B and C of each utility function are influenced by all previous choices. In the aforementioned example of the two students thinking about enrolling in either a STEM or humanities major, we already saw how the past choice of their advanced courses could influence their perceived probability of success in a STEM major. Similarly, the perceived benefit in terms of interest fit and self-fulfillment of a STEM major is shaped by the extent to which previous decisions influenced the building and maintenance of interests in, e.g., sciences or humanities. The case that past decision influence current ones will become especially important again when I describe the dropout decision and how it relates to the study program decision.

1.1.1 The Study Program Decision and the Impact of Information

The first two papers of my dissertation (Chapter 2 and 3) evaluate how information affects intended choices and actual choices of study programs. Hence, let me illustrate how information can affect utility functions (U_i) and $\max_{i \in F} U_i$.

Within the model of educational decision making of Erikson and Jonsson (1996), information exerts influence on the perceived P, B and C of the utility function. For example, students have different perceived probabilities of success for a study program based on the knowledge they possess on the difficulty of said study program and the accuracy of their self-evaluation. Similarly, they can only assess the benefit of a study program, such as interest-fit and future career prospects, according to the information they possess about the content and job prospects of the study program. Finally, the information students possess on the costs of tertiary education and specific study programs can also be inaccurate or lacking (Barone et al., 2017), resulting in inferior educational decisions (Usher, 2005). In fact, most experimental research on the effectiveness of information treatments in higher education focuses on improving the information accuracy concerning monetary constraints in C or monetary benefits in B of tertiary education as a whole or specific study programs (Barone et al., 2017; Ehlert et al., 2017; French & Oreopoulos, 2017; Kerr et al., 2020; McGuigan et al., 2016).

An extension that may be implicit in the Erikson and Jonsson (1996; Jonsson & Erikson, 2000) model but should be made explicit when assessing the role of information on educational decision making, is the extent to which information influences the set of study programs under consideration. While the choice of study program was already restricted to feasible study options only, such that $\Omega \supseteq F$ with $F \in i$, and those where the risk was lower than the maximum risk students are willing to take $R \geq r_i$, the set of feasible study programs is itself a subset of study programs a student has information on or is aware of (I), such that $\Omega \supseteq I \supseteq F$. To explicitly state this is useful because students may not develop a utility function for some study programs, simply because they are not aware of their existence. Similarly, people are only capable of contemplating a finite number of feasible alternatives. Even if the set of study programs students know about is very large, they cannot contemplate all these study programs at once, due to natural limitations of human cognitive capacity. Therefore, only the subset of accessible (Chaiken & Trope, 1999; Tversky & Kahneman, 1973) study programs (A) are likely to be given full attention, such that $I \supseteq A$, which is why fringe study programs about which little is known may be disregarded too soon. Formally, we can define the relationship between these sets as

EQ 2. $$\Omega \supseteq I \supseteq A \supseteq F$$

Whereby only the utility functions U of study programs i are considered from the set of feasible alternatives ($i \in F$).

Information can influence the set size of I, A and F through different means. For example, students may receive novel information about study programs they have never heard of increasing the set size of I, or information may just increase the accessibility of a study program heard of but previously not under consideration, thus including it in set A. Obviously, simply increasing elements in I or A does not necessarily increase the set size of F, since the study programs still need to be deemed feasible. But if students have new information on study programs and these programs are kept accessible, some study programs are likely to end up in F and in turn may result in a new $\max_{i \in F} U_i$. In a more direct manner, information deficits and wrong information about the content of a study program may also result in wrongfully deemed feasible and unfeasible study programs F, a mistake that can be corrected when these information deficits are resolved. In other words, after information deficits on study programs are overcome the elements of F are a better representation of truly feasible study options.

In sum, information within the RC framework of educational decision making can influence the choice of study program by virtue of affecting parameters of the utility function P, B and C as well as by increasing the set size of considered alternatives in F, I or A.

But information comes with a cost, the cost of gathering and consideration (Simon, 1955). Students will need to expend mental resources, time and sometimes even money (such as the costs of attending a workshop or visiting a counselor) to acquire and consider new information on study programs. Social psychological research suggests that people tend to minimize their mental load and be content with satisfactory, rather than optimal behavioral alternatives by relying on heuristics (Chaiken & Trope, 1999; Logan, 1980; Tversky & Kahneman, 1973).

Dual process models (Chaiken & Trope, 1999; Kahneman, 2003) and the action models based on them (for example, model of frame selection (Kroneberg, 2014)) give a clue as to when new information influences decision making and when it does not. In a nutshell, dual process theories suggest that there are two cognitive systems: (1) an intuitive, fast paced, automatic, heuristic and effortless mode and (2) a reasoned, slow, controlled and effortful mode. The RC account of educational decision-making fits well into the second cognitive system. But do educational decisions elicit activation of the first or second system? Which mode people use for decision making and information processing depends, among other things, on time availability, mental capacity, motivation, willingness to expend effort and accessibility of heuristics fit for a given situation (Chaiken & Trope, 1999). If time, mental capacity and the willingness to expend effort are high and the availability of heuristics is low, the reasoned mode is more likely to be dominant, whereas the first system may become dominant, if the opposite is the case.

For most students, the decision for a specific study program should be dominated by the second, more deliberate system for multiple reasons. First, the risks and potential rewards of the educational decision are high, which should increase students' motivation and willingness to expend effort. Second, the educational decision is generally not subject to severe time constraints, and the cognitive capacity of individuals is not otherwise restricted. However, a factor that may still tip the scale towards activation of the first system is the *accessibility* of mental constructs. Similar to the accessibility of information on a study program, high accessibility of a mental construct, such as enrolling in a specific study program (engrained, for example, by repeated exposure to the study program as I will argue in Chapters 2 and 3), could lead students to automatically and prematurely choose an easily accessible study program, without properly engaging in information deliberation within the second system. But as long as students are open to information deliberation and willing to revise their readily accessible mental constructs, new information should still be able to make a difference.

Given the theoretical background on when and how information may impact educational decision making, we should expect information to influence decision making if (1) the information students possess on study programs is inaccurate or incomplete, (2) students are willing to improve the accuracy of the information they have, (3) the costs of considering and gathering the information is not too high and (4) they have not reached a satisfactory decision yet or are willing to revise their decision.

1.1.2 The Dropout Decision

The third paper of my dissertation (Chapter 4) assesses the factors within higher education that influence students' dropout decision. Within the RC framework, modeling the dropout decision is straight forward (for a similar application see Breen & Goldthorpe, 1997). Students are said to weigh the utility of leaving tertiary education (U_g) against the utility of completing it (U_c), whereby each student's utility of U_g and U_c is once again a function of probability of success P, benefit B and costs C (see *EQ1*). The set of behavioral alternatives (D) within this simplified model only contains two elements i, such that $D = \{g, c\}$. The decision a student makes from this set of behavioral alternatives is the one with the highest utility ($\max_{i \in D} U_i$).

There are some interesting implications that can be made when framing the dropout decision within an RC framework, some of which are also explored by Breen and Goldthorpe (1997) as well as Erikson and Jonsson (1996). First, the benefit of completing an educational path should, in most cases, be greater than the benefit of leaving the chosen educational path ($B_c > B_g$). This is because, in general, the potential return of completing higher education outweighs that of dropping out of higher education. Of course, the benefit of completing tertiary education is dependent on the chosen study program, since not all study programs yield the same labor market returns. Additionally, the benefit of leaving tertiary education should also be evaluated in light of the behavioral alternative that leaving would enable. For example, a student who leaves higher education to focus his or her undivided attention and dedication toward an already momentum gaining endeavor that does not require formal education, such as acting, sports or being a social media influencer may well regard $B_c < B_g$. However, on average, completing higher education should be more beneficial than dropping out of it, which is empirically supported (e.g. Oreopoulos & Petronijevic, 2013). Second, the immediate monetary costs C of staying in college are in general greater than the costs for leaving college ($C_c > C_g$). This might be a lesser issue in Germany where the costs of attending university are only moderate, but in other countries, such as the United States, college tuition costs are high and frequently the only way to pay them is by taking on debt. The costs of staying in higher education are usually deemed worth it if the anticipated benefit makes up for it in the long run. Consequently, one might think that as long as $B_c - C_c > B_g - C_g$, students would stay in higher education. But there is still one parameter missing that severely influences the benefit, namely the probability of success P. While C is fixed and not affected by P, the benefit B is. If students do not think they will be able to successfully complete their study program (low P_c), the weight of the benefit (B_c) within the utility function (U_c) is severely diminished. Thus, students' perceived probability of success P_c is a central component of their dropout decision. This finding is corroborated by research that finds that a major predictor for dropout are students' grades, challenges with and success in their chosen study program (e.g. Heublein, 2014; Heublein et al., 2017; Respondek et al., 2020).

One of the most interesting insight gained from framing the dropout decision within the RC framework comes from comparing the study program decision with the dropout decision. When comparing these decisions, the following insight reveals itself:

The decision for a particular study program $\max_{\mathrm{i} \in \mathrm{F}} \mathrm{U_i}$ *is a direct precursor of* $\mathrm{U_c}$.

Why is that? It is because the utility of the study program is the same as the utility of completing the study program, just calculated at different time points. But more importantly, the proposed connection also implies that the higher the *absolute* utility of $\max_{i \in F} U_i$ of the chosen study program the less likely $U_g > U_c$. In other words, the larger the perceived utility of the chosen study program the less likely it becomes that students will drop out of it. As alluded to in the previous chapter, an approximately optimal study program decision can only be made if students are able to assess their skills, wants and believes. When students can accurately assess themselves and the study programs they are considering, they are more likely to choose a study program that fits them - an undertaking that should be supported by providing students with relevant, novel and high-quality information. Accordingly, helping students with their study program decision should also reduce study program dropout. However, although $\max_{i \in F} U_i$ should be similar to U_c they are not the same. A lot can change between choosing to enroll into a study program and choosing to drop out of it. The factors that influence the parameters and the resulting utility of U_c, thus making them less and less similar to the parameters and the utility of $\max_{i \in F} U_i$, are the focus of the third paper of my dissertation (Chapter 4). That is, the paper evaluates the factors influencing students' dropout decision that exert their influence after enrollment into higher education, namely *within* higher education.

1.2 Extended Summaries of Articles

Before starting with the extended summaries for each paper, I will introduce the dataset all articles utilize and provide a quick reference table for methods, dependent and independent variables, and moderators used in the articles (see Table 1.1).

The best way to empirically assess the proposed impact of information on study program considerations and choices is by means of randomized experiments. By utilizing a random assignment mechanism that allocates students into a treatment group, which receives novel information, and a control group, which does not receive novel information, a causal link between information and study program considerations and choices can be made. A study which does exactly that is the “PraeventAbb” (preventing dropout from tertiary education) project. The primary aim of the study was to evaluate the impact of a counseling workshop on dropout behavior in higher education. To this end, a randomized controlled trial was developed, where the treatment consisted of a full day university guidance workshop. The guidance workshop was developed with and administered by the student counseling departments of two cooperating universities in North Rhine Westphalia, Germany. Central components of the workshop were (1) the exploration of interests, values and motives, (2) the exchange of information on types of study programs and fields as well as information on how to acquire more information, (3) assessment of skills and interests as part of a self-assessment test that students needed to do before the workshop, (4) goal setting in form of writing down ideas and plans for their future and (5) receiving a firsthand account of studying at university from a currently enrolled student. The target group for the experiment were voluntarily participating high school students in their penultimate year of high school, who expressed interest in going to university and participating in a university guidance workshop. Participants were recruited through directly contacting schools in the surrounding area of the two large universities and asking them to advertise our study and counseling workshop to the applicable students. Furthermore, the study and workshop were advertised through the usual channels of the university counseling departments, such as their social media presence and their direct connections to schools.

Students interested in participating who belonged to the target group needed to fill out a welcoming survey (w1) which was open for registration over the span of the year 2018. Within this survey all pre-treatment variables, demographic information and contact information was gathered. At the end of the welcoming survey students were randomly allocated to treatment or control group. The treatment group was given the opportunity to participate in one of the workshops, while the control group was compensated with participation in a lottery. A total of 725 high school students participated in w1 and constitute the full sample of the study. Because the registration period spanned a year and we wanted to keep the duration between pre and post treatment survey relatively constant, we divided invitations to the second survey (w2), where all relevant short-term post-treatment variables were collected, into three tranches. The average time passed between w1 and w2 over all three tranches was 150 days. Between w1 and w2 students of the treatment group participated in one of the workshops. In total, 28 workshops with, on average, 9 participants were conducted as part of the project. Not accounting for item-missing, w2 was completed by 607 participants (response rate of 83.72%). At the end of the year 2019, after students should have graduated from high school and had a chance to enroll into higher education, the third survey (w3) was distributed and completed by 567 students (78.21% of w1 respondents). The fourth and final survey of the study (w4) followed a year later, at the end of 2020, when students who started higher education right after high school should have been in their third semester and was completed by 573 participants (79.03% of w1 respondents). By following a pre-post design, where all students participated in a survey before and after the treatment, we were able to leverage both between and within person differences. Furthermore, the study goes beyond previous research on the efficacy of student counseling by not only looking at short- but also long-term effects. All papers of my dissertation utilize data from different waves of this research project. As a general overview and quick reference Table 1.1 depicts the methods, data waves used, independent variable(s) of interest, main outcome(s) and moderators of each paper.

Table 1.1. Quick reference table for all Chapters

	Chapter 2	**Chapter 3**	**Chapter 4**
Method	Experiment	Experiment utilizing data from egocentric networks	Vignette Experiment
Waves used	W1 & W2	W1, W2 & W3	W3
Independent variable(s) of interest	Information treatment (counseling workshop)	Information treatment (counseling workshop)	Social and academic integration
Main outcome(s)	1. Gender-atypical study program consideration 2. Non-beaten path study program consideration	1. Intended field of study homogeneity 2. Chosen field of study homogeneity	Vignette -dropout intention
Moderator	Prior level of information	Network homogeneity	Academic family background

What follows now are extended summaries of each article. Within these summaries I focus on integrating the contents of each paper into the previously described larger theoretical framework.

1.2.1 Extended Summary of "Do Students Need More Information to Leave the Beaten Paths? The Impact of a Counseling Intervention on High School Students' Choice of Major."

The first paper of my dissertation (Chapter 2) evaluates the effect of an information treatment, in the form of a university guidance workshop, on students' intended choice of major. It is motivated by the fact that many students focus on a small range of well known (beaten paths) and gender typical study programs (Jonsson, 1999; Morgan, Gelbgiser, & Weeden, 2013; OECD, 2020). This choice pattern may become problematic because it promotes gender segregation and inequality (Leuze & Strauß, 2014) and generates a lack of qualified individuals from niche study programs for sectors dependent on them. The RC account of educational decision making provides compelling reasons as to why students are predominantly focusing on both well-known and gender typical study programs. As previously described, students do not contemplate the whole universe of study programs (Ω) but only the subset of feasible study options (F), which itself is a subset of the study options students are aware of (I) and those that are accessible (A) (see *EQ2*).

Most people are aware of well-known and gender typical study programs because the mental accessibility of these study programs is very high, which can be attributed to the high frequency by which students are exposed to them. Simply put, students have a high chance of encountering someone who studies/studied a common university major because they are the most studied university majors and as such are most frequently represented in the population. Well known or popular study programs, such as business, psychology, medicine and law also have high mental accessibility because they are frequently embedded in popular media, such as tv shows and movies, and the professions they enable, like medical doctors, lawyers, judges and psychologists, permeate people's lives.

Relatedly, students are frequently more aware of gender typical study programs because they associate and compare themselves to similar others (Festinger, 1954; McPherson et al., 2001), such as same-gendered individuals (Ridgeway, 2011). Given that there is an unequal distribution of males and females in study programs (Hägglund & Lörz, 2020), by virtue of preferred comparison with same-gendered individuals students have a higher chance of encountering and gathering information on gender typical study programs compared to gender-atypical ones. For example, a man may know details of a gender typical study program, such as computer science, and the occupations this study program enables because it is male dominated and the chances of encountering and meaningfully associating with someone who wants to study or studied the same is higher than for a study program that is female dominated, such as educational sciences.

Students may either not be aware of gender-atypical and less well-known (non-beaten paths) study programs and not compute their utility $i \notin I$, or these study programs are not readily accessible $i \notin A$. Additionally, students may have only little or inaccurate information on the parameters P, B and C of these study programs' utility functions, making their calculation less precise and more uncertain. Consequently, non-beaten path and gender-atypical study programs are either bad contenders for $\max_{i \in F} U_i$, or they are not considered feasible in the first place $i \notin F$. Well known and gender typical study programs on the other hand have a higher chance to be considered and their parameters are more likely to be accurate (or thought to be accurate).

Providing students with relevant and high-quality information should therefore in theory increase information on and reduce misconceptions about gender-atypical and non-beaten study programs, thus increasing the likelihood that students consider these programs. Information deficits may also be reduced by students themselves when they start doing independent research on study programs as a natural part of engaging with and thinking about higher education. As a result, the extent to which information deficits occur should partly depend on the amount of information people have gathered on study programs or when they started researching study programs. Consequently, the effect of information on consideration of gender-atypical and non-beaten path programs may depend on the information level prior to the information treatment.

We test these hypotheses by means of the previously described randomized controlled trial, where a voluntary sample of students in their penultimate year of high school, who expressed an explicit interest in attending higher education, was randomly allocated to treatment and control group. Students voluntarily signed up for the study and the workshop. By signing up, these high school students already showed high engagement and openness to information, which is why this sample likely meets the previously stated prerequisites needed for information effects to occur. Our two main outcomes are whether students consider non-beaten study programs (less well-known study programs) and gender-atypical study programs. Additionally, we look at students' confidence in gathering information on study programs (career decision-making self-efficacy, CDMSE) (Betz et al., 1996), which we treat as a proxy for their information level. To account for two-sided non-compliance, we run instrumental variable regressions with treatment assignment as an instrumental variable for treatment participation. Our analyses reveal that the treatment increases students' confidence in gathering information by 0.14 points on a Likert scale of 5 ($z = 2.05$), consideration of non-beaten path study programs by 5.12 percentage points ($z = 1.17$) and gender-atypical study programs by 6.36 percentage points ($z = 1.87$). Subsequent analyses also reveal that the effect of the treatment on non-beaten path considerations is moderated by the information students possess prior to participating in the workshop. The treatment has a stronger effect on considering a non-beaten study program, if students have low starting levels of information. However, no significant interaction is observed for the consideration of gender-atypical study programs.

The results convey a multifaceted picture. First, we see a significant main effect for the consideration of gender-atypical study programs (around $p = 0.05$ depending on the model) and the workshop successfully increased students' self-assessment of their own information gathering skills (a subscale of CDMSE) ($p < 0.05$). However, the main effect of the treatment on considering a non-beaten path study program is not significant on average ($p > 0.1$) but strong and statistically significant ($p < 0.05$) for participants with lower starting levels of information. Meanwhile, consideration of gender-atypical study programs does not vary by prior level of information.

1.2.2 Extended Summary of "University Field of Study Homogeneity within Close Friend Networks - Does Information Matter? Evidence from an Experiment."

The second paper of my dissertation (Chapter 3) examines how information impacts field of study homogeneity in close friend networks. This paper is motivated by the empirically well-established tendency of students to associate with and befriend people according to principles of homophily (McPherson et al., 2001) and that these networks become more homogenous over time when looking at educational outcomes, such as choice of college major (Giorgi et al., 2010; Lyle, 2007; Poldin et al., 2015), college enrollment (Alvarado & López Turley, 2012; Bifulco et al., 2014; Fletcher, 2015), university dropout (Cherng et al., 2013; Sommerfeld, 2016) and academic achievement (Gašević et al., 2013; Kretschmer et al., 2018; Lomi et al., 2011). The observed trend towards homogeneity is a factor that can exacerbate educational inequality through cumulative (dis-)advantage building (DiMaggio & Garip, 2012; Raabe et al., 2019). Therefore, understanding why this trend towards homogeneity occurs and how it can be effectively counteracted may help reduce the inequality it promotes.

Close friend networks are tightly knit networks where each node is, through triadic closure, likely connected with one another (Goodreau et al., 2009; Granovetter, 1973). Ties within these networks exhibit features such as high emotional intensity, large time investment, intimacy and reciprocal services (Granovetter, 1973; Krackhardt, 1992). Two theoretical ideas may help explain the observed trend towards homogeneity in these types of dense networks: *information redundancy* and the subsequent potential for *echo chamber effects* (Burt, 2005).

Although network effects are not an explicit focus in most RC accounts of educational decision making (notable exceptions are Boudon, 1974; Sewell et al., 1969), their effects can easily be incorporated into the model. Within RC, network effects can be modeled as the influence others exert on the parameters of the utility function P, B and C, or on the set of study programs under consideration $i \in F$.

Close friends are regarded as a valuable and trusted source of information when students make educational decisions (Crosnoe et al., 2003; Klepper et al., 2010), but the amount of information diversity close friends provide is low and information exchange within these type of networks can have an echo chamber effect (Burt, 2005). Information diversity tends to be low because students choose friends based on similarity (homophily), and therefore the information they possess and disclose also tends to be similar (Burt, 2005). Echo chamber effects occur in these networks because students exchange information on study programs based on limited information diversity. As a result, similar information on study programs gets repeated frequently, solidifying in the minds of network members and becoming salient information with a high chance to influence educational decision making (Fletcher, 2012; Rosenqvist, 2017). In sum, the lack of information diversity and the resulting echo chamber effect could explain the empirically observed educational outcome homogeneity in these networks. Inside of the RC framework this argument can be integrated as follows: due to low information diversity within the close friend network, students frequently consider the same study programs as their close friends. Consequently, the study programs of close friends likely belong to set A. Repeatedly conversing about the same study programs also provides students with seemingly more precise and confident estimates on P, B and C for the U_i of these study programs. The result is that the few study programs drifting through the close friend network make good candidates for $\max_{i \in F} U_i$. As each network member is subject to these effects, study program homogeneity is high. These arguments, connecting information with network effects to explain the tendency towards educational homogeneity in close friend networks, have multiple similarities with the arguments presented in the first paper of my dissertation on the relation between information and students' focus on well-known and gender typical study programs.

If these information processes lead to educational homogeneity in close friend networks, one way to reduce homogeneity could be to increase information diversity within the network by providing a network member with new information through an external source (H1). Network homogeneity within the close friend network may be reduced by information through several mechanisms. For example, an individual who receives new information may change his or her own study program considerations and choice, thus contrasting the dominant study program considerations within the close friend network. Alternatively, the individual who receives novel information may become a conduit of information for his or her friends, leading them to consider and choose from a wider range of study programs, leading to study program diversification within the close friend network. Independent of how information dissipates in the network, the new information has the potential to diversify study program considerations and choices inside the network and reduce educational homogeneity. However, it is not enough to look at students' *considered* field of study because what matters in the end is the *chosen* field of study. For this reason, this study also assesses the impact of information on chosen field of study homogeneity in close friend networks. When examining field of study choices, which take place later than field of study considerations, natural information saturation effects should be considered. Students gather information at different time points, but most students start independent information gathering before enrolling into higher education (Obermeit, 2012). Accordingly, the effect of information on field of study homogeneity is likely higher for the intended field of study than the later chosen field of study, where information saturation has likely occurred (H2). Finally, it is also possible that the extent to which information can flourish inside a close friend network depends on the homogeneity of the network. If the network is already diversified and information not redundant, information as a means to reduce homogeneity may not be very effective, since there is little potential for further information diversification, thus ceiling effects may occur. Consequently, the impact of information on field of study homogeneity may depend on network homogeneity (H3).

I test these hypotheses with the same sample and treatment as utilized in the first paper of my dissertation. In addition to using w1 (pre-treatment, in high school) and w2 (post-treatment, in high school), this study also takes advantage of w3 (post-treatment, after high school), to assess the effect of the information treatment on students' study choice. Within each wave, the egocentric close friend network of participating students was gathered via a name generator. After naming up to four of their closest friends, students were asked to indicate what these friends want to study (w1 and w2) and what they are studying (w3). The central construct of close friend network homogeneity is defined as the overlap between egos study plans and choices and that of his or her close friends, expressed as a percentage. Analyses reveal that the information treatment reduces the intended field of study homogeneity by 6.8 percentage points ($p < 0.05$) but does not have a statistically significant impact on field of study choice homogeneity. The treatment effect does not exhibit statistically significant variation depending on the amount of pre-treatment homogeneity, defined as intended field of study homogeneity at w1 and the percentage of close friends ego knows from school.

The results convey an interesting image. First, the information treatment effect successfully reduces intended field of study homogeneity, in the short run, indicating that homogeneity is at least partly driven by information processes. Second, the information treatment effect could not be observed for field of study choice homogeneity, meaning that the effect was short-lived and, in the end, did not contribute to a reduction in educational homogeneity in the long run. Third, the degree of pre-treatment homogeneity in the network did not have an effect on the strength of the information treatment, which could mean that more homogeneous networks are not necessarily less information diversified.

1.2.3 Extended Summary of "The Relevance of Social and Academic Integration for Students' Dropout Decisions. Evidence from a Factorial Survey in Germany."

The last paper of my dissertation (Chapter 4) investigates the next major educational decision after choosing a study program: whether to drop out or change the study program. In this chapter, we depart from the impact of information on study program choices and instead focus on the factors *within* higher education that influence whether students remain in or drop out of a study program.

The dropout decision within RC is straight forward to model (for similar applications see Breen & Goldthorpe, 1997). Students are said to weigh the utility of leaving tertiary education for good or changing subjects (U_g) against the utility of completing the study program (U_c). Given the utility function (see *EQ1*), the two questions that come up are: which factors affect the parameters of this educational decision and how do they do it? Tinto (1975) developed an influential model that sheds light on these questions. He proposed that students' dropout decisions are the result of a longitudinal process of individuals interacting with two main components of the higher education system, namely the academic and social systems. Integration into the social system is composed of two subdimensions: (1) social integration with faculty, which is the relationship between students and faculty (e.g., the support students receive from faculty) and (2) social integration with fellow students, i.e., the extent to which students were able to form meaningful relationships with other students. Integration into the academic system, in turn, is also composed of two subdimensions: (1) academic grade performance and (2) intellectual development. The first describes students' challenge and performance in higher education, while the second is the extent to which they intellectually align with the academic content of a study program, which we interpret as academic interest.

The idea that the dropout decision is a longitudinal process is akin to the RC account of prior influencing later educational decision. Tinto's student dropout model predominantly focusses on the interactions *within* higher education and not those that came *before*. This constitutes an important extension of the first two papers of my dissertation that focus on the mechanisms at the beginning of a higher education trajectory. The two approaches complement each other well and provide the following insight: a suboptimal educational decision does not necessarily lead to dropout, if social and academic integration are high, just as an optimal educational decision does not necessarily shelter one from dropout, if social and academic integration is low.

In terms of the RC model of educational decision making, these factors can influence different aspects of the utility function. High academic challenge or low social integration with the faculty, for example, most likely reduces the probability of success (P) of completing the study program (U_c), while a lack of academic interest or low social integration with fellow students may increase the perceived benefit (B) of changing the study program or leaving higher education (U_g) in the hope of better interest fit and building new friendships elsewhere.

The primary aim of this study is to find causal evidence on the extent to which all aspects of academic (composed of academic interest and challenge) and social (composed of social integration with faculty and students) integration influence the dropout decision. Furthermore, we consider one of the most salient topics in higher education research that permeates through all educational decision-making theories: social and educational inequality. From Boudon's (1974) primary and secondary effects, to the Wisconsin model of status attainment (Sewell et al., 1969), and the RC account of Erikson and Jonsson (1996) as well as Breen and Goldthorpe (1997), the explanandum looked at is educational inequality and why it persists in seemingly meritocratic societies. A central component of these theories is that students' educational decisions are influenced by their social origin, which is frequently conceptualized as parents' status, parents' educational background or their social class. In combination with Tinto's model, we focus on the question whether the extent to which students rely on their academic and social integration for their dropout decision depends on their academic family background. The compensatory advantage argument (Bernardi, 2014) suggests that students from academic family backgrounds rely less on their degree of academic and social integration in their dropout decision because they are able to compensate for low degrees of integration with family resources and their already more academically oriented habitus. Students from non-academic family backgrounds do not have these resources to fall back on and are thus more dependent on social and academic integration.

To test these hypotheses, we conducted a factorial survey within the third wave of the "PraventAbb" study, where we gave participants vignettes (fictitious situations) with varying degrees of social and academic integration. Each vignette asked students to indicate how likely they would drop out of their study program (from 1 "not likely at all" to 10 "very likely"), if at the beginning of their third semester, they had achieved specific levels of academic and social integration. Each of the four integration dimensions had three levels (high, medium and low integration), resulting in a 3x3x3x3 factorial survey design, with 81 possible combinations. From all possible combinations, a D-Efficient sample of 72 vignettes (D-Efficiency = 97.4192) was drawn and allocated to 18 decks with four vignettes each. Each participant received one randomly allocated deck as part of the survey. We calculate random effect models to account for the hierarchical data structure and included cross-level interaction terms between academic family backgrounds and each integration dimension to assess the "compensatory advantage" argument.

Our analyses reveal that all integration dimensions have a statistically significant effect on students' dropout intention, although their magnitude varies. The integration dimension with by far the largest impact on dropout intention is academic interest, which increases dropout intention by 4.025 ($p < 0.001$) scale points when changing from high to low integration. Social integration with fellow students also has a substantial, although smaller effect on dropout intention (1.652, $p < 0.001$, low vs. high), while academic challenge (0.896, $p < 0.001$, low vs. high) and social integration with faculty (0.371, $p < 0.01$, low vs. high) clearly trail behind effect size wise. Cross level interactions show that the slopes of integration dimensions do, for the most part, not statistically significantly vary by students' academic family background. Only the interaction academic interest with academic family background is significant (joint significance of $p < 0.05$), unveiling that lower levels of integration have a lesser effect on dropout intention for students from non-academic backgrounds than for students from academic backgrounds. This result contrasts the "compensatory advantage" argument, since we either find no effect heterogeneity pertaining to academic family background, or the effect was opposite to the one expected.

Overall, this paper reveals that social and academic integration have a causal influence on dropout intention and that the impact of social and academic integration for the most part does not depend on academic family background. Tying it into the RC framework of educational decision making, one should seriously consider accounting for academic and social integration, especially academic interest, when modeling dropout with utility functions as they are a central reason as to why $\max_{i \in F} U_i \ != U_c$.

Chapter 2: “Do Students Need More Information to Leave the Beaten Paths? The Impact of a Counseling Intervention on High School Students’ Choice of Major.”

2.1 Abstract

Despite an almost endless list of possible study programs and occupational opportunities, high school students frequently focus on pursuing a small number of well-known study programs. Students also often follow gender-typical paths and restrict their attention to study programs in which the majority of students consists of same-gendered people. This choice pattern has far-reaching consequences, including persistent gender segregation and an undersupply of graduates in emerging sectors of the industry. Building on rational choice and social psychological theory, we argue that this pattern partly occurs due to information deficits that may be altered by counseling interventions. To assess this claim empirically, we evaluated the impact of a counseling intervention on the intended choice of major among high school students in Germany by means of a randomized controlled trial (RCT). We estimate the effect by instrumental variable estimation to account for two-sided noncompliance. Our results show that the intervention has increased the likelihood that participants will consider less well-known or gender-atypical study programs, particularly for high school students with lower starting levels of information. Supplementary analyses confirm that a positive impact on information seems to be one of the relevant causal mechanisms. These results suggest that counseling services have the potential to guide high school students to less gender-typical and well-known majors, possibly reducing gender segregation and smoothing labor market transitions after graduation.

Keywords: Choice of Study Program, Gender Segregation, Counseling Intervention, Transition to College

Author Contributions:

Lukas Fervers is my co-author for this paper. He and I contributed equally to the development of the theoretical arguments, data gathering, cleaning and coding, conduction of the analyses and writing of the paper.

2.2 Introduction

High school students can pick from an almost endless list of possible college majors. Despite this abundance, a large share of high school students focus on a small number of occupational opportunities and the college majors that lead up to them. In a recent survey in countries participating in PISA, approximately 50% of high school students indicated that they expect to work in one of the 10 most well-known jobs. Among women, approximately 30% focus on only four jobs—medical doctor, psychologist, lawyer and manager—as their first choice (OECD, 2020). The focus on these well-known jobs and the programs of study that allow them to be accessed (*beaten paths*) has severe consequences for the transition to the labor market and the economy as a whole. While there is an oversupply of students and graduates in well-known study programs, new and emerging professions face increasing difficulties in attracting qualified applicants (OECD, 2020). Consequently, leaving the beaten paths may smooth the transition to the labor market after graduation. Similarly, many students restrict their attention to gender-typical study programs where their own gender is in the majority (Chang & ChangTzeng, 2020; Morgan, Gelbgiser, & Weeden, 2013). In Germany, the share of females in some engineering majors is below 10%, while reaching nearly 90% in pedagogy (Destatis, 2019). This gender segregation in higher education is a major driver of gender inequality in later life, as male-dominated subjects offer more favorable employment prospects (García-Aracil, 2008; Gerber & Cheung, 2008; Naess, 2020).

In sum, both choice patterns have undesirable consequences for society that reach well beyond the educational system itself. This raises the question of why these patterns emerge and whether they can be altered. In this paper, we build on rational choice (Breen et al., 2014; Breen & Goldthorpe, 1997) and social psychological theory (Eccles, 2005; Festinger, 1954; Mannes et al., 2012; Ridgeway, 2011) and argue that both patterns can be attributed in part to a lack of information about available college majors and in part to insufficient knowledge about the individual's own preferences and how well the various alternatives fit the individual.

We argue that in the absence of complete information, the disadvantages of leaving common behavioral paths (e.g., in terms of insecure employment prospects or study requirements) are apparent, while the benefits gained from, for example, choosing a career path that involves less common and/or gender-atypical majors, are uncertain. Consequently, students may avoid taking this risk, opting to stick to the more conventional behavioral patterns, even when other choices would better fit their personal preferences. In addition to the societal consequences outlined above, this may result in inferior educational outcomes, since students who choose a career path or major that does not fit their goals, interests or personality type have lower choice satisfaction, consistency and persistence within the chosen path (Holland, 1996; Rocconi et al., 2020; Suhre et al., 2007). The high dropout rates from beaten path programs suggest that simply following common behavioral choice patterns may be a poor decision for high school students (Neugebauer et al., 2019).

If these choice patterns emerge partly due to a lack of information, they may be weaker when information is more accurate and complete. We thus hypothesize that providing counseling services to high school students may lead them to consider less well-known or gender-atypical programs more frequently. To test this claim empirically, we assess the impact of a counseling intervention for high school students on the intended choice of major. The counseling intervention provides students with basic information on study programs as well as an assessment of their skills and interests complemented by individualized feedback. Broadly speaking, this intervention aims to direct students to college majors that best fit their personal skill profile and occupational preferences. Participants in the counseling workshop were in their penultimate or final year before graduating from high school. To achieve the highest degree of internal validity, we employ a field experiment that randomly assigns high school students to the treatment or control group (RCT) and estimate the treatment effect by instrumental variable regression to account for the possibility of endogenous noncompliance. In addition, we argue that the effect is not homogenous within the sample but varies systematically with the level of information students had prior to the workshop. Our results confirm that the workshop indeed increased the likelihood that high school students would consider gender-atypical and less well-known majors, particularly if the level of information they started with was low.

Our analyses contribute to previous research in numerous ways. At the most general level, they deepen our understanding of the determinants of choice of major in general and gender segregation in particular (Barone & Assirelli, 2020; Chang & ChangTzeng, 2020; Haas & Hadjar, 2020). Moreover, they build on and extend the evaluation literature that assesses the effectiveness of policy interventions for high school students. Previous research on similar counseling interventions (e.g. Le et al., 2016; Moore & Cruce, 2020; Turner & Lapan, 2005) has rarely employed experimental designs. Studies that have used random assignment have mostly focused on short info treatments of selected aspects of college enrollment, such as costs and benefits (Barone et al., 2017; Ehlert et al., 2017; Finger et al., 2020). To date, experimental evaluations of more comprehensive counseling services remain rare and therefore constitute an important gap in the literature. Finally, we assess a dimension of effect heterogeneity (starting level of information) that has largely been neglected by previous research and may enable future interventions to employ more effective targeting strategies.

The remainder of this paper is organized as follows. The next section develops a theoretical framework on the relationship between information, choice of major and the role of counseling and reviews previous research in this field. The research design and methodology, including the counseling workshop under discussion, are then described, followed by a presentation of our results. The final section concludes with a summary and discussion for future research and policy-making.

2.3 Choice of Major, Information and the Role of Counseling

2.3.1 Theoretical Framework

According to rational choice theory, students weigh the costs and benefits of different options against each other when making educational choices (Breen et al., 2014; Breen & Goldthorpe, 1997; Erikson & Jonsson, 1996). When selecting between different possible majors, relevant financial evaluation parameters include direct costs, time and effort, the likelihood of succeeding and employment prospects after graduation. Additionally, students seek to optimize the fit between their personal skills and interests and the skill profile of the respective major (Holland, 1959).

However, most of these parameters are subject to severe information deficits, particularly concerns about the fit between personal interests and the content of a particular study program (major-interest fit), since most high school students have only vague knowledge about the content of the study programs they are considering (Heublein et al., 2017). Consequently, students may end up making a decision they would not have made in the presence of complete information, as several parameters of the utility function are misestimated.

Generally, information deficits can lead to suboptimal choices in multiple ways since information updates affect each individual's utility function differently. However, we argue that two patterns concerning the choice of major are more common when information deficits are high. First, a lack of information may partly explain why high school students often focus on a small number of well-known study programs, such as law, business, psychology or medicine (*beaten paths*), as their first choice. This argument assumes that programs with a better interest-major fit may exist for several students, but the benefit of leaving the beaten paths is uncertain when only incomplete information about one's own preferences as well as the exact content of alternative study programs are available. In contrast, the disadvantage of less secure employment prospects after graduation or the likelihood of failing in niche study programs may seem apparent. Consequently, some high school students may opt not to leave the (seemingly) safe harbor of well-known mass study programs, even though a different major would be a better fit for them personally. This argument is substantiated by social psychological theory, which holds that people tend to follow more common behavioral choice patterns (Cialdini & Goldstein, 2004; Mannes et al., 2012) if uncertainty about available options is high.

Second, we argue that some information deficits tend to increase gender-typical choices. Information deficits are (partly) created through the general tendency of people to associate and gain information from (McPherson et al., 2001) and compare themselves to similar others (Festinger, 1954), such as same-gendered people. Considering that the choices and behavior of same-gender individuals can also function as a first or primary point of reference (Ridgeway, 2011), students likely possess detailed information on gender-typical choices but insufficient information on gender-atypical choices. Similarly, within expectancy-value theory (Wigfield & Eccles, 2000), Eccles (2005) argues that people do not choose from all available options but only from the most salient ones. Some options are not considered at all because of insufficient or inaccurate information. The crucial point is that these information deficits are gender-stereotyped (Eccles, 2005), i.e., students often possess information on gender-typical study programs and their contents but may have insufficient or inaccurate information on gender-atypical study programs. As a result, students may prematurely exclude gender-atypical majors even though they might better fit their interests and skill-set (Eccles, 2005).

In sum, we argue that information deficits and inaccuracies increase the likelihood that students focus on well-known and gender-typical majors. At this point, the role of counseling comes into play. If a counseling intervention successfully decreases information deficits and dissolves information inaccuracies, the obstacles to leaving the beaten paths and/or considering gender-atypical choices are (partly) removed. Given that high school students may have difficulties finding and parsing reliable sources of information relating to the content of majors and how they fit them personally (Heublein et al., 2017), it seems reasonable to argue that targeted counseling and profiling interventions have the potential to tackle these problems. Correspondingly, we hypothesize that counseling interventions will increase the likelihood that less well-known study programs and gender-atypical majors will be considered.

In addition, we argue that the effect is not homogenous within the sample. The lower the starting level of information prior to the workshop, the higher the potential for improvement. In contrast, our argument becomes obsolete for students with very high levels of information due to ceiling effects. Therefore, we expect the effect on the intended choice of major to be stronger for students with lower levels of information prior to the intervention.

2.3.2 Related Work

Previous literature on counseling interventions is characterized by strong heterogeneity in terms of the intervention type and the outcomes considered. The first group of studies administers counseling and profiling interventions aimed at fostering the major-interest fit, college enrollment as such or academic performance. Turner and Lapan (2005) evaluated a computer-assisted intervention designed to increase nontraditional career interests, reporting positive effects for boys and girls. In contrast, Moore and Cruce (2020) did not find a positive impact of fit signals given to students on their planned major certainty. Domina (2009), Le et al. (2016), Cunha & Weisburst (2018) and van Herpen, Meeuwisse, Hofman & Severiens (2020) evaluate large-scale counseling and college preparation programs from the United States and the Netherlands and find consistently positive effects on academic performance and the likelihood of enrolling in college, respectively. A second group of profiling interventions mostly focuses on the short-term effects of psychological constructs such as career decision-making self-efficacy (CDMSE) or career indecision. While Behrens and Nauta (2014) detected no effect on CDMSE when letting college students complete a self-assessment questionnaire (self-directed search, SDS; Holland, 1996), the majority of studies (Dik & Steger, 2008; Isik, 2013; for a meta-analysis see Whiston et al., 2017) report positive effects of interest inventories on CDMSE or related constructs. However, randomized designs are a rare exception among both groups of studies.

The third group of studies more frequently relies on RCTs while focusing on short info treatments, such as brief oral presentations or provision of information booklets or flyers (Ehlert et al., 2017; Loyalka et al., 2013), about the financial aspects of college majors such as costs or financial aid. These studies rely on the assumption that providing information about costs and benefits could affect college enrollment as such or the field of study, especially for students from nonacademic households (Ehlert et al., 2017; French & Oreopoulos, 2017; Hastings et al., 2015; Loyalka et al., 2013; Oreopoulos & Dunn, 2013). The effects tend to be positive but limited in size. For example, Barone et al. (2017) report positive effects on the likelihood of moving to longer or more ambitious fields of study, though the increase is, at 2.1 percentage points, slight.

In summary, previous research has revealed important insights for guiding high school students to suitable majors. Both comprehensive counseling and profiling interventions as well as short info treatments have the potential to improve the transition from high school to college. At the same time, several gaps in the literature remain. Most importantly, few studies have investigated the impact of counseling and profiling interventions by means of RCTs. Those that have employed RCTs have mostly focused on info treatments of short duration (20-60 minutes). Correspondingly, little evidence exists on the impact of comprehensive counseling and profiling interventions that meet the highest methodological standards (especially random treatment assignment). Moreover, the analysis of effect heterogeneity has mostly focused on effect differences with respect to socioeconomic background. Further moderators, such as the starting level of information, have largely been neglected. In this regard, our analysis extends the previous literature at both the substantive and methodological levels.

2.4 Methodology

2.4.1 Research Design and Target Group

Students who were between 6 and 18 months before graduating high school and planned to go on to university were invited to participate in the study. Participants were actively recruited from high schools in the areas surrounding two large German cities, resulting in a sample of 725 voluntary participants. Workshops were carried out by the department for student services of the two universities. The recruiting strategy was similar to that usually employed for workshops offered by the department for student services (promotion of the workshop via high schools or during campus days, among others). Consequently, the resulting sample is not representative of all high school students but very similar to groups usually entering university counseling interventions in Germany.

To register for the study, participants needed to take part in the first survey, where pretreatment and time-invariant covariates were gathered. At the end of the first survey, participants were randomly assigned to the treatment group, the members of which were invited to the counseling workshop, or to the control group, whose members participated in a lottery. After the workshop took place, the participants were invited to take part in the second survey of the study, during which all outcome variables were measured again. The time between the first and second survey was between three and six months for most participants and was held constant between the treatment and control groups to avoid collinearity between treatment status and calendar time.

A total of 607 students participated in the second survey, resulting in a relatively high response rate of 83.72%. This rate was achieved by reminding participants multiple times via e-mail, SMS and telephone calls to fill out the second survey. The 16.28% who did not respond mostly consisted of students with invalid telephone numbers or those who never answered the phone. As telephone numbers were collected before the randomization, there is no reason to believe that this is correlated with treatment status, thus refuting concerns about endogenous panel attrition (for an in-depth discussion of possible biases due to panel attrition and item-nonresponse, see the robustness section). A total of 574 participants provided complete information on all relevant variables and were used for the analyses (see table A2.1 for summary statistics).

2.4.2 The Counseling Workshop

The intervention is a full-day university guidance workshop. Prior to attending the workshop, students were asked to do an online self-assessment test made available by the German Federal Employment Agency. The test assesses both cognitive and noncognitive skills as well as vocational interests. During the workshop, participants received individual feedback from professional counselors about study opportunities that could fit their skills and preferences. The workshop also provided information on different kinds of study programs (e.g., university vs. university of applied sciences) and fields of studies as well as sources and strategies for gathering further information. Afterward, an enrolled student shared his or her own experience with studying at university. At the end of the workshop, the participants were asked to write out everything they had learned about themselves and their plans for the future. The design of the workshop thus followed previous work on critical ingredients for successful counseling intervention (Brown et al., 2003). In total, 28 counseling workshops with an average of nine participants each were conducted as part of the experiment.

To put this into the context of our theoretical argument, we expect this workshop to affect the decision process of participants in several ways. First, we hypothesize that the workshop increases students' level of information concerning possible study opportunities as well as their ability to gather additional information about available majors. Second, online self-assessment with individual feedback is intended to help students increase awareness of their individual skills and occupational preferences, enabling them to identify college majors with a better major-interest fit. Finally, the workshop conveys the general notion that this major-interest fit is the most important parameter for their choice. As a result, students should become more inclined to focus on their personal preferences rather than following the behavior of others in general or the behavior of people of the same gender in particular when choosing a college major.

2.4.3 Data and Variables

Our primary outcome is students' intended choice of major. The intention to enroll in a gender-atypical major and a major that is off the beaten path (non-beaten path) are dichotomous outcomes, coded from an open question, in which students were asked to name up to five majors they were considering. The answers were coded according to the classification of study programs by the German Office of Statistics (Destatis, 2020). If at least one of the majors listed was gender-atypical, the answer was coded as 1, and 0 otherwise. The same procedure is used for coding the non-beaten path. That is, if students mentioned a study program outside of medicine, psychology, law or business, they were considering a non-beaten path and coded as 1. This definition seems the most plausible from a theoretical point of view. While it may also be possible to count the number of non-beaten path or gender-atypical study programs in the choice set, we argue that mentioning only one particular study program reflects a greater certainty that it is the most suitable option. Therefore, the dichotomous measurement seems more accurate than the numeric measurement.

Gender-atypical majors are defined based on the distribution of enrolled males and females in the respective study program. We define a gender-atypical major as one in which the share of students from the opposite gender is at least 60%. For example, a woman who intends to enroll in a major in which at least 60% of the enrolled students are male intends to study a gender-atypical major. To see whether the results are robust to different threshold specifications, we also varied the threshold to 65% and 70% (see robustness section). The definition of a non-beaten path is based on the distribution of intended study choices within our sample. Four study programs listed within the choice sets were by far the most common: medicine (mentioned by 20.55%), business and economics (29.93%), law (19.31%) and psychology (22.48%). All other fields of study were mentioned by at most 10% of the participants, implying that the four most common fields have an outstanding position. This pattern is broadly consistent with that found in representative surveys.

As outlined in the theory section, we argue that a change in one's intended choice may partly be due to an increase in the amount of information. Correspondingly, we conduct supplementary analyses concerning the impact on the level of information. This includes (1) self-appraisal (i.e., being aware of one's own skills and preferences), (2) the ability to gather information and (3) goal selection (i.e., being able to select between different majors). However, measuring these dimensions of the actual level of information is not straightforward. We therefore follow previous research that has often relied on psychometric scales such as CDMSE, which measure confidence in one's own knowledge and decision-making competence. Assuming that students can estimate their own level of information with some reliability, CDMSE can be treated as a proxy for the actual level of information.

We slightly adapted the CDMSE short-form scale (Betz et al., 1996) to more precisely measure what the workshop is intended to affect. It includes the three outlined subdimensions with three items each. In each question, respondents are asked to rate their ability to perform a specific task on a scale ranging from "no confidence at all" (1) to "complete confidence" (5). We calculated the mean over all subscales (CDMSE-Total) as well as the mean of each subscale separately. The scales show high consistency, with Cronbach's alpha ranging between 0.71 and 0.86.

To increase the efficiency of our treatment effect estimations, we included pretreatment outcomes and time-invariant covariates in our analyses (Imbens & Rubin, 2015). These include gender, parental education level (whether both parents have a university degree or not), household composition (living with siblings/with both parents), age, average grades in high school, study aspiration of schoolmates and parents' ambition (their desire for their child to have a university vs. vocational education). We also included a variable indicating when students started to gather information about study opportunities (before upper-secondary education, during the last school year, during this school year, not yet). The last two categories were combined due to the low number of observations in the highest category. Finally, we present information on both treatment assignment and treatment participation as reported by the counselors to the research team.

Table A2.1 provides summary statistics for all variables used in the analyses. As stated in the previous section, the sample is not representative of the population of German students in this age group. Workshops of this type are usually offered to a self-selected rather than representative group. Therefore, our study group is similar to students who usually enter such workshops, as opposed to a representative sample of high school students. Our sample consists of a larger share of female students, students from academic households and high-achieving students (in terms of grades), mirroring the pattern often observed in such interventions. Consequently, the results tend to generalize to similar, voluntary workshops but might be different for large-scale interventions that cover all or a representative set of students.

2.4.4 Estimation Technique and Robustness Checks

In RCTs, endogenous selection is ruled out due to random treatment assignment. One major problem that nonetheless needs to be addressed is two-sided noncompliance, i.e., some participants assigned to the treatment group did not appear in the workshops and vice versa. Consequently, the main analyses are conducted using instrumental variable regression (two-stage least squares (2SLS) IV-Regression) with treatment assignment as IV for the actual treatment status (Athey & Imbens, 2017; Imbens & Rubin, 2015). The amount of noncompliance within our study is small (12.41%), but ignoring it could bias our estimates significantly, as replication of our estimations by means of naïve OLS regression shows (see robustness section). To further substantiate the robustness of our results, we perform a wide range of alternative estimations (see robustness section).

2.5 Results and Discussion

2.5.1 Main Results and Effect Heterogeneity

Table 2.1 summarizes the results from the IV regressions, which estimate the treatment effect on our primary outcomes plus the supplementary analyses on CDMSE. Point estimates and confidence intervals are further displayed in Graph 2.1. The results point in the expected direction but are somewhat mixed in terms of statistical significance. Focusing on the intended choice of major, having a non-beaten path in the choice set is not significantly affected ($z = 1.17$), while considering a gender-atypical major is significant at the 10% level ($z = 1.87$). In this linear probability model, the treatment coefficient represents an increase of 6.36 percentage points on the probability of considering gender-atypical majors.

Beyond the treatment effect itself, it is interesting to see that the start of gathering information strongly predicts the consideration of a non-beaten path major. This substantiates the argument that the decision to take the beaten path may result from insufficient knowledge about suitable alternatives. Those who started looking for information before commencing their upper-secondary education were 15.9 percentage points more likely to consider a non-beaten path major than those who started during this school year/not yet. At the same time, we do not see a statistically significant effect of this variable on gender-atypical choice. While the treatment effect of the counseling intervention is medium strong, the starting point of independent information gathering seems to be of less importance for gender-atypical choice. Possibly, gender-atypical study programs were rarely considered during the independent information gathering, implying that exogeneous sources of information do matter for gender-atypical choice, whereas independent information gathering does not.

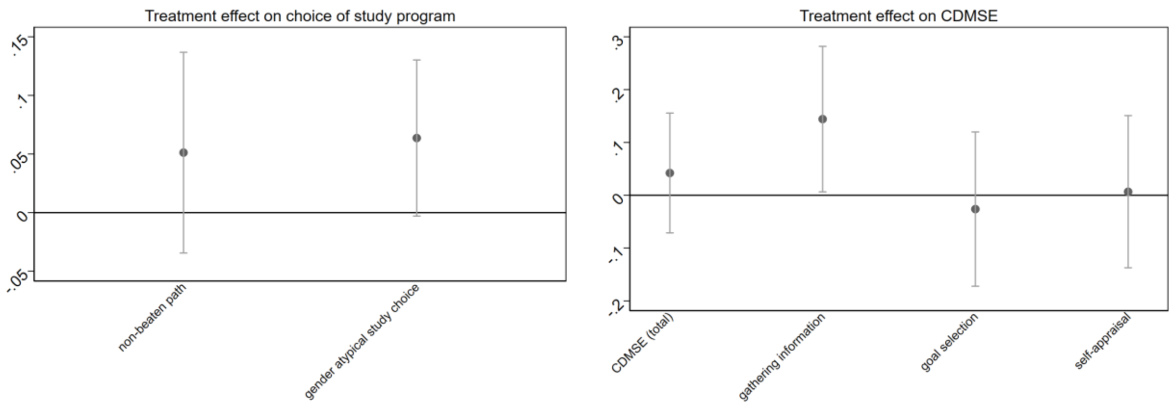
In addition to the impact on choice of major, we display the treatment effect on CDMSE in Graph 2.1. Among the three subscales of level of information, one out of three (gathering information) is significant at the 5% level ($z = 2.05$). The two other scales are clearly insignificant, and the point estimates are close to zero. Considering the measurement issues with these outcomes, it is worth noting that the strong correlation between CDMSE and the starting level of gathering information can be seen as an implicit validation of the CDMSE measure as a proxy for the level of information, as longer periods of information gathering should result in a higher level of information.

Table 2.1. Treatment effect on main outcomes and supplementary analyses on CDMSE

	(1) non-beaten path	(2) gender-atypical study choice	(3) CDMSE	(4) CDMSE (Gathering Information)	(5) CDMSE (Goal Selection)	(6) CDMSE (Self-Appraisal)
Treatment	**0.0512**	**0.0636^{+}**	**0.0421**	**0.144***	**-0.0262**	**0.00672**
	(1.17)	**(1.87)**	**(0.73)**	**(2.05)**	**(-0.35)**	**(0.09)**
Pre-treatment outcome	0.440**	0.480**	0.593**	0.508**	0.518**	0.517**
	(10.47)	(8.80)	(12.81)	(10.06)	(11.55)	(12.75)
Female	-0.0352	-0.239**	-0.0293	0.0910	-0.0894	-0.0541
	(-0.93)	(-4.98)	(-0.54)	(1.25)	(-1.28)	(-0.84)
Parents: university degree	-0.0635^{+}	-0.0234	0.0249	-0.0219	0.0466	0.0507
	(-1.76)	(-0.85)	(0.51)	(-0.36)	(0.74)	(0.83)
Start of information gathering: during this school year/not yet						
during the last school year	0.111**	-0.0302	0.0554	0.0858	0.0962	0.0579
	(2.91)	(-1.08)	(1.07)	(1.37)	(1.45)	(0.91)
before upper-secondary education	0.159**	0.0436	0.136*	0.139^{+}	0.195*	0.169*
	(3.25)	(1.04)	(2.12)	(1.75)	(2.38)	(2.04)
Household: siblings	0.0102	-0.0422	-0.0755	-0.0437	-0.168**	-0.0282
	(0.28)	(-1.43)	(-1.49)	(-0.70)	(-2.69)	(-0.44)
Household: both parents	-0.00693	-0.00276	0.00457	0.0469	-0.000493	-0.0195
	(-0.16)	(-0.08)	(0.08)	(0.67)	(-0.01)	(-0.28)
Parental study aspiration	-0.0781*	0.0383	-0.0110	-0.0799	0.00152	0.109^{+}
	(-2.17)	(1.29)	(-0.21)	(-1.29)	(0.02)	(1.66)
Study aspiration of schoolmates	0.0273	0.0145	0.0263	0.0565^{+}	-0.0222	0.0361
	(1.45)	(1.02)	(1.08)	(1.86)	(-0.70)	(1.18)
Age in months	0.00123	0.000478	0.00074	-0.00106	-0.000124	0.00311
	(1.20)	(0.48)	(0.44)	(-0.42)	(-0.06)	(1.49)
Grade school report 2017 (mean)	0.0139	-0.0171	0.0167	0.0358	0.0443	-0.0281
	(0.57)	(-0.87)	(0.47)	(0.80)	(0.94)	(-0.65)
Constant	0.0198	0.173	1.321**	1.873**	1.995**	0.999^{+}
	(0.08)	(0.69)	(2.84)	(2.84)	(3.52)	(1.86)
Observations	574	574	574	574	574	574

IV regressions with robust standard errors. z statistics in parentheses.
Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01

Graph 2.1. Summary of treatment effects



As outlined in the previous sections, the treatment effect may not be homogeneous among participants, but stronger for those who start with lower levels of information. In the next step, we therefore interact the treatment with the starting level of information. For the sake of robustness, we repeat the analysis twice with two different measures of the current state of information: the point in time at which students started to gather information and the self-assessed level of information (CDMSE) they started with in wave 1. We limit the analysis to the two main outcomes plus the subscale of CDMSE that has been significantly affected in the baseline specification.

The results for the two different measurements are summarized in Table 2.2 (start of gathering information) and Table 2.3 (starting level of CDMSE). The regression tables only display coefficients from the interaction terms and base variables, but the covariates included are the same as those in the baseline results. As the tables indicate, there is no visible interaction for a gender-atypical study program or the CDMSE subscale. In contrast, there is a fairly strong interaction between treatment status and the interaction variables for choosing non-beaten paths. As the regression tables only show the coefficient and significance for the lowest value of the interaction variable, Graph 2.2 displays the estimated effects at different levels of the interaction variable. The left panel shows that the effect gains significance for those who are at a rather early stage of their decision-making process. While the effect is insignificant (the point estimate is even negative) for the other two categories, the positive effect amounts to approximately 15 percentage points for those in the lowest category. Similarly, the effect is rather strong and significant at the 5% level for those at the bottom of the CDMSE distribution but becomes insignificant once the starting level rises above a certain point. This finding reinforces the argument that the focus on well-known study programs may partly result from insufficient knowledge about possible alternatives but may be altered by interventions that deliver this information.

Table 2.2. Interaction effects between treatment and start of gathering information

	(1) non-beaten path	(2) gender-atypical study choice	(3) CDMSE (Gathering Information)
Treatment	0.151*	0.0488	0.127
	(2.02)	(0.86)	(1.02)
Start of information gathering: during this school year/not yet			
during the last school year	0.160**	-0.0368	0.0529
	(3.03)	(-0.97)	(0.58)
before upper secondary education	0.252**	0.0274	0.194
	(3.84)	(0.48)	(1.57)
Treatment*start of gathering information: during the last school year	-0.134	0.0179	0.0846
	(-1.38)	(0.24)	(0.54)
Treatment*start of gathering information: before upper-secondary education	-0.255+	0.0443	-0.148
	(-1.82)	(0.36)	(-0.70)
Observations	574	574	574

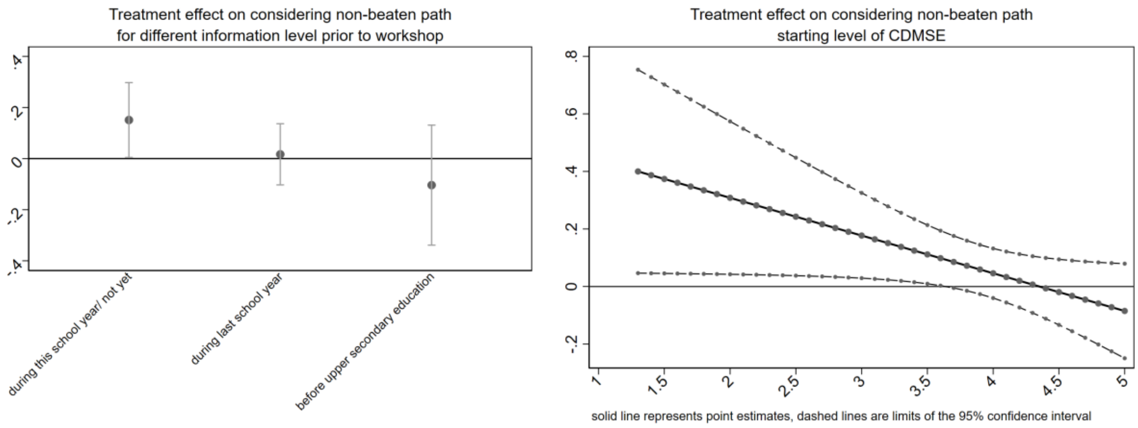
IV regressions with robust standard errors. Same control variables used as in Table 2.1 (not depicted).
z statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01

Table 2.3. Interaction effects between treatment and starting level of CDMSE

	(1) non-beaten path	(2) gender-atypical study choice	(3) CDMSE (Gathering Information)
Treatment	0.400*	0.0386	0.314
	(2.22)	(0.26)	(0.83)
CDMSE	0.0250	-0.0148	0.182*
	(0.61)	(-0.53)	(2.03)
Treatment*CDMSE starting level	-0.131*	0.00940	-0.0668
	(-1.97)	(0.18)	(-0.51)
Observations	574	574	574

IV regressions with robust standard errors. Same control variables used as in Table 2.1 (not depicted). z statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01. CDMSE was anchored at the smallest observed value (1.33) – the main treatment effect is thus the increase for students with a CDMSE-Score of 1.33

Graph 2.2. Treatment effect on considering a non-beaten path, depending on starting level of information



2.5.2 Discussion

These results reveal an interesting picture. Broadly speaking, the effects tend to confirm the theoretical expectations. Workshop participation indeed increases the likelihood that high school students will consider less well-known and/or gender-atypical majors. This finding suggests that counseling services may be a promising tool for encouraging students to engage in gender-atypical study programs, thereby weakening gender segregation in higher education. Similarly, such interventions seem to widen students' horizons, especially for those who are uninformed, and lead them away from beaten paths in the direction of otherwise less frequently considered study programs. This may contribute to both a better major-interest fit and better educational outcomes, as well as improved posteducation matching in certain sectors of the labor market. While it is hard to assess the causal mechanisms in such encompassing treatments, the positive effect on one CDMSE subscale suggests that information could be a key factor in this process. Consistent with our expectations, the effect is not homogenous within the sample but appears to be particularly strong for students with lower levels of information. This adds to discussions on the effect of the heterogeneity of such interventions and reinforces that targeting may be highly relevant to their effectiveness.

At the same time, the results are not absolutely clear-cut. The average treatment effect is only significant in the case of gender-atypical choices, while the interaction with the information variables is only visible for considering non-beaten paths. While this puzzle cannot be solved completely, the much stronger prediction of the starting level of information for non-beaten path choices leads to the conclusion that the information argument accounts more heavily for less well-known study programs than for gender-atypical choices. This finding appears plausible, as gender segregation is driven by many more factors that are not affected by the counseling intervention. In sum, these results show that counseling services have the potential to alter the intended choice of major and that finding the right target group can make a larger difference, while additional research in this area is needed to arrive at more definite conclusions.

From a theoretical point of view, these results reinforce the notion that information (deficits) matters for the decision-making process of high school students. This conclusion is most apparent in the case of the strong correlation between the starting level of information and considering non-beaten path majors. On a related note, the finding that providing exogenous sources of information by means of counseling interventions increases the likelihood of considering less well-known or gender-atypical study programs further substantiates this notion. At a more abstract level, this suggests that information deficits should be considered in theoretical approaches aimed at predicting and understanding students' field of study decisions. While these findings may be important for multiple theoretical fields, they matter most for rational choice theory, as incomplete information about available options undermines the process of rationally weighing the costs and benefits against each other. Findings on choice of major that seem to contradict the basic assumption that students make rational choices might therefore be explained by information deficits that lead to misestimation of the different parameters that make up the utility function.

2.5.3 Robustness Checks and Methodological Notes

As outlined in the methods section, we perform various robustness checks and sensitivity analyses. Most importantly, we assess biases that could occur due to endogenous panel attrition, i.e., if drop-out between the treatment and control groups is asymmetric. For example, if particularly motivated participants from the control group refuse to answer the second survey, the treatment assignment would be confounded in the analysis sample. While asymmetric attrition on unobservables cannot be completely ruled out, we assess attrition issues in two ways.

First, we test whether attrition is asymmetric on observables (covariates as well as outcomes). To this end, we first run a series of regressions where participation in the second survey is the outcome and the interaction between treatment and covariates is the independent variable. Significant interactions would point to asymmetric attrition. Table A2.2 shows that none of the

15 interactions is significant at the 10% level. Second, we regress each pretreatment outcome (measured at wave 1) and covariate on the treatment assignment indicator, attrition indicator and their interaction (for a similar approach, see Wang et al., 2016). Once again, significant interactions would point to asymmetric attrition, but the results show that none of the interactions for the outcomes (Table A2.3) or covariates (Table A2.4) is significant. Finally, we perform a multivariate test for covariate imbalance in the remaining sample of the second wave by regressing treatment assignment on all covariates for those who participated in both waves. The results (see Table A2.5) show an explained variation of treatment assignment of almost zero ($R^2 < 0.02$, p-value of joint significance 0.98). These tests show that the attrition between treatment arms is fairly symmetric, making attrition bias less likely in our sample. It has to be admitted that unobserved endogenous attrition cannot be ruled out completely. The calculation of Lee bounds (Lee, 2009) shows that the estimated treatment effects might get insignificant for the lower bound scenario (the calculated bounds are [0.0090966 : 0.1267873] for non-beaten path and [-0.0058021 : 0.0823596] for gender-atypical study choice). However, it should be considered that Lee bounds perform a correction assuming the most extreme possible asymmetric attrition, by trimming excess observations from the treatment group with the highest and lowest values of the outcome variable. In our case, attrition regarding wave 1 outcomes is fairly symmetrical between both groups, which makes this seem like a hypothetical scenario. Nonetheless, we cannot completely rule out that our results are biased by endogenous selection, but all tests conducted up to this point strongly refute this concern.

Second, we rerun the estimation for all outcomes by handling attrition in four different ways. We begin by running more parsimonious models that only include the treatment assignment indicator and pretreatment outcome. We then run all models without dropping observations that have missing information on other outcome variables. Finally, we conduct multiple imputation for all covariates and subsequently for all missing outcomes. The results are summarized in Tables A2.6 to A2.11. The results remain similar for all outcomes and all four modes of handling missing data. One of the rare differences is that with item-missing and outcome imputation the impact on choosing non-beaten path study programs gains significance at the 10% level (see Table A2.6). In sum, these tests refute concerns about endogenous panel attrition and reveal that different modes of handling missing data do not lead to different conclusions.

Two further issues at the design level include possible spillover effects as well as substitute guidance counseling that students in the control group could have received through different means. While both mechanisms might bias treatment effects, they can quite safely be assumed to bias estimated effects downward. In this regard, the outlined treatment effects tend to represent lower bounds and would be larger in the absence of these possible biases.

Concerning the estimation itself and the coding of the variables, we replicate our estimations with bivariate probit as an alternative to the linear probability model (for binary outcomes), intent-to-treat (ITT) and bootstrap estimations (see Tables A2.12 to A2.14). In addition, we changed the threshold for when a study program is considered gender-atypical (Table A2.15). The results rarely differ compared to the baseline specification. The only major difference is that the effect on considering gender-atypical majors gains significance at the 5% level in the bivariate probit model. In contrast, the effect tends to become weaker if the threshold for the definition of gender-atypical study programs is increased. Overall, the high number of robustness checks confirms that the results are relatively robust to different measurements of the variables or changes in the estimation technique.

Once again referring to the estimation technique, we want to stress the importance of handling noncompliance appropriately. As outlined in the methods sections, employing an RCT and monitoring actual treatment status are major advantages of our research design. To facilitate these claims, we replicated the baseline specification by means of naïve OLS estimation using actual treatment participation as a regressor. The results (see Table 2.4 and Graph 2.3) show that the estimates change considerably. The impact seems to be more positive for five out of the six outcomes, with the gender-atypical variable gaining significance at the 5% level and the aggregate CDMSE category gaining significance at the 10% level ($t = 1.87$ compared to 0.73 in the baseline specification). These differences are remarkable given our rather high compliance rates, which suggest that even ignoring mild levels of noncompliance might substantially bias the treatment effect estimates. This finding reinforces the need for randomized designs at a more general level since self-selection into nonrandomized treatments is likely to be evenly strong or even stronger than the selectivity into actual participation among those randomly selected for the treatment after voluntary registration. The observation that our estimated effects are weaker than those from similar interventions may therefore (partly) result from differences in the research design, i.e., a slight overestimation of the effect in previous nonrandomized interventions.

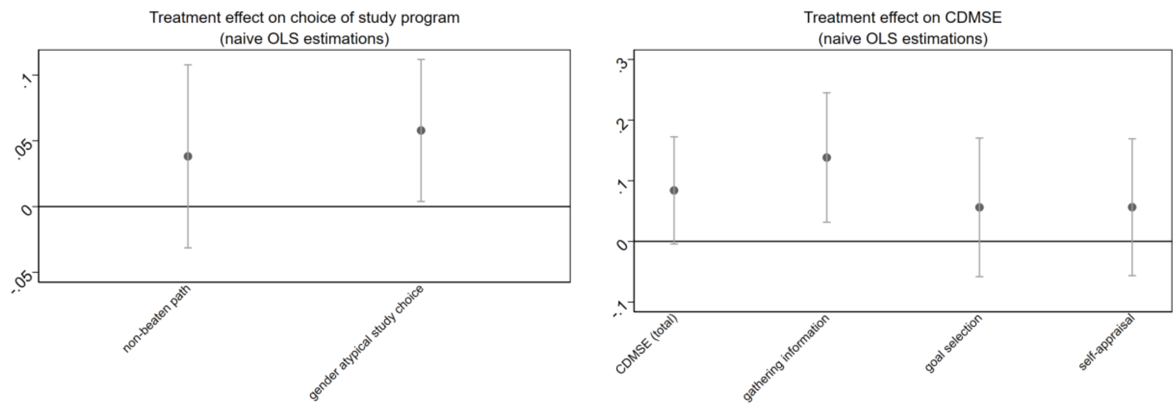
Finally, we want to make a cautious note on multiple testing. Significance tests might lose their validity if tests on one intervention are conducted for multiple outcomes or subgroups. In our case, however, it has to be considered that we are dealing with only two major outcomes, which are (1) consideration of a non-beaten path and (2) gender-atypical study choice consideration, while the treatment effects on CDMSE are only supplementary analyses. In regard to the interaction, we technically test two variables, but these variables constitute different measurements for the same concept. In this particular case, applying Bonferroni corrections would be misleading, as the finding that the effect is significant at the 5% level at the bottom of the distribution for both variables should be seen as a sign of robustness rather than a multiple testing issue (for an in-depth discussion see Streiner, 2015). Nevertheless, it should be considered that significance tests on the outcomes for the whole sample might be subject to multiple testing and should therefore be interpreted with some caution.

Table 2.4. Treatment effect on main outcomes, naïve OLS estimation

	(1) non-beaten path	(2) gender-atypical study choice	(3) CDMSE	(4) CDMSE (Gathering Information)	(5) CDMSE (Goal Selection)	(6) CDMSE (Self-Appraisal)
Treatment participation	0.0382	0.0579^{*}	0.0840^{+}	0.138^{*}	0.0561	0.0564
	(1.08)	(2.11)	(1.87)	(2.54)	(0.96)	(0.98)
Observations	574	574	574	574	574	574

OLS Regression with robust standard errors. Same control variables used as in Table 2.1 (not depicted). t statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01

Graph 2.3. Replication of treatment effects on main outcomes with naïve OLS estimation



2.6 Conclusion

This paper was inspired by the observation that high school students often focus on gender-typical and well-known study programs. Due to the undesirable consequences of these patterns at the individual and aggregate levels, this tendency prompted us to ask why these patterns emerge and whether policy interventions may weaken them. Building on a theoretical framework that combines rational choice and social psychological theory, we have argued that these patterns partly result from information deficits concerning college majors and the respective major-interest fit and may therefore be reduced by counseling interventions. The results from our field experiment with random assignment tend to confirm this argument. The workshop indeed seems to encourage students to consider less well-known and/or gender-atypical majors, particularly if the level of information they are starting with is low. At the same time, the results are somewhat mixed, and not all of the average treatment effects and interactions reach significance. From a theoretical point of view, our results confirm that information deficits seem to contribute to the existence or at least the strength of the observed choice patterns, which is of particular importance for further development of rational choice approaches in education research. The replication of the results with naïve OLS estimation reinforces both the need to consider and accurately address noncompliance and the general need for random assignment in future evaluations.

While these results inform higher education policy-making and contribute to previous research in this field in many regards, they also have limitations and raise at least three questions that remain to be answered. First, more research is needed to enable general conclusions to be drawn on the research questions addressed in this paper. On the one hand, the results are somewhat mixed and leave some room for interpretation. On the other hand, it remains subject to future research to confirm to what extent these findings generalize to other contexts (external validity). In fact, our interaction analyses suggest that treatment effects may (among others) differ with respect to the access to and quality of information that students possess prior to the intervention and may differ for other target groups or institutional contexts. The results might therefore differ in large-scale interventions targeted at all or a representative sample of students. Second, the effect size was rather moderate, suggesting that more intense treatments may be needed to achieve stronger effects, especially in the long run. The results therefore encourage further research on higher-intensity interventions to assess whether they yield more extensive effects. Finally, mirroring previous research, our analyses have focused on intended rather than actual study choice. Whether this short-term effect on the intended choice of major translates into long-term effects on actual choice remains subject to future research. A related and as yet open question is whether students are actually better off leaving the gender-typical and beaten paths in regard to their level of achievement in higher education and their postgraduate labor market outcomes. While this is a limitation of our results, a stronger focus on long-term effects could be beneficial for this body of literature as a whole, as research on the long-term effects of such interventions remains in its infancy.

2.7 Appendix

Table A2.1. Summary statistics

Variable	Coding	Mean	SD	Min	Max
Age	Coded in months	212.45	13.24	192	325
Grades school report 2017	Mean formed form grades in English, German & Math	2.21	0.68	1	4.33
Study aspirations of schoolmates	Share of schoolmates who want to go to university, ranging from 0 (none) to 6 (all)	3.83	0.94	0	6
Gender	1=female, 0=male	75.26		0	1
Parents: university degree	0=parents do not have a university degree, 1=at least one parent has a university degree	58.36		0	1
Household: siblings	1=respondent has siblings	69.51		0	1
Household: both parents	1=respondent lives with both parents	77.35		0	1
Parental study aspirations	1=parents want child to go to university	72.47		0	1
Treatment participation	1=participated in the workshop	37.46		0	1
Treatment assignment	1=was assigned to the workshop	44.6		0	1
Start of information gathering:					
during this schoolyear/not yet		40.94		0	1
during the last school year		42.86		0	1
before upper secondary education		16.2		0	1
Non-beaten path (at wave 1)	1=at least one considered major was a non-beaten path	72.13		0	1
Gender-atypical major (at wave 1)	1=at least one considered major was gender-atypical	18.29		0	1
Non-beaten path (at wave 2)	Same coding as in wave 1	70.73		0	1
Gender-atypical major (at wave 2)	Same coding as in wave 1	19.86		0	1
CDMSE (at wave 1)	Mean of the three Likert-scales below	3.96	0.64	1.33	5
CDMSE (Gathering Information)		4.2	0.77	1	5
CDMSE (Goal Selection)	“no confidence at all” (1) to “complete confidence” (5)	3.89	0.8	1	5
CDMSE (Self-Appraisal)		3.79	0.76	1	5
CDMSE (at wave 2)	Same coding as in wave 1	3.96	0.66	1.56	5
CDMSE (Gathering Information)		4.19	0.78	1	5
CDMSE (Goal Selection)		3.9	0.81	1	5
CDMSE (Self-Appraisal)		3.79	0.78	1	5

Observations = 574

Table A2.2. Series of regressions. Participation in wave 2 on treatment, one covariate and their interaction.

Interaction variable	**Coefficient**	**P-value**
CDMSE	0.0199	0.6563
CDMSE (Gathering Information)	-0.0039	0.9189
CDMSE (Goal Selection)	0.0251	0.4694
CDMSE (Self-Appraisal)	0.0108	0.7605
Non-beaten path	-0.0306	0.6141
Gender-atypical study choice	0.0268	0.6921
Age in months	-0.0035	0.1056
Grades school report 2017 (mean)	0.0224	0.601
Study aspirations of schoolmates	-0.0274	0.3614
Female	-0.0687	0.2879
Parents: university degree	0.0474	0.3948
Household: siblings	-0.0658	0.2661
Household: both parents	0.0687	0.3066
Parental study aspirations	0.0581	0.3418
Start of information gathering	0.0476	0.2206

OLS regressions with robust standard errors. Coefficients and p-values refer to the interaction term

Table A2.3. Check on asymmetric attrition between groups: regression of w1 outcome on treatment assignment, participation indicator and their interaction

	(1) non-beaten path	(2) gender-atypical study choice	(3) CDMSE	(4) CDMSE (Gathering Information)	(5) CDMSE (Goal Selection)	(6) CDMSE (Self-Appraisal)
Treatment assignment	0.0577	-0.0295	-0.0156	-0.0152	-0.0564	0.0263
	(0.67)	(-0.41)	(-0.11)	(-0.08)	(-0.34)	(0.18)
w2 participated	0.0257	0.00365	-0.138^{+}	-0.0477	-0.197*	-0.167^{+}
	(0.45)	(0.08)	(-1.71)	(-0.47)	(-2.06)	(-1.82)
w2 participated*treatment	-0.0457	0.0365	-0.00459	-0.0522	0.0507	-0.0148
	(-0.49)	(0.47)	(-0.03)	(-0.27)	(0.29)	(-0.09)
Constant	0.692**	0.179**	4.105**	4.274**	4.090**	3.949**
	(13.21)	(4.12)	(55.84)	(45.26)	(47.64)	(48.02)
Observations	725	725	725	725	725	725
R^2	0.001	0.001	0.007	0.003	0.007	0.007

OLS regressions with robust standard errors. t statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01

Table A2.4. Check on asymmetric attrition between groups: regression of w1 covariate on treatment assignment, participation indicator and their interaction

	(1) Age in months	(2) Grades school report 2017 (mean)	(3) Study aspirations of schoolmates	(4) Female	(5) Parents: University degree
Treatment assignment	3.201	-0.0828	0.135	0.0705	-0.0839
	(1.03)	(-0.57)	(0.69)	(0.81)	(-0.85)
w2 participated	-0.0231	-0.231*	0.111	0.0850	0.0247
	(-0.01)	(-2.50)	(0.94)	(1.47)	(0.39)
w2 participated*treatment	-4.746	0.0389	-0.176	-0.0860	0.111
	(-1.43)	(0.25)	(-0.84)	(-0.92)	(1.04)
Constant	213.7**	2.455**	3.740**	0.679**	0.545**
	(137.68)	(29.01)	(35.14)	(12.82)	(9.59)
Observations	723	717	723	725	721
R^2	0.005	0.016	0.001	0.003	0.004

Table A2.4. (continued)

	(6) Household: siblings	(7) Household: both parents	(8) Parental study aspirations	(9) Start of information gathering
Treatment assignment	0.113	-0.135	-0.0814	-0.125
	(1.25)	(-1.46)	(-0.92)	(-0.86)
w2 participated	0.0696	0.00862	-0.0384	-0.256**
	(1.15)	(0.16)	(-0.70)	(-2.74)
w2 participated*treatment	-0.103	0.121	0.0896	0.137
	(-1.05)	(1.23)	(0.93)	(0.87)
Constant	0.623**	0.766**	0.756**	2.000**
	(11.26)	(15.84)	(15.52)	(23.45)
Observations	717	717	725	725
R^2	0.003	0.005	0.001	0.012

OLS regressions with robust standard errors. t statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01

Table A2.5. Check for selection into treatment assignment (for those who participated in both waves)

	(1) Treatment assignment	(2) Treatment assignment	(3) Treatment assignment
Age in months	-0.00151		-0.00163
	(-0.98)		(-0.98)
Grades school report 2017 (mean)	-0.0240		-0.0262
	(-0.75)		(-0.80)
Study aspirations of schoolmates	-0.0178		-0.0171
	(-0.78)		(-0.75)
Female	-0.0289		-0.0158
	(-0.58)		(-0.28)
Parents: university degree	0.0116		0.00940
	(0.26)		(0.21)
Household: siblings	0.0255		0.0247
	(0.56)		(0.54)
Household: both parents	-0.0364		-0.0376
	(-0.72)		(-0.74)
Parental study aspirations	-0.00154		0.00223
	(-0.03)		(0.05)
Start of information gathering: during this school year/not yet			
during the last school year	-0.00653		0.000352
	(-0.14)		(0.01)
before upper- secondary education	0.0310		0.0414
	(0.50)		(0.65)
CDMSE (Gathering Information)		-0.0400	-0.0442
		(-1.28)	(-1.33)
CDMSE (Goal Selection)		0.00598	0.00717
		(0.18)	(0.21)
CDMSE (Self-Appraisal)		0.0191	0.0160
		(0.56)	(0.46)
Non-beaten path		0.0142	0.0195
		(0.31)	(0.41)
Gender-atypical study choice		0.00486	-0.00487
		(0.09)	(-0.08)
Constant		0.494^{**}	0.998^{*}
		(3.71)	(2.36)
Observations	594	607	594
R^2	0.005	0.003	0.009

OLS regressions with robust standard errors. t statistics in parentheses.
Significance levels are indicated as follows: + p<0.10, * p<0.05, ** p<0.01

Table A2.6. OLS-ITT analysis on non-beaten path with different approaches to account for missing data

	(1) Only treatment and pre-treatment outcome	(2) Full model	(3) Item-missing multiple imputation[1]	(4) Outcome and item-missing multiple imputation[1]
Treatment	0.0512	0.0424	0.0556^{+}	0.0556^{+}
	(1.52)	(1.25)	(1.65)	(1.74)
Pre-treatment outcome	0.447^{**}	0.435^{**}	0.434^{**}	0.426^{**}
	(10.67)	(10.29)	(10.36)	(10.31)
Female		-0.0305	-0.0231	-0.0223
		(-0.79)	(-0.60)	(-0.54)
Parents: university degree		-0.0674^{+}	-0.0608^{+}	-0.0575
		(-1.87)	(-1.69)	(-1.62)
Household: siblings		0.0152	0.0124	0.0115
		(0.42)	(0.34)	(0.32)
Household: both parents		0.00711	0.0141	0.0186
		(0.17)	(0.33)	(0.44)
Parental study aspirations		-0.0736^{*}	-0.0631^{+}	-0.0639^{+}
		(-2.04)	(-1.75)	(-1.86)
Start of information gathering: during this school year/not yet				
during the last school year		0.112^{**}	0.116^{**}	0.126^{**}
		(2.94)	(3.08)	(3.48)
before upper secondary education		0.160^{**}	0.161^{**}	0.183^{**}
		(3.24)	(3.31)	(3.96)
Age in months		0.00128	0.00101	0.000986
		(1.24)	(1.27)	(1.19)
Grades school report 2017 (mean)		0.0110	0.0114	0.0125
		(0.45)	(0.46)	(0.51)
Study aspirations of schoolmates		0.0247	0.0222	0.0234
		(1.32)	(1.20)	(1.24)
Constant	0.360^{**}	0.00868	0.0444	0.0381
	(9.01)	(0.03)	(0.20)	(0.16)
Observations	598	585	598	725

OLS regressions with robust standard errors. t statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01. [1]Multiple imputation using chained equations (MICE) with 20 imputations, separate imputation models for treatment and control group

Table A2.7. OLS-ITT analysis on gender-atypical study choice with different approaches to account for missing data

	(1) Only treatment and pre-treatment outcome	(2) Full model	(3) Item-missing multiple imputation[1]	(4) Outcome and item-missing multiple imputation[1]
Treatment	0.0519^{+} (1.90)	0.0490^{+} (1.87)	0.0497^{+} (1.92)	0.0479^{+} (1.75)
Pre-treatment outcome	0.596^{**} (12.97)	0.477^{**} (8.67)	0.481^{**} (8.93)	0.473^{**} (8.84)
Female		-0.238^{**} (-5.00)	-0.240^{**} (-5.10)	-0.244^{**} (-5.30)
Parents: university degree		-0.0205 (-0.75)	-0.0226 (-0.84)	-0.0224 (-0.82)
Household: siblings		-0.0439 (-1.49)	-0.0439 (-1.52)	-0.0466 (-1.51)
Household: both parents		-0.000603 (-0.02)	-0.00562 (-0.17)	-0.00106 (-0.03)
Parental study aspirations		0.0405 (1.40)	0.0377 (1.33)	0.0339 (1.12)
Start of information gathering: during this school year/not yet				
during the last school year		-0.0245 (-0.88)	-0.0263 (-0.96)	-0.0177 (-0.64)
before upper secondary education		0.0469 (1.11)	0.0538 (1.30)	0.0652 (1.43)
Age in months		0.000356 (0.35)	0.000207 (0.28)	0.000300 (0.40)
Grades school report 2017 (mean)		-0.0143 (-0.73)	-0.0130 (-0.67)	-0.0108 (-0.56)
Study aspirations of schoolmates		0.0145 (1.02)	0.0151 (1.08)	0.0174 (1.20)
Constant	0.0656^{**} (4.19)	0.186 (0.74)	0.219 (1.12)	0.189 (0.92)
Observations	598	585	598	725

OLS regressions with robust standard errors. t statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01. [1]Multiple imputation using chained equations (MICE) with 20 imputations, separate imputation models for treatment and control group

Table A2.8. OLS-ITT analysis on CDMSE with different approaches to account for missing data

	(1) Only treatment and pre-treatment outcome	(2) Full model	(3) Item-missing multiple imputation[1]	(4) Outcome and item-missing multiple imputation[1]
Treatment	0.0340	0.0325	0.0367	0.0355
	(0.77)	(0.72)	(0.82)	(0.84)
Pre-treatment outcome	0.602**	0.593**	0.586**	0.549**
	(14.13)	(12.63)	(12.67)	(13.05)
Female		-0.0293	-0.0224	-0.0356
		(-0.53)	(-0.41)	(-0.70)
Parents: university degree		0.0272	0.0310	0.0369
		(0.55)	(0.63)	(0.80)
Household: siblings		-0.0766	-0.0794	-0.0844+
		(-1.50)	(-1.57)	(-1.80)
Household: both parents		0.00779	0.00177	0.000459
		(0.14)	(0.03)	(0.01)
Parental study aspirations		-0.00953	-0.0111	-0.000691
		(-0.18)	(-0.21)	(-0.01)
Start of information gathering: during this school year/not yet				
during the last school year		0.0589	0.0684	0.0699
		(1.12)	(1.32)	(1.42)
before upper secondary education		0.136*	0.136*	0.134*
		(2.10)	(2.13)	(2.05)
Age in months		0.000728	0.000831	0.000578
		(0.43)	(0.68)	(0.46)
Grades school report 2017 (mean)		0.0168	0.0186	0.0257
		(0.46)	(0.52)	(0.81)
Study aspirations of schoolmates		0.0258	0.0252	0.0239
		(1.05)	(1.05)	(1.03)
Constant	1.565**	1.321**	1.320**	1.513**
	(8.85)	(2.82)	(3.48)	(4.19)
Observations	587	575	587	725

OLS regressions with robust standard errors. t statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01. [1]Multiple imputation using chained equations (MICE) with 20 imputations, separate imputation models for treatment and control group

Table A2.9. OLS-ITT analysis on CDMSE (Gathering Information) with different approaches to account for missing data

	(1) Only treatment and pre-treatment outcome	(2) Full model	(3) Item-missing multiple imputation[1]	(4) Outcome and item-missing multiple imputation[1]
Treatment	0.109^{*}	0.112^{*}	0.114^{*}	0.107^{*}
	(2.04)	(2.05)	(2.11)	(2.08)
Pre-treatment outcome	0.531^{**}	0.509^{**}	0.506^{**}	0.473^{**}
	(11.58)	(10.02)	(10.12)	(10.34)
Female		0.0878	0.0905	0.0849
		(1.20)	(1.26)	(1.23)
Parents: university degree		-0.0135	-0.0146	-0.00517
		(-0.22)	(-0.24)	(-0.09)
Household: siblings		-0.0496	-0.0494	-0.0655
		(-0.78)	(-0.79)	(-1.15)
Household: both parents		0.0551	0.0490	0.0562
		(0.76)	(0.69)	(0.81)
Parental study aspirations		-0.0763	-0.0776	-0.0683
		(-1.22)	(-1.25)	(-1.15)
Start of information gathering: during this school year/not yet				
during the last school year		0.0949	0.101	0.108^{+}
		(1.51)	(1.63)	(1.71)
before upper secondary education		0.141^{+}	0.154^{+}	0.157^{+}
		(1.75)	(1.95)	(1.92)
Age in months		-0.00123	-0.00136	-0.00127
		(-0.48)	(-0.75)	(-0.73)
Grades school report 2017 (mean)		0.0364	0.0412	0.0497
		(0.80)	(0.92)	(1.21)
Study aspirations of schoolmates		0.0533^{+}	0.0514^{+}	0.0451
		(1.74)	(1.72)	(1.51)
Constant	1.915^{**}	1.908^{**}	1.943^{**}	2.075^{**}
	(9.26)	(2.85)	(3.66)	(4.24)
Observations	589	577	589	725

OLS regressions with robust standard errors. t statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01. [1]Multiple imputation using chained equations (MICE) with 20 imputations, separate imputation models for treatment and control group

Table A2.10. OLS-ITT analysis on CDMSE (Self-Appraisal) with different approaches to account for missing data

	(1) Only treatment and pre-treatment outcome	(2) Full model	(3) Item-missing multiple imputation[1]	(4) Outcome and item-missing multiple imputation[1]
Treatment	0.00268	0.00426	0.00457	0.00636
	(0.05)	(0.07)	(0.08)	(0.12)
Pre-treatment outcome	0.527**	0.518**	0.511**	0.485**
	(13.67)	(12.64)	(12.50)	(12.67)
Female		-0.0532	-0.0537	-0.0769
		(-0.82)	(-0.84)	(-1.23)
Parents: university degree		0.0498	0.0453	0.0509
		(0.81)	(0.75)	(0.89)
Household: siblings		-0.0276	-0.0257	-0.0306
		(-0.43)	(-0.40)	(-0.52)
Household: both parents		-0.0177	-0.0237	-0.0350
		(-0.25)	(-0.34)	(-0.53)
Parental study aspirations		0.110+	0.101	0.110+
		(1.66)	(1.53)	(1.67)
Start of information gathering: during this school year/not yet				
during the last school year		0.0598	0.0735	0.0737
		(0.92)	(1.15)	(1.18)
before upper secondary education		0.169*	0.165*	0.148+
		(2.01)	(2.02)	(1.80)
Age in months		0.00319	0.00266+	0.00222
		(1.52)	(1.73)	(1.39)
Grades school report 2017 (mean)		-0.0276	-0.0251	-0.0146
		(-0.63)	(-0.58)	(-0.35)
Study aspirations of schoolmates		0.0371	0.0349	0.0369
		(1.20)	(1.16)	(1.31)
Constant	1.789**	0.972+	1.125**	1.304**
	(11.93)	(1.80)	(2.60)	(3.08)
Observations	587	575	587	725

OLS regressions with robust standard errors. t statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01. [1]Multiple imputation using chained equations (MICE) with 20 imputations, separate imputation models for treatment and control group

Table A2.11. OLS-ITT analysis on CDMSE (Goal Selection) with different approaches to account for missing data

	(1) Only treatment and pre-treatment outcome	(2) Full model	(3) Item-missing multiple imputation[1]	(4) Outcome and item-missing multiple imputation[1]
Treatment	-0.0181	-0.0279	-0.0173	-0.0110
	(-0.31)	(-0.48)	(-0.30)	(-0.19)
Pre-treatment outcome	0.539**	0.527**	0.520**	0.486**
	(12.24)	(11.45)	(11.47)	(11.82)
Female		-0.0952	-0.0821	-0.0866
		(-1.35)	(-1.18)	(-1.31)
Parents: university degree		0.0380	0.0539	0.0535
		(0.60)	(0.86)	(0.86)
Household: siblings		-0.174**	-0.185**	-0.167**
		(-2.74)	(-2.93)	(-2.75)
Household: both parents		-0.00593	-0.0104	-0.0133
		(-0.08)	(-0.15)	(-0.18)
Parental study aspirations		-0.00335	0.00161	0.00414
		(-0.05)	(0.02)	(0.07)
Start of information gathering: during this school year/not yet				
during the last school year		0.0871	0.0906	0.0896
		(1.28)	(1.35)	(1.42)
before upper secondary education		0.195*	0.182*	0.182*
		(2.35)	(2.22)	(2.21)
Age in months		-0.000136	0.000822	0.000522
		(-0.07)	(0.49)	(0.29)
Grades school report 2017 (mean)		0.0527	0.0507	0.0441
		(1.09)	(1.06)	(1.02)
Study aspirations of schoolmates		-0.0208	-0.0181	-0.0177
		(-0.65)	(-0.58)	(-0.59)
Constant	1.813**	1.961**	1.766**	1.965**
	(9.94)	(3.46)	(3.60)	(4.05)
Observations	589	577	589	725

OLS regressions with robust standard errors. t statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01. [1]Multiple imputation using chained equations (MICE) with 20 imputations, separate imputation models for treatment and control group

Table A2.12. Treatment effects, bootstrap estimation

	(1) non-beaten path	(2) gender-atypical study choice	(3) CDMSE	(4) CDMSE (Gathering Information)	(5) CDMSE (Goal Selection)	(6) CDMSE (Self-Appraisal)
Treatment	0.0512	0.0636^{+}	0.0421	0.144*	-0.0262	0.00672
	(1.15)	(1.80)	(0.75)	(2.00)	(-0.36)	(0.09)
Observations	574	574	574	574	574	574

IV regressions with bootstrap estimation. Covariates as in baseline specification Table 2.1 (not shown).
z statistics in parentheses, significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01

Table A2.13. Treatment effects, ITT-estimation

	(1) non-beaten path	(2) gender-atypical study choice	(3) CDMSE	(4) CDMSE (Gathering Information)	(5) CDMSE (Goal Selection)	(6) CDMSE (Self-Appraisal)
Treatment	0.0395	0.0491^{+}	0.0325	0.111*	-0.0202	0.00519
	(1.16)	(1.85)	(0.72)	(2.02)	(-0.35)	(0.09)
Observations	574	574	574	574	574	574

OLS regressions with robust standard errors. Covariates as in baseline specification Table 2.1 (not shown).
t statistics in parentheses, significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01

Table A2.14. Treatment effect on main outcomes, bivariate probit

	(1) non-beaten path	(2) gender-atypical study choice
Treatment	0.178	0.374*
	(1.18)	(2.07)
Observations	574	574

Unstandardized coefficients from bivariate probit models. Coefficients do not represent marginal effects. Covariates as in baseline specification Table 2.1 (not shown). z statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01

Table A2.15. Treatment effect on gender-atypical study choice, different definition of gender-atypical choice

	(1) Threshold: 60% opposite sex	(2) Threshold: 65% opposite sex	(3) Threshold: 70% opposite sex
Treatment	0.0636^{+}	0.0547^{+}	0.0127
	(1.87)	(1.70)	(0.43)
Observations	574	574	574

IV regressions with robust standard errors. Covariates as in baseline specification Table 2.1 (not shown). z statistics in parentheses. Significance levels indicated as follows: + < 0.1, * < 0.05, ** < 0.01

Chapter 3: “University Field of Study Homogeneity within Close Friend Networks - Does Information Matter? Evidence from an Experiment.”

3.1 Abstract

Students and their close friends are homogeneous in respect to many educational outcomes. While the causal mechanisms that drive this homogeneity are manifold, one important reason could be that within close friend networks information tends to be redundant and echo chamber effects are likely to occur. This study focusses on field of study homogeneity in close friend networks, and how providing students with novel information can change it. To assess the role of information empirically, egocentric-network data on field of study *intentions,* and later *choices,* of German high school students (ego) and their close friends (alters) was collected at three time points. Between waves, a randomized controlled trial was conducted in which the treatment group participated in a counseling workshop and was hereby exposed to an exogeneous source of information. Results of the experiment show that the information intervention reduced field of study intention homogeneity between students and their close friends but did not change field of study choice homogeneity in the long run. Furthermore, the effect of the information treatment was not moderated by pre-treatment network homogeneity. From a theoretical point of view, the results suggest that network homogeneity (partly) results from access to similar information among close friends and decreases when external sources of information become available.

Keywords: Network Homogeneity, Information Experiment, Field of Study, Close Friends, Higher Education

Author Contributions:

This paper is single authored. I developed the theoretical arguments, designed and implemented the ego-networks, conducted the analyses and wrote the paper.

3.2 Introduction

Educational decisions do not happen in social vacuums. The network students are embedded in can have a profound impact on their educational trajectory (Ream & Rumberger, 2008). Research has shown that friends and peers influence many educational decisions and outcomes, such as the choice of major (Giorgi et al., 2010; Lyle, 2007; Poldin et al., 2015), college enrollment (Alvarado & López Turley, 2012; Bifulco et al., 2014; Fletcher, 2015), university dropout (Cherng et al., 2013; Sommerfeld, 2016) and academic achievement (Gašević et al., 2013; Kretschmer et al., 2018; Lomi et al., 2011; Rambaran et al., 2017). A common narrative within these studies is that educational intentions, attitudes, behaviors and choices of friends and peers tend to be homogenous, a finding that mirrors the general tenor of network research that people associate with similar others and become more similar over time (Klepper et al., 2010; McPherson et al., 2001).

The trend towards educational homogeneity within friend networks has the potential to exacerbate inequality (DiMaggio & Garip, 2012; Raabe et al., 2019) because it can be a conduit for cumulative (dis-)advantage. For instance, students embedded in a high achieving and aspiring network may become even higher achievers and able to realize their ambitions, whereas students embedded in a low achieving and aspiring network can be caught in a race to the bottom (Bond et al., 2017). The frequently observed trend towards homogeneity may thus result in a widening gap between advantaged and disadvantaged students. Consequently, it is pertinent to theoretically understand how educational homogeneity is formed and from there on out test how educational homogeneity may be reduced.

This paper takes a close look at one understudied aspect of educational homogeneity, namely field of study homogeneity between students and their close friends, in respect to both the *intended* and *chosen* field of study. One influential idea within network theory that can be used to explain field of study homogeneity in close friend networks is the role of information (Burt, 2001; Granovetter, 1973). In combination with a rational choice framework, where students are said to make rational decisions based on the information they have available, the following insights become evident. While gathering information on what to do after high school and building an educational decision making utility function (Breen & Goldthorpe, 1997; Erikson & Jonsson, 1996; Sewell et al., 1969), students swap information on university and field of study expectations with their friends. These social sources of information are vital for forming educational intentions and expectations (Hemsley-Brown & Oplatka, 2015; Obermeit, 2012).

But some social sources of information, especially strong ties like close friends, tend to provide little novel information and may become echo chambers (Burt, 2005; Granovetter, 1973). This line of argumentation presents an important, testable implication. If lack of information diversity is a driving force for field of study homogeneity between students and their close friends, said homogeneity should be reduced when students receive new information. In other words, by providing students with novel and relevant information to update their personal utility functions, field of study intentions and choices within their close friend networks could become more heterogenous. An effect that may depend on prior level of network homogeneity because the more homogeneous a network is, the greater the amount of information redundancy and echo chamber potential.

Utilizing an experimental study design, I show how an information intervention (student counseling workshop) in Germany affects short- (intentions) and long-term (choices) field of study overlap between students (egos) and their close friends (alters). Egocentric network data was collected at three different time points: (wave 1) before the treatment, (wave 2) after the treatment, but still in high school and (wave 3) after high school. Egos were randomly allocated to treatment and control group to assess the treatment effect of the information intervention on the degree of field of study intention overlap in wave 2 and choice overlap in wave 3 between ego and alters. Additionally, I assess if the treatment effect on intention and choice overlap is dependent on the pre-treatment degree of homogeneity within the close friend ego-network.

This study makes several contributions to the existing literature on network effect on educational outcomes. It employs experimental procedures and explicitly tests for the effect of information on network homogeneity, which is uncommon in network studies. Additionally, it informs higher education research, where information treatments are more common, that network parameters should not be ignored. Lastly, a major shortcoming of many information treatment experiments and to some extent network research is that they frequently only look at short-term effects and neglect the long-term, which this study overcomes by presenting both.

3.3 Literature, Theory and Hypotheses

3.3.1 Literature Review

In sociology and economics, network effects on educational outcomes have received considerable amounts of theoretical attention (e.g. Coleman, 1988; Sewell et al., 1969) and have amassed an abundance of empirical research (for a review see Sacerdote, 2011). One of the most frequently analyzed types of network effects on educational outcomes are peer effects on educational achievement. The literature shows that students with peers or friends who have a positive attitude towards education are less likely to drop out of school (Carbonaro & Workman, 2016; Ream & Rumberger, 2008) and are more successful in school (Cherng et al., 2013; Crosnoe et al., 2003; Kretschmer et al., 2018; Rambaran et al., 2017) and college (Hasan & Bagde, 2013; Lomi et al., 2011; Zimmerman, 2003). On the flipside, pupils with low-achieving peers display decreased educational performance themselves (Bond et al., 2017). A related body of literature assesses the impact of academic aspirations, which shows that being embedded in a peer group with high aspirations leads pupils to have high aspirations themselves (Duncan et al., 1968; Kandel & Lesser, 1969; Rosenqvist, 2017), although these effects are only moderate. Research on educational decisions, such as enrolling into university, consistently show that young adults have a higher probability of attending university if their peers and parents have university aspirations or attended university themselves (Alvarado & López Turley, 2012; Fletcher, 2015; Sommerfeld, 2016). Similar evidence is uncovered for university students, who are less likely to drop out of university and perform better in university with an academically oriented peer group (Bifulco et al., 2014; Gašević et al., 2013; Ost, 2010).

Network effects on choice of university major or specialization within a chosen field of study has received comparatively little attention and has produced mixed results. Poldin et al. (2015) examined specialization overlap between students of a Russian college and found that study partners as well as friends tend to choose the same specialization, an effect that was even larger for reciprocal friendships. A study by Lyle (2007), leveraging an experimental design, found that role models affect the choice of plebes within a military school in the US to choose an engineering major, but he did not find an effect on the choice of other majors and no effect of peers on choice of major. Arcidiacono & Nicholson (2005) found peer effects pertaining to specialization in a medical school, that disappeared once school fixed effects were included. Finally, Sacerdote (2001), also utilizing an experimental design, found no effect of residential peers on choice of major. A reason as to why some researchers find effects and others don't may be due to different definitions of the network. It is important to pin down who the significant others are that influence educational decisions (Sewell et al., 1969). Empirical evidence suggests that peers may not be significant enough to influence educational specialization (Lyle, 2007; Sacerdote, 2001) but friends are (Poldin et al., 2015).

Ultimately, previous research shows that there are persistent and well-documented network effects, especially friend networks, on educational outcomes, but there is little research on how or why these network effects occur. In the following section, I develop theoretical arguments and hypotheses for one network effect mechanism, namely the impact of information.

3.3.2 Theoretical Arguments and Hypotheses

The literature on network influence on educational outcomes paints a clear picture: students' friend networks start off and become more similar over time with respect to educational achievement, aspirations, attitudes and choices. From a network theoretical perspective, friendship networks are homogenous for two reasons. First, students select friends based on homophily (Goodreau et al., 2009; Kossinets & Watts, 2009; McPherson et al., 2001). Within the pool of potential friends, largely determined by geographical propinquity and institutional factors (Frank et al., 2013; Kossinets & Watts, 2009), students tend to befriend similar others (Goodreau et al., 2009; Klepper et al., 2010). Second, friends tend to become more homogenous in their characteristics over time through reciprocal social influence (Coleman, 1988; DiMaggio & Garip, 2012; Festinger, 1954).

To understand how information can promote homogeneity formation inside close friend networks we first need to look at these types of networks in more detail. Close friend networks persist of strong ties, characterized by a large amount of time investment, high emotional intensity, intimacy and reciprocal services (Granovetter, 1973; Krackhardt, 1992). Through triadic closure (Goodreau et al., 2009; Granovetter, 1973), such networks tend to become very dense, interconnected and small in size. The information close friends provide is not only frequently utilized by students, especially when it comes to educational choices regarding tertiary education (Galotti & Mark, 1994; Hemsley-Brown & Oplatka, 2015; Johnston, 2010; Obermeit, 2012), but is also valued highly because the information is treated as trusted and reliable (Crosnoe et al., 2003; Hallinan & Williams, 1990; Klepper et al., 2010). But there is a potential drawback to the information close friends provide, which may contribute to homogeneity formation inside these networks. The processes through which information within these networks may foster homogeneity are: *information redundancy* and the subsequent potential for *echo chamber effects* (Burt, 2005).

Information redundancy is high in close friend networks because students choose friends based on similarity (homophily), therefore the information they possess and disclose also tends to be similar (Burt, 2005). Echo chamber effects occur in these networks because students exchange information on, e.g., potential fields of studies (the educational outcome under scrutiny in this article) based on this limited information diversity. As a result, similar information on study programs and fields of study in general gets repeated frequently, solidifying in the minds of network members and becoming salient information with a high chance to influence educational decision making (Fletcher, 2012; Rosenqvist, 2017). Familiarity with a field of study, fostered through echo chamber effects in the close friend networks, may make students more comfortable with these educational pathways. In other words, the priming that takes place within the close friend networks through the high frequency of conversing about a limited number of study programs, or fields of study, makes these educational pathways highly accessible and good candidates for educational choices. In sum, close friends are a valued source of information that tend to provide little new information and foster the potential for acting on the information that gets repeated within the close friend network.

These arguments are also convincing when educational-decision making is viewed through the lens of rational choice theory (Breen & Goldthorpe, 1997; Erikson & Jonsson, 1996). Rational choice theory postulates that students choose the educational alternative that provides them with the highest utility given their subjective probability of success, costs and benefits of the educational alternatives. But the parameters of the utility function can only be accurately approximated for fields of study students have reliable (or thought to be reliable) information on, such as the fields of study being talked about in the close friend network. Consequently, these fields of study have a high chance of being considered and chosen by members of the close friend network, thus promoting network homogeneity.

It follows that providing students with novel information through an external source reduces information redundancy within the close friend network, which in turn leads to decreases in study program homogeneity within this network. In other words, through novel external information students may find a new best fitting field of study that is different from the fields of study dominant within the close friend network, thus diversifying study program intentions and choices within the close friend network.

Hypothesis 1:

> *Field of study intention and choice homogeneity within close friend networks is reduced when students receive novel information from an external source.*

However, the question arises whether providing students with novel information is only effective in the short and not in the long term. It is possible that students are considering or intending to study something similar as their friends as a first point of reference, but as soon as they vigorously engage in the subject matter and do more independent research, they form their own opinions and make an interest and skill congruent field of study decision. Considering that most students start gathering information before enrolling into university and carefully consider educational alternatives at some point (Obermeit, 2012; Wiswall & Zafar, 2015), even if they start at different timepoints, the information injection may only lead to diversification in the short run.

Hypothesis 2:

> *The effect of novel information from an external source is higher for field of study intention homogeneity than for field of study choice homogeneity.*

Another point that warrants consideration is the fact that not all friend networks are equally homogenous. Some networks may have a higher heterogeneity to start with, which should influence the degree of information redundancy within that network. The more homogeneous a network is, the higher the potential for information redundancy and vice versa. Consequently, the impact of an external source of information may be dependent on the initial homogeneity of the close friend network, which leads to my last hypothesis:

Hypothesis 3:

> *The effect of novel information from an external source on field of study intention and choice homogeneity is larger for students with a homogeneous close friend network than for students with a heterogeneous close friend network.*

3.4 Methods

To estimate changes in field of study homogeneity within close friend networks through novel external information, a randomized control trial (RCT) was conducted. Aside from providing causal effect estimates, the advantage of RCT is that all unobserved factors, such as information flow and density of the larger peer network, are by design equally distributed across treatment and control group through randomization and subsequently pose no threat to the experiment's internal validity and estimated treatment effects.

3.4.1 Research Design and Sample

Participants of the experiment were high school students set to graduate in 2019 with the German higher education entrance qualification diploma ("Abitur"). Students were recruited in close collaboration with the department of student counseling of two large universities in Germany, who conducted the workshops. By utilizing the connections of these departments, schools were contacted and asked to deliver information on the university counseling workshop to the appropriate students. Furthermore, the workshop was promoted on open campus days and social media channels.

To participate in the workshop, interested students needed to register for the study online. Within the self-administered registration process, the first survey (w1), demographic variables and pre-treatment outcomes were measured. At the end of the first survey, participants were randomly allocated to either treatment or control group. Students in the treatment group were given the opportunity to participate in the workshop, while students in the control group were compensated with participation in a lottery. 725 participants in total completed the first survey. The second self-administered survey (w2) was distributed via mail a few months after the workshop took place, while the students were still in high school. Within the second survey, the short-term effects of the treatment were assessed. 607 students participated in the second survey (response rate of 83.72%). The third survey (w3) was distributed at the end of 2019, after participants had graduated from high school and had a chance to enroll into university. In this wave, the actual educational choices of students were gathered. 567 (response rate of 78.21%) students participated in the third survey of which 343 (60.49%) had started university. Only students who provided full information on all relevant covariates were used in the analyses, reducing the observation count (more on that later).

3.4.2 Information Treatment

The information treatment was a daylong university guidance workshop on campus, administered by the aforementioned departments of student counseling at their respective university. Every student allocated to the treatment group was asked to participate in an online self-assessment test, designed by the Federal Employment Agency of Germany, before going to the workshop. This 2-3 hour long self-assessment tool tests cognitive skills, gathers personal preferences and suggests fitting study opportunities. The results of the test were used within the workshop as a first point of orientation and basis for discussion. Within the workshop, students (1) explored their interests, values and motives, (2) discussed the results of the online self-assessment test, (3) were given general information on study programs and their content, (4) put their plans for their future in writing, (5) and received first-hand information by a student on what it is like to study at university. 28 workshops with an average of 9 participants were held as part of the experiment.

3.4.3 Data and Measurements

The main outcomes are network homogeneity pertaining to intended and chosen field of study between students and their close friends. Within an egocentric-network framework, homogeneity is defined as similarity between ego (student) and alters (close friends) regarding specific attributes (Crossley et al., 2015). Strictly speaking, homogeneity in this context refers to ego's connection with similar others and not necessarily overall network homogeneity. In wave 1 and 2, the data collection process was as follows. First, participants were asked which major they want to study after graduating from high school. To attain a measure for the degree of overlap between participants and their friends an egocentric-network was created using a name-generator. Each study participant was asked to provide the initials of up to four of their closest (best) friends. The comparatively low number of possible alters and the few questions asked about each alter helps reduce fatigue and satisficing effects within the web-survey (Silber et al., 2019). After the name generating process, participants were asked to provide information on study intention, university major intention and where they know the friend from for each alter. Majors of egos and alters were coded into 11 fields of study (e.g., social sciences, natural sciences, engineering etc.), according to the classification scheme from the German Office of Statistics (Destatis, 2020) with the addition of teaching as an additional category. Afterwards the percentage of field of study intention overlap between egos and their alters was calculated. Alters who did not want to study were coded as not overlapping. For example, if ego wanted to study engineering and named four friends, 2 who want to study engineering as well, 1 who wants to study natural sciences and one who does not plan on studying at university, ego has an intended field of study overlap of 50%. During wave 1 and 2 many respondents did not know what to study yet, which poses a challenge for matching their answers with those of their friends. In the main analysis, I have chosen to treat the case where students did not know what to study and indicated that their friends did not know what to study as a match. As a robustness check I also conducted the analyses when they are treated as a mismatch or dropped. In wave

3 participants who started university were asked whether they enrolled in university and if so, which major they chose. Later, the initials of their wave 1 alters were displayed and participants were asked to indicate if and what those alters were studying. Major overlap was coded the same as in wave 1 and 2, with the exception that there was no uncertainty pertaining to the major they are studying, since participants either started studying a specific major or did not.

Table 3.1 provides summary statistics and information on the coding of all variables used in the main analysis. The distribution of intended and chosen field of study overlap is positively skewed (see Table 3.1), which means that to some degree the close friend networks are already heterogenous regarding their field of study intentions and choices. This is not per se a problem and to some degree an artifact of how the variable is coded. Different egocentric network characteristics show that these networks are homogeneous, such as intention to attend university (73.47% of egos friends also want to attend university in wave 1), or gender (82.77% same gendered friends in wave 1), indicating that the egocentric networks are homogenous in respect to other measures. The key difference between these variables and intention and choice overlap pertaining to field of study is the number of possible values within the variables. There are 11 values for field of study, whereas the other variables only have 2-3 possible values. Consequently, high heterogeneity is at least partly attributable to the coding of the variable. Furthermore, friends who do not want to study are coded as not overlapping, as such they are a contributor to heterogeneity. Another possible explanation for the observed heterogeneity could be the self-selected sample. The sample consists of students who are interested in participating in a counseling workshop, which means that this group of students is already more actively engaged in the information search process than students who do not attend these kinds of workshops. The information treatment may thus be subject to a ceiling effect since heterogeneity as well as information cannot increase indefinitely. The network homogeneity measures to test hypothesis 2 are the pre-treatment percentage of field of study overlap and the percentage of alters ego knows from school, both measured in wave 1. These two homogeneity measures are included as interaction terms in the upcoming analyses (Treatment * % pre-treatment field of study overlap, Treatment * % of friends from school).

The analyses also included pre-treatment outcomes and time invariant covariates, which increase the efficiency of the treatment effect estimation and correct for possible imbalances (Imbens & Rubin, 2015). The pre-treatment outcome is the intended field of study overlap in wave 1. Time invariant covariates and wave 1 control variables included in the analyses are age in months, gender, school grades, parental education level, the study aspirations parents have for ego, study aspirations of students in ego's class, whether the participant has siblings, whether ego lives with both parents and finally, when the participant started gathering information on university majors.

Table 3.1. Data description and summary statistics

Variables	Description	Mean (Std. dv.)	Min-Max
Wave 1[1]			
Pre-treatment Outcome			
% intended field of study overlap	Percentage of field of study overlap between ego and his close friends	0.24 (0.27)	0-1
Interaction Variables			
% pre-treatment field of study overlap	Same variable as the pre-treatment outcome	0.24 (0.27)	0-1
% of friends from school	Percentage of close friends ego knows from school	0.78 (0.27)	0-1
Control Variables			
Age	Age in months	212.58 (12.58)	192-323
School grades	Mean of grade school report, consisting of the grades in German, Math and English	2.19 (0.69)	1-4.33
Schoolmates: study aspiration	The amount of students who wanted to attend university in ego's class (0 "no one" to 6 "everyone")	3.83 (0.92)	0-6
Gender	Ego's gender (0 male, 1 female)	76.96	0-1
Parents: university degree	Whether at least one parent of ego has a university degree (0 "neither parent has a university degree", 1 "at least one parent has a university degree")	55.82	0-1
Household: siblings	Whether ego has siblings (0 "no siblings", 1 "siblings")	69.12	0-1
Household: both parents	Whether ego lives with both parents (1 "both parents live within the household", 0 otherwise)	76.72	0-1
Parents: study aspirations	Study aspirations of parents for ego (0 "parents expect ego to pursue vocational education", 1 "parents expect ego to pursue university education")	72.92	0-1
Treatment assignment	0 ego was assigned to the control group, 1 ego was assigned to the treatment group	47.74	0-1
Treatment participation	0 ego did not participate in the workshop, 1 ego participated in the workshop	40.86	0-1
Start of information gathering	When ego started gathering information on university majors, 3 categories		
during this schoolyear/not yet		37.53	0-1
during the last school year		45.13	0-1
before upper secondary ducation		17.34	0-1
Wave 2[1]			
Outcome			
% intended field of study overlap	Percentage of field of study overlap between ego and his close friends	0.19 (0.26)	0-1
Wave 3[2]			
Outcome			
% field of study choice overlap	Percentage of field of study overlap between ego and his close friends. If ego and alter both did not know what to study, they are counted as an overlap	0.13 (0.21)	0-1

[1] Observations in wave 1 & wave 2 = 421
[2] Observations in wave 3 = 309

3.4.4 Sample Selection, Balance and Attrition

To preserve power by keeping as many observations as possible while still making the results comparable across waves and outcomes the analyses are conducted with three samples. The first sample (intent sample) consists of students who provided complete information on all relevant wave 1 and wave 2 covariates (N=421) and is used to calculate the treatment effect on intended field of study overlap (intent analysis). The second sample (choice sample) is composed of study participants who provided answers for all relevant covariates in wave 1 and wave 3 and is used to estimate the treatment effect on field of study choice overlap (choice analysis). The main reason why the choice sample is smaller compared to the intent sample is because not every respondent started university right after high school in wave 3. The panel study also gathered data a year after wave 3, in a fourth wave, in which wave 3 nonrespondents were asked to indicate whether they had started studying a year earlier and what their wave 1 friends where studying. The additional data of wave 3 nonrespondents in wave 4 yielded 16 additional responses. As a result, the choice sample consists of N=309 participants. To compare the intent and choice analyses with each other, a third sample was drawn (comparison sample), consisting of respondents who answered every relevant question in all three waves of N=229. With this sample, the treatment effects in intended field of study and chosen field of study can be compared directly to each other.

Since each sample is only a fraction of the original sample due to different patterns of item and unit nonresponse, it is pertinent to assess randomization within each sample and potential asymmetric attrition. To assess the initial randomization of the experiment, a series of OLS-regressions was conducted, where each variable used in the analyses is regressed on an intent to treat indicator (see Table A3.1 in the appendix). As the table reveals, the treatment indicator did not produce a single significant effect, indicating that initial randomization was successful. Another concern of lower observation counts due to attrition and item-nonresponse in these samples is that the loss of observations is not random but systematically related to the treatment, leading to attrition bias. To assess attrition bias, I performed a series of OLS-regressions for each variable used in the analysis and each sample. The outcome of each regression is a pre-treatment covariate while the independent variables are an intent to treat indicator, a sample indicator (e.g., 0 participant is in the intent analysis sample, 1 participant is not in the intent analysis sample) and their interaction term (for a similar approach see Wang et al., 2016). This analysis was done for all variables within all samples (see Tables A3.2-A3.4 in the appendix). Statistically significant interaction terms point towards asymmetric attrition for the specific variable within the sample under scrutiny. No significant interactions ($p < 0.05$) between treatment indicator and sample indicator were found for any sample variable combination. Consequently, no observable nonrandom selection problem was discovered for the upcoming analysis.

3.4.5 Estimation Technique

To estimate the average treatment effect of the information intervention, I use two stage least square instrumental variable regressions (2SLS-IV-Regression) and intent to treat fractional response regression (ITT-FR-Regression). Both approaches can mitigate possible endogenous non-compliance issues (e.g., students assigned to the treatment group who would have greatly benefited from the treatment not attending the counseling workshop) by either using treatment assignment as an instrumental variable for treatment participation (2SLS IV-Regression) or by calculating the intent to treat effect, based on treatment assignment (ITT) (Angrist & Pischke, 2009). Generally speaking, non-compliance is not much of an issue in these samples (e.g., 6.88% noncompliance within the intent sample), which is corroborated by the observation that the ITT-Analyses produce very similar results to the IV-Analyses. FR-Regression is a quasilikelihood estimator used to calculate the conditional mean of fractional response dependent variables (Papke & Wooldridge, 1996). This type of analysis can handle percentages as the dependent variable more adequately than OLS-Regression, since out of bounds predictions (above 1 and below 0) are not possible by design. Within the FR-Regressions, I expect the dependent variable to follow a fractional logistic pattern. Both modelling approaches (2SLS-IV-Regression & ITT-FR-Regression) also include the pre-treatment (wave 1) outcome and multiple other pre-treatment covariates, which are not of substantive concern but increase the efficiency of the estimates (Imbens & Rubin, 2015). In the results section, I present the results of both the 2SLS-IV-Regression as well as the ITT-FR-Regression for the main treatment effects used to test H1 and H2. For the interaction effects, assessing H3, I present the results from the 2SLS-IV-Regression only, since the difference between estimation techniques was marginal to begin with.

3.5 Results

To assess H1 and H2, 2SLS-IV-Regressions (Table 3.2) and ITT-FR-Regressions (Table 3.3) are estimated. Tables 3.2 and 3.3 consist of four models each. The first two models describe the average treatment effect of the information intervention on *intended* field of study overlap within the intent sample (M1) and the comparison sample (M2), whereas the latter two depict the treatment effects on field of study *choice* overlap for the choice sample (M3) and the comparison sample (M4). Starting with the results from the 2SLS-IV-Regression, a significant ($p < 0.05$) treatment effect within the intent sample can be seen in M1. The information intervention decreases the intended field of study overlap between ego and friends by 6.8 percentage points. However, there is no significant effect ($t = 0.51$) of the information intervention on field of study choice within the choice sample (M3). To make comparison more robust between samples and test H2, treatment effects in the comparison sample (M2 and M4) are looked at. M2 reveals that for students who started university right after high school the effect of the information intervention was much larger. Students who are in university at W3 have a decrease in field of study intention overlap of 10.9 percentage points ($p < 0.05$). Like the results in the choice sample, the treatment still did not have a statistically significant effect ($t = 0.59$) on field of study choice overlap within the comparison sample (M4).

Results from the ITT-FR-Regression corroborate these findings. Table 3.3 M1 shows that the average marginal effect of the treatment is significant ($p < 0.05$) and decreases intention homogeneity by 5.71 percentage points, which is slightly less than the 2SLS-IV-Regression estimate. The effect is again higher within the comparison sample M2, with a significant 8.56 percentage point ($p < 0.01$) decrease in intention homogeneity. Models M3 and M4 in Table 3.3 mirror the 2SLS-IV-Regression results and confirm that the treatment did not have substantial or statistically significant impact on field of study choice homogeneity.

In sum, the information intervention successfully decreased intention homogeneity but not choice homogeneity. Thus, both H1 and H2 are partly supported, but instead of finding a weaker effect on choice homogeneity than intention homogeneity, there is no effect on choice homogeneity. The treatment effect on intention homogeneity is marginally strong and consistent with alternative specifications of the dependent variable, although statistical significance and magnitude vary slightly (seen Table A3.6 and A3.7 in the appendix).

Table 3.2. 2SLS IV-Regression results

	M1 % intended field of study overlap (intent sample)	*M2* % intended field of study overlap (comparison sample)	*M3* % field of study choice overlap (choice sample)	*M4* % field of study choice overlap (comparison sample)
Treatment	**-0.0680****	**-0.109***	**0.0171**	**0.0203**
	(-2.29)	**(-2.59)**	**(0.51)**	**(0.59)**
Pre-treatment outcome	0.320***	0.342***	0.0406	0.0392
	(5.83)	(4.41)	(0.86)	(0.75)
Age	0.000267	-0.000211	-0.0000188	-0.000758
	(0.28)	(-0.21)	(-0.02)	(-1.00)
School grades	0.000583	0.0432	0.0112	0.00651
	(0.03)	(1.50)	(0.61)	(0.30)
Schoolmates: study aspiration	-0.0319*	-0.0383	0.0167	0.0235
	(-2.31)	(-1.77)	(1.23)	(1.68)
Gender	0.0238	0.0195	0.0128	-0.00426
	(0.79)	(0.49)	(0.51)	(-0.15)
Parents: university degree	0.0145	0.0541	0.0153	-0.0139
	(0.55)	(1.46)	(0.55)	(-0.44)
Household: siblings	0.00262	-0.0364	-0.0287	-0.0401
	(0.09)	(-0.95)	(-1.03)	(-1.27)
Household: both parents	-0.0268	-0.0570	0.0126	0.0347
	(-0.88)	(-1.38)	(0.36)	(0.98)
Parents: study aspiration	0.0915***	0.0729*	-0.00886	0.0188
	(3.61)	(2.07)	(-0.29)	(0.60)
Start of information gathering: (ref) during this school year/not yet				
during the last school year	-0.0155	-0.0160	-0.0189	-0.0331
	(-0.54)	(-0.41)	(-0.68)	(-1.13)
before upper secondary education	-0.0330	-0.0445	-0.0403	-0.0327
	(-0.89)	(-0.85)	(-1.17)	(-0.83)
Constant	0.147	0.262	0.0478	0.193
	(0.63)	(0.97)	(0.18)	(1.02)
N	421	229	309	229

t statistics in parentheses, robust standard errors
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.3. ITT- FR-Regression results

	M1 % intended field of study overlap (intent sample)	*M2* % intended field of study overlap (comparison sample)	*M3* % field of study choice overlap (choice sample)	*M4* % field of study choice overlap (comparison sample)
Treatment	**-0.0571***	**-0.0856****	**0.0130**	**0.0148**
	(-2.36)	**(-2.63)**	**(0.52)**	**(0.54)**
N	421	229	309	229

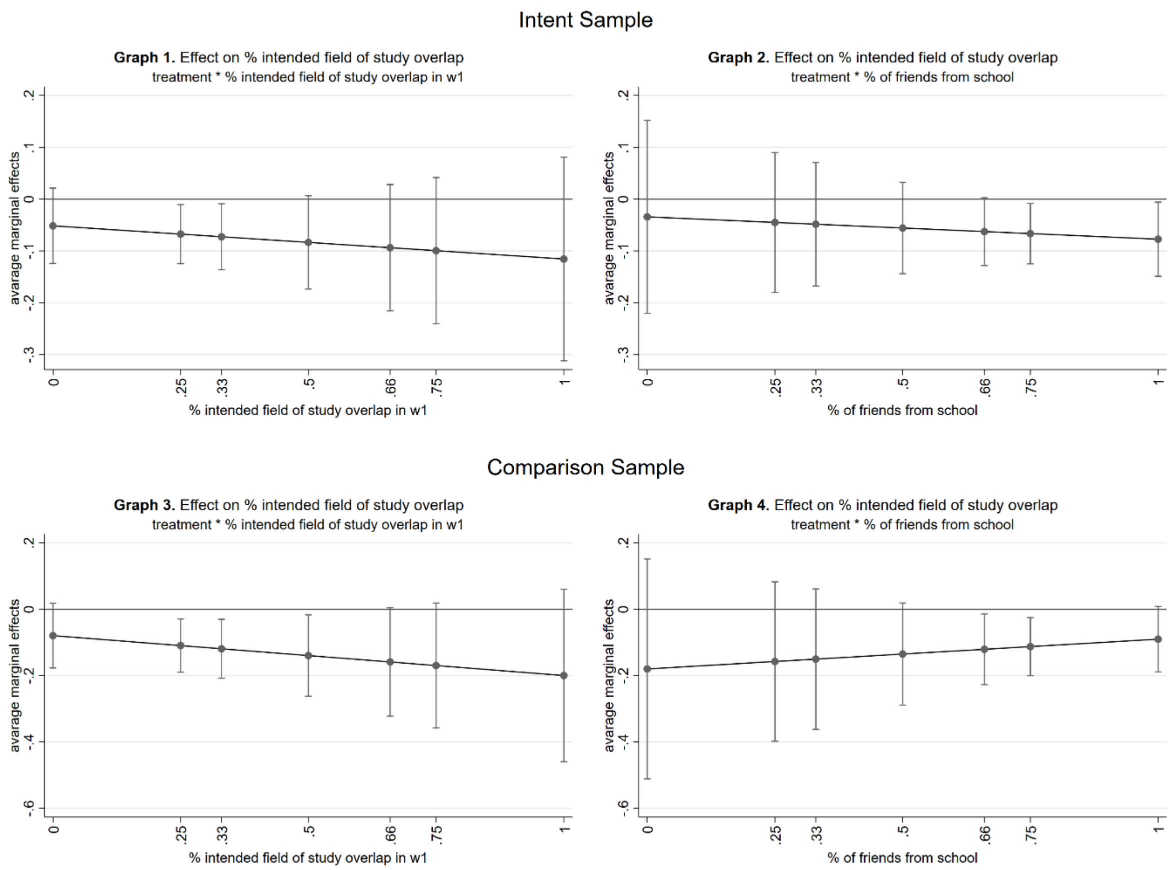
z statistics in parentheses, coefficients are average marginal effects, full model in Table A3.5 (appendix)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

To test H3, whether the treatment effect gets stronger as the degree of pre-treatment network homogeneity rises, an interaction term between treatment and two network homogeneity measures (% field of study overlap in w1 and % of friends from school) was introduced into the 2SLS-IV-Regression model (see Table A3.8 and A3.9 in the appendix). I focus on the interaction within for the *intent analysis* because the main treatment effect was only substantial and statistically significant for intended and not the chosen field of study homogeneity. Figure 3.1 portrays the average marginal effects and their 95% confidence intervals for the intent analysis within the intent sample (Graph 1&2) and the comparison sample (Graph 3&4), for % intended field of study overlap in w1 (Graph 1&3) and % of friends ego knows from school (Graph 2&4).

Graph 1 reveals that as the percentage of overlap in wave 1 increases, the treatment effect becomes stronger. The average marginal effect on intention homogeneity for egos who had no overlap between their intended field of study and their alters is -5.15 percentage points, which rises to -8.35 percentage points when students have a pre-treatment intended field of study homogeneity of 50%. In other words, with each percentage point increase in pre-treatment homogeneity the treatment effect increases by 0.064 percentage points. While the substantial direction of the interaction is in line with H3, the interaction coefficient itself is not statistically significant. Within the comparison sample (Graph 3), the interaction coefficient is larger. The negative treatment effect decreases further in the comparison sample by 0.12 percentage point for each additional percentage point more in pre-treatment intended field of study overlap, although the interaction is still not statistically significant. Similar to Graph 1, the treatment effect in Graph 2 also becomes stronger as the percentage of friends ego knows from school increases. However, within the comparison sample (Graph 4) the direction is reversed and both interactions within the intent and comparison sample do not reach statistical significance. The interactions for the choice analysis mirror the patterns of the main effect analysis in that they are substantially weak and statistically insignificant.

Overall, the results generally do not, or only weakly, support H3. The direction of the interaction effect between pre-treatment field of study overlap and the treatment, while in line with expectations was statistically insignificant. While the interaction between percentage of friends ego knows from school and treatment was inconsistent and statistically insignificant.

Figure 3.1. 2SLS-IV-Regression interaction effects for the intent analysis



3.6 Discussion

There are some points that warrant further consideration, which I will elaborate on in this section. Given the positively skewed distribution of the outcomes, the main effect of the information treatment may be subject to ceiling effects since there is little room for an increase in heterogeneity. The effect may be more pronounced in less heterogenous networks. A representative sample of all university eligible high-school students in Germany is likely to show a different field of study homogeneity distribution than the self-selected sample of this study, which is more representative of students who participate in voluntary university guidance workshops. Students who participated in this study are likely more independent, motivated and already more engaged in gathering information on fields of studies than a representative sample of university eligible high-school students. Consequently, it is plausible that the treatment effect found in this sample depicts a lower bound, with the treatment effect potentially being stronger in a representative sample.

Another important consideration is how the treatment effect of the intent analysis came to be. For example, the reduction in overlap may be due to a reshuffling of the friend network as a response to the treatment. For the choice analysis, students have to provide information on their friend network generated in wave 1, before the treatment, which is why any effect, or the lack thereof, cannot be due to the selection of new friends. Within the intent analysis it is theoretically possible that students changed their network because of the information treatment since the egocentric network was generated anew in wave 2. After attending the information intervention, students could have sought out new close friends that share their newly realised interests or ambitions. This scenario is unlikely because close friend networks are formed over a long period of time and rather stable (Crosnoe, 2000). To substantiate this claim, I have empirically tested for network change due to the treatment. The results show that there is no significant treatment effect on network change (see Table A3.10 M3 in the appendix), i.e., workshop participants are not more likely to experience a change in their composition of friends. Furthermore, when repeating the intent analysis with only alters mentioned in both wave 1 and 2, the effect size and significance of the treatment slightly decreases (see Table A3.10 M1 and M2 in the appendix), but the results are overall still consistent with the ones presented here.

Another potential explanation for a reduction in overlap between ego and alters is that egos change their field of study preference as a reaction to the treatment, while the field of study preferences of alters stay the same. The accuracy of this explanation may be determined by testing whether there is a treatment effect on egos changing their preferred field of study. Supplementary analysis show (Table A3.10 M4 in the appendix) that there is a positive but statistically insignificant treatment effect on students changing their preferred field of study. Consequently, this reason alone is unlikely to explain the treatment effect.

A change in overlap may also occur when alters change their field of study intentions in response to the treatment. For example, if egos share what they have learned with their close friends (functioning as an information conduit), those friends will receive an information influx from a trusted source and will also be more inclined to start their information search process as well, potentially altering their intended field of study. Considering that students (egos) participating in the study had to self-register for it, they had to already have started with their information gathering process, which could explain why a treatment effect on changes in field of study preference for ego could not be observed. Egos' friends on the other hand, may be at the start of their information gathering process and subsequently more malleable. Analysis (Table A3.10 M5 in the appendix) reveals that the treatment increased the percentage of alters who changed their preferred field of study by 5.02 percentage points, although the effect is not significant ($t = 1.63$).

In the end, a single definitive factor that explains the treatment effect on changes in overlap for the intent analysis was not found. However, the change in overlap may also be the result of a combination of the aforementioned factors, which jointly lead to the observed treatment effect. These supplementary analyses also highlight an important future research avenue that should concentrate on information diffusion to find out through which channels information can reduce network homogeneity.

3.7 Conclusion

This study focused on educational field of study choice and intention homogeneity of students and their closest friends. In particular, it assessed how this type of homogeneity can be reduced when students are provided with novel information. The main argument is that an important reason for observed homogeneity within close friend networks is information redundancy and the accompanying echo chamber effect, which could be reduced when students are exposed to new information (H1). The results, utilizing an experimental design where ego is randomly chosen to receive information through a counseling intervention or not, show that the treatment decreased the overlap in field of study intentions between egos and their close friends. This finding suggests that information does indeed partly drive educational homogeneity as the latter decreases when external sources of information are provided. I also suggested that the effects of the information intervention could be stronger for field of study intention homogeneity than for choice homogeneity (H2) because every student might eventually gather enough relevant information prior to making a field of study choice, which is why, in the long run, the information intervention will be less effective. The results showed that, in the long run, the information intervention is not only less effective but ceases to be effective. Whether this pattern is attributable to natural information saturation prior to making a field of study choice, as suggested in the lead up to H2, requires additional empirical research. Finally, this study assessed whether the degree of homogeneity within the network moderates the impact of the treatment (H3). The analyses showed that in ego-networks marked by high similarity between ego and alters regarding their pre-treatment intended field of study, the average treatment effect was larger, although the interaction itself was statistically insignificant, while the treatment effect for egos who had many friends outside of school was inconsistent and statistically insignificant across samples. Therefore, I did not find convincing evidence for H3. In the end, a major takeaway from this study is that exogenous information can change field of study intention homogeneity, consequently substantiating the claim that some types of homogeneity are spurred in part by the lack of information or by information redundancy (Burt, 2005; Granovetter, 1973).

In contrast to previous studies about network effects on educational outcomes utilizing (quasi) experimental designs (Lyle, 2007; Sacerdote, 2001; Stinebrickner & Stinebrickner, 2006; Zimmerman, 2003), this study did not randomly vary network composition or take advantage of naturally occurring random network composition. Instead, what was randomly varied was the information treated students received to see if that changes study program overlap between them and their close friends. The advantage of this approach is the comparative ease of acquiring data on the network of interest through a name generator and the straightforwardness of the experimental design to evaluate the effect of an information intervention on field of study overlap. The sample of the experiment consisted of students who usually attend counseling workshops offered by universities. It mirrors the real-life effectiveness of these types of information intervention closely, although it is not representative of an information intervention that would target all German students close to graduating from high school. This circumstance does not impede the internal validity of the experiment, but it does limit its generalizability. Future research should also scrutinize the way through which homogeneity is reduced through novel information. Within the discussion I inspected multiple potential explanations for the observed short-term reduction in homogeneity, focusing on different mechanisms of information utilization and dissemination. The supplementary analyses did not reveal a single driving factor for the short-term reduction in homogeneity, which is why more research on this topic is needed.

Homogeneity remains a universally present phenomenon permeating through many facets of social life. Theory driven empirical testing fosters the understanding of the phenomenon and may help mitigate the potentially negative consequences excessive amounts of homogeneity harbor. Information is one essential mechanism, but there are others, such as normative influence and selection, that are equally important. The extent to which a mechanism dominates homogeneity formation is likely dependent on the specific social setting. For educational decisions, I believe that information plays an important role, if the actor makes conscious rational decisions. In different circumstances, such as smoking behavior (Mercken et al., 2010) or other risky behaviors like delinquency (McMillan et al., 2018), the relevance of information may take a back seat to other mechanisms, such as normative pressure towards conformity (Coleman, 1988).

3.8 Appendix

Table A3.1. Series of regressions to assess randomization

	% Field of study overlap (in w1)	Gender	Parents: university degree	Household: siblings	Household: both parents
Intent to treat	0.0120	0.000360	0.0166	0.0279	-0.0283
	(0.55)	(0.01)	(0.44)	(0.80)	(-0.87)
Constant	0.231***	0.749***	0.564***	0.680***	0.773***
	(15.94)	(35.42)	(23.29)	(29.81)	(37.75)
N	617	725	723	717	717
R^2	0.000	0.000	0.000	0.001	0.001

Table A3.1. (continued)

	Parents: study aspiration	Start of information gathering	Age	School grades	Schoolmates: study aspiration
Intent to treat	-0.00565	-0.0192	-0.919	-0.0617	-0.0125
	(-0.17)	(-0.35)	(-0.83)	(-1.20)	(-0.17)
Constant	0.725***	1.791***	213.7***	2.266***	3.831***
	(33.32)	(50.71)	(255.38)	(66.00)	(84.10)
N	725	725	723	717	723
R^2	0.000	0.000	0.001	0.002	0.000

OLS-Regression with robust standard errors, *t* statistics in parentheses, robust standard errors
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3.2. Test for asymmetric attrition in the intent sample

	% field of study overlap (in w1)	% field of study overlap (in w2)	Gender	Parents: university degree	Household: siblings	Household: both parents
Intent to treat	0.00506	-0.0481	-0.00656	0.0221	0.0387	-0.0115
	(0.19)	(-1.90)	(-0.16)	(0.46)	(0.86)	(-0.28)
Intent sample	-0.00677	0.0242	-0.0500	0.0300	0.0157	0.00114
	(-0.23)	(0.52)	(-1.18)	(0.62)	(0.34)	(0.03)
Intent to treat * intent sample	0.0245	0.114	-0.000528	-0.00410	-0.0261	-0.0510
	(0.51)	(1.07)	(-0.01)	(-0.05)	(-0.36)	(-0.74)
Constant	0.233***	0.215***	0.773***	0.550***	0.673***	0.773***
	(12.78)	(11.51)	(27.27)	(16.35)	(21.21)	(27.27)
N	617	475	725	723	717	717
R^2	0.001	0.015	0.003	0.001	0.001	0.002

Table A3.2. (continued)

	Parents: study aspiration	Start of information gathering	Age	School grades	Schoolmates: study aspiration
Intent to treat	0.0231	-0.0516	-0.900	-0.0419	-0.0626
	(0.53)	(-0.74)	(-0.74)	(-0.62)	(-0.70)
Intent sample	0.0145	-0.0653	1.461	0.127	-0.0686
	(0.33)	(-0.92)	(0.86)	(1.86)	(-0.75)
Intent to treat * intent sample	-0.0793	0.0687	0.547	-0.00645	0.121
	(-1.12)	(0.60)	(0.23)	(-0.06)	(0.81)
Constant	0.718***	1.823***	213.0***	2.206***	3.864***
	(23.61)	(38.11)	(228.72)	(45.55)	(63.91)
N	725	725	723	717	723
R^2	0.002	0.001	0.004	0.010	0.001

OLS-Regression with robust standard errors, *t* statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3.3. Test for asymmetric attrition in the choice sample

	% field of study overlap (in w1)	Gender	Parents: university degree	Household: siblings	Household: both parents
Intent to treat	0.0311	0.0288	-0.0158	-0.0130	-0.0561
	(1.07)	(0.57)	(-0.28)	(-0.24)	(-1.18)
Choice sample	0.0711*	0.0453	-0.0688	0.0123	-0.0541
	(2.49)	(1.03)	(-1.39)	(0.26)	(-1.32)
Intent to treat * choice sample	-0.0317	-0.0469	0.0509	0.0812	0.0444
	(-0.72)	(-0.70)	(0.68)	(1.15)	(0.68)
Constant	0.193***	0.721***	0.606***	0.673***	0.806***
	(10.15)	(20.60)	(15.89)	(18.37)	(26.11)
N	617	725	723	717	717
R^2	0.013	0.002	0.003	0.005	0.003

Table A3.3. (continued)

	Parents: study aspiration	Start of information gathering	Age	School grades	Schoolmates: study aspiration
Intent to treat	0.0193	-0.0828	-0.0640	-0.0465	-0.103
	(0.40)	(-1.02)	(-0.04)	(-0.66)	(-0.96)
Choice sample	-0.0433	-0.114	-0.0770	0.233***	-0.0588
	(-0.98)	(-1.58)	(-0.05)	(3.54)	(-0.64)
Intent to treat * choice sample	-0.0545	0.103	-1.642	0.00752	0.163
	(-0.81)	(0.94)	(-0.74)	(0.07)	(1.14)
Constant	0.752***	1.861***	213.8***	2.125***	3.867***
	(22.28)	(33.44)	(170.19)	(46.42)	(55.57)
N	725	725	723	717	723
R^2	0.006	0.004	0.002	0.031	0.002

OLS-Regression with robust standard errors, *t* statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3.4. Test for asymmetric attrition in the comparison sample

	% field of study overlap (in w1)	Gender	Parents: university degree	Household: siblings	Household: both parents
Intent to treat	0.0207	0.0284	0.00366	-0.0144	-0.0680
	(0.61)	(0.50)	(0.06)	(-0.23)	(-1.25)
Intent sample	0.0454	0.0164	-0.0201	0.0306	-0.0584
	(1.53)	(0.34)	(-0.37)	(0.59)	(-1.33)
Intent to treat * comparison sample	-0.00725	-0.0423	0.0173	0.0745	0.0538
	(-0.16)	(-0.60)	(0.22)	(0.98)	(0.79)
Constant	0.200***	0.737***	0.579***	0.658***	0.816***
	(8.51)	(17.82)	(12.49)	(14.77)	(22.41)
N	617	725	723	717	717
R^2	0.006	0.001	0.000	0.007	0.003

Table A3.4. (continued)

	Parents: study aspiration	Start of information gathering	Age	School grades	Schoolmates: study aspiration
Intent to treat	0.0283	-0.0596	-0.621	0.00796	-0.103
	(0.50)	(-0.64)	(-0.33)	(0.10)	(-0.88)
Intent sample	-0.0281	-0.0934	0.426	0.250***	-0.0394
	(-0.58)	(-1.20)	(0.23)	(3.65)	(-0.41)
Intent to treat * comparison sample	-0.0597	0.0487	-0.407	-0.0691	0.139
	(-0.84)	(0.42)	(-0.17)	(-0.66)	(0.94)
Constant	0.746***	1.860***	213.4***	2.085***	3.860***
	(18.23)	(28.31)	(138.51)	(38.58)	(49.76)
N	725	725	723	717	723
R^2	0.004	0.002	0.001	0.024	0.001

OLS-Regression with robust standard errors, *t* statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3.5. ITT-FR-Regression results

	M1 % intended field of study overlap (intent sample)	*M2* % intended field of study overlap (comparison sample)	*M3* % field of study choice overlap (choice sample)	*M4* % field of study choice overlap (comparison sample)
Treatment	0.675*	0.566**	1.120	1.138
	(-2.37)	(-2.64)	(0.52)	(0.54)
Pre-treatment outcome	7.048***	6.889***	1.421	1.403
	(6.10)	(4.61)	(0.95)	(0.87)
Age	1.002	0.999	1.000	0.992
	(0.30)	(-0.11)	(-0.04)	(-0.84)
School grades	0.984	1.335	1.107	1.055
	(-0.13)	(1.43)	(0.67)	(0.29)
Schoolmates: study aspiration	0.798*	0.787	1.158	1.207
	(-2.46)	(-1.77)	(1.17)	(1.51)
Gender	1.186	1.110	1.133	0.961
	(0.83)	(0.42)	(0.57)	(-0.17)
Parents: university degree	1.108	1.435	1.157	0.900
	(0.56)	(1.45)	(0.61)	(-0.40)
Household: siblings	1.025	0.784	0.781	0.724
	(0.13)	(-1.02)	(-1.07)	(-1.25)
Household: both parents	0.802	0.670	1.131	1.414
	(-1.09)	(-1.58)	(0.38)	(0.97)
Parents: study aspiration	1.942***	1.626	0.918	1.173
	(3.42)	(1.87)	(-0.33)	(0.56)
Start of information gathering: (ref) during this school year/not yet				
during the last school year	0.862	0.854	0.869	0.773
	(-0.78)	(-0.63)	(-0.60)	(-1.05)
before upper secondary education	0.794	0.734	0.696	0.777
	(-0.87)	(-0.88)	(-1.11)	(-0.74)
N	421	229	309	229

z statistics in parentheses, robust standard errors, coefficients are odds ratios

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3.6. 2SLS-IV-Regression: alternative specifications of intended field of study overlap

	M1[1]	*M2*[1]	*M3*[2]	*M4*[2]
	% intended field of study overlap (intent sample)	% intended field of study overlap (comparison sample)	% intended field of study overlap (intent sample)	% intended field of study overlap (comparison sample)
Treatment	-0.0406	-0.0670	-0.0713*	-0.0997*
	(-1.65)	(-1.91)	(-2.05)	(-2.05)
Pre-Treatment Outcome	0.359***	0.379*	0.502***	0.522***
	(4.51)	(3.33)	(5.85)	(4.27)
Age	0.00116	0.000508	0.000822	-0.000101
	(1.44)	(0.57)	(0.91)	(-0.11)
School grades	-0.000677	0.0196	0.00870	0.0314
	(-0.05)	(0.89)	(0.45)	(1.03)
Schoolmates: study aspiration	-0.0206	-0.00964	-0.0225	-0.0237
	(-1.95)	(-0.63)	(-1.58)	(-1.18)
Gender	0.0295	0.0271	0.0483	0.0771
	(1.21)	(0.81)	(1.48)	(1.90)
Parents: university degree	0.0208	0.0296	0.0317	0.0341
	(0.98)	(1.01)	(1.10)	(0.87)
Household: siblings	-0.0173	-0.0546	-0.0301	-0.0574
	(-0.76)	(-1.65)	(-1.00)	(-1.40)
Household: both parents	-0.00923	-0.0360	0.00735	-0.0322
	(-0.34)	(-0.91)	(0.21)	(-0.70)
Parents: study aspiration	0.0355	0.00937	0.0672*	0.0743
	(1.66)	(0.31)	(2.39)	(1.97)
Start of information gathering: (ref) during this school year/not yet				
during the last school year	0.0345	0.0397	0.0288	0.0173
	(1.58)	(1.38)	(0.96)	(0.45)
before upper secondary education	0.0368	0.0562	0.0343	0.0336
	(1.22)	(1.29)	(0.88)	(0.62)
Constant	-0.125	0.0101	-0.118	0.0750
	(-0.68)	(0.05)	(-0.56)	(0.31)
N	421	229	240	137

t statistics in parentheses, robust standard errors
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
[1]If ego and alter both don’t know what to study, it is not counted as an overlap between ego and alter
[2] egos who did not know what they wanted to study in wave 1 or wave 2 were dropped from the analysis samples

Table A3.7. ITT-FR-Regression: alternative specifications of intended field of study overlap

	M1[1]	*M2*[1]	*M3*[2]	*M4*[2]
	% intended field of study overlap (intent sample)	% intended field of study overlap (comparison sample)	% intended field of study overlap (intent sample)	% intended field of study overlap (comparison sample)
Treatment	-0.0333	-0.0552*	-0.0571*	-0.0733*
	(-1.67)	(-2.04)	(-2.19)	(-2.18)
N	421	229	240	137

z statistics in parentheses, coefficients are average marginal effects,
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
[1]"don't know's" are not counted as overlap between ego and alter
[2] "don't know's" of egos are dropped from the analysis samples

Table A3.8. 2SLS-IV-Regression: treatment * % intended field of study overlap in w1

	M1 % intended field of study overlap (intent sample)	*M2* % intended field of study overlap (comparison sample)	*M3* % field of study choice overlap (choice sample)	*M4* % field of study choice overlap (comparison sample)
Treatment	-0.0515	-0.0795	0.00876	0.00227
	(-1.39)	(-1.60)	(0.19)	(0.05)
% intended field of study overlap in w1	0.347***	0.399***	0.0247	0.00350
	(4.53)	(4.07)	(0.36)	(0.05)
Treatment * % intended field of study overlap in w1	-0.0640	-0.120	0.0352	0.0747
	(-0.53)	(-0.77)	(0.31)	(0.64)
Age	0.000262	-0.000171	-0.0000246	-0.000783
	(0.29)	(-0.18)	(-0.02)	(-1.06)
School grades	0.000355	0.0438	0.0111	0.00615
	(0.02)	(1.57)	(0.62)	(0.29)
Schoolmates: study aspiration	-0.0325*	-0.0380	0.0167	0.0234
	(-2.38)	(-1.81)	(1.25)	(1.72)
Gender	0.0246	0.0189	0.0128	-0.00388
	(0.83)	(0.49)	(0.52)	(-0.14)
Parents: university degree	0.0152	0.0543	0.0156	-0.0140
	(0.59)	(1.53)	(0.57)	(-0.46)
Household: siblings	0.00245	-0.0398	-0.0283	-0.0380
	(0.09)	(-1.06)	(-1.04)	(-1.24)
Household: both parents	-0.0256	-0.0530	0.0118	0.0322
	(-0.87)	(-1.36)	(0.34)	(0.94)
Parents: study aspiration	0.0922***	0.0722*	-0.00890	0.0192
	(3.73)	(2.15)	(-0.29)	(0.63)
Start of information gathering: (ref) during this school year/not yet				
during the last school year	-0.0175	-0.0170	-0.0181	-0.0325
	(-0.62)	(-0.45)	(-0.67)	(-1.15)
before upper secondary education	-0.0326	-0.0389	-0.0415	-0.0363
	(-0.90)	(-0.77)	(-1.21)	(-0.94)
Constant	0.143	0.239	0.0526	0.208
	(0.63)	(0.91)	(0.20)	(1.11)
N	421	229	309	229

t statistics in parentheses, robust standard errors
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3.9. 2SLS-IV-Regression: treatment * % friends from school

	M1 % intended field of study overlap (intent sample)	*M2* % intended field of study overlap (comparison sample)	*M3* % field of study choice overlap (choice sample)	*M4* % field of study choice overlap (comparison sample)
Treatment	-0.0342	-0.180	-0.0344	0.0387
	(-0.36)	(-1.06)	(-0.27)	(0.27)
% of friends from school	0.0193	-0.0406	0.0375	0.0444
	(0.27)	(-0.31)	(0.40)	(0.37)
Treatment * % of friends from school	-0.0431	0.0898	0.0692	-0.0207
	(-0.38)	(0.46)	(0.45)	(-0.12)
Pre-treatment outcome	0.320^{***}	0.341^{***}	0.0415	0.0391
	(5.94)	(4.52)	(0.90)	(0.77)
Age	0.000262	-0.000202	-0.0000701	-0.000794
	(0.29)	(-0.21)	(-0.07)	(-1.09)
School grades	0.00136	0.0406	0.00920	0.00651
	(0.08)	(1.36)	(0.50)	(0.30)
Schoolmates: study aspiration	-0.0322^{*}	-0.0374	0.0162	0.0225
	(-2.36)	(-1.74)	(1.23)	(1.63)
Gender	0.0246	0.0170	0.0114	-0.00319
	(0.83)	(0.43)	(0.46)	(-0.11)
Parents: university degree	0.0149	0.0533	0.0135	-0.0130
	(0.57)	(1.47)	(0.50)	(-0.41)
Household: siblings	0.00407	-0.0383	-0.0336	-0.0412
	(0.14)	(-1.00)	(-1.22)	(-1.34)
Household: both parents	-0.0276	-0.0569	0.0117	0.0337
	(-0.92)	(-1.41)	(0.33)	(0.97)
Parents: study aspiration	0.0910^{***}	0.0747^{*}	-0.0105	0.0158
	(3.62)	(2.18)	(-0.34)	(0.49)
Start of information gathering: (ref) during this school year/not yet				
during the last school year	-0.0156	-0.0173	-0.0187	-0.0328
	(-0.55)	(-0.45)	(-0.68)	(-1.11)
before upper secondary education	-0.0336	-0.0439	-0.0368	-0.0310
	(-0.92)	(-0.86)	(-1.09)	(-0.80)
Constant	0.132	0.298	0.0415	0.171
	(0.55)	(0.99)	(0.15)	(0.78)
N	421	229	309	229

t statistics in parentheses, robust standard errors
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3.10. 2SLS-IV-Regression results for supplementary analysis

	M1[1] % intended field of study overlap (intent sample)	*M2*[1] % intended field of study overlap (comparison sample)	*M3*[2] Network Change (intent sample)	*M4*[3] Changes in preferred subject of ego (intent sample)	*M5*[4] % of alters who changed their preferred subject (intent sample)
Treatment	-0.0401	-0.0590	-0.0681	0.0137	0.0502
	(-1.20)	(-1.27)	(-0.30)	(0.23)	(1.63)
Pre-treatment outcome	0.354***	0.416***			
	(6.08)	(5.03)			
Age	0.000463	-0.000506	-0.00294	-0.00151	-0.000315
	(0.43)	(-0.43)	(-0.35)	(-0.78)	(-0.30)
School grades	-0.0204	0.0272	0.130	0.0473	0.00207
	(-1.04)	(0.87)	(0.91)	(1.30)	(0.11)
Schoolmates: study aspiration	-0.0153	-0.0424	0.164	0.0158	-0.0262
	(-1.00)	(-1.80)	(1.63)	(0.59)	(-1.92)
Gender	0.0354	0.0641	-0.314	0.0326	0.0236
	(1.06)	(1.48)	(-1.35)	(0.56)	(0.81)
Parents: university degree	0.00696	0.0221	0.423*	-0.0362	-0.0215
	(0.23)	(0.52)	(2.21)	(-0.71)	(-0.82)
Household: siblings	0.0124	-0.0142	0.255	-0.00469	-0.00156
	(0.40)	(-0.34)	(1.31)	(-0.09)	(-0.06)
Household: both parents	-0.0327	-0.0405	-0.483*	-0.0107	0.0195
	(-0.93)	(-0.88)	(-2.17)	(-0.18)	(0.70)
Parents: study aspiration	0.0742*	0.0675	-0.224	-0.125*	0.0215
	(2.52)	(1.64)	(-1.05)	(-2.22)	(0.75)
Start of information gathering: (ref) during this school year/not yet					
during the last school year	0.00397	-0.000355	0.0114	-0.0356	-0.00194
	(0.12)	(-0.01)	(0.06)	(-0.65)	(-0.07)
before upper secondary education	-0.0548	-0.0459	0.135	-0.0114	-0.0155
	(-1.35)	(-0.83)	(0.49)	(-0.16)	(-0.40)
Constant	0.0605	0.285	2.595	0.711	0.358
	(0.23)	(0.92)	(1.34)	(1.51)	(1.38)
N	390	214	421	421	421

t statistics in parentheses, robust standard errors

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[1]Only friends who were mentioned in both w1 and w2 are used to calculate the outcome.

[2]Network change is the outcome, defined as the number of changes within the network structure between w1 and w2. Changes constitute (1) dropping w1 friends in w2 and (2) adding w2 friends who were not mentioned in w1.

[3]The outcome is whether ego changed his preferred subject between w1 and w2 (0 = no change in preferred subject, 1 = changed preferred subject)

[4]Percentage of ego's alters who changed their subject according to ego between w1 and w2

Chapter 4: “The Relevance of Social and Academic Integration for Students’ Dropout Decisions. Evidence from a Factorial Survey in Germany.”

4.1 Abstract

Dropout rates from higher education programs are high and constitute a problem for both the individual and society as a whole. To effectively develop measures to combat dropout, the reasons why students drop out of higher education need to be understood. Building on Tinto’s integration model, this paper tests the extent to which students’ social and academic integration leads to higher dropout intentions and whether the effect differs by students’ academic family background. A sample of German students in their first year of university studies were presented hypothetical scenarios with varying degrees of academic and social integration (vignettes) and they evaluated the likelihood of dropping out under the described conditions. This factorial survey design improves upon previous studies that were unable to separate the ambiguous causal ordering of the relationship. Subsequent multilevel analyses corroborate Tinto's integration model by revealing that all subdimensions of academic and social integration predict dropout intention and indicate that not all subdimensions are equally important. Cross level interactions unveil that the effect of academic and social integration largely does not depend on students’ academic family background.

Keywords: Dropout, Tinto, Germany, Higher Education, Factorial Survey

Author Contributions:

Janina Beckman was my co-author for this paper. Both authors conceptualized the factorial survey design and developed the theoretical framework. I implemented and analyzed the factorial survey. Both authors contributed equally to writing, reviewing, and editing the paper.

4.2 Introduction

Student dropout in higher education constitutes a significant and growing problem across many countries (Heublein, 2014; Vossensteyn et al.). In Germany, where this study is conducted, on average one out of four students who start a bachelor program (27%) drop out prematurely. In some bachelor programs, such as mathematics and natural sciences, the dropout rate is as high as 43% (Heublein et al., 2020). Dropout is problematic because it is associated with negative consequences for both the individual, e.g. labor market disadvantages (Neugebauer et al., 2019) or psychological burdens (Faas et al., 2018), and society, e.g. skills shortage or financial costs at the level of institutions (Heublein & Wolter, 2011). By thoroughly understanding under which conditions students decide to leave their initially chosen study program, institutional conditions that reduce dropout can be developed and improved, benefiting both students and society.

Previous research acknowledges that student dropout needs to be approached as an interplay of individual predispositions and institutional conditions (Heublein, 2014; Tinto, 1975). Tinto's (1975) classic integration model is one major theoretical approach to understanding dropout in higher education. He explains students' dropout decision as the result of students interacting with the higher education institution. Accordingly, students who integrate in their higher education environment both academically and socially, are more likely to complete their studies. Extensive empirical tests of this model have been conducted internationally, e.g., in the U.S. (Bers & Smith, 1991; Ishitani, 2016, for an overview see Pascarella & Terenzini, 2005) and – to a slightly lesser extent – in Europe (e.g. Chrysikos et al., 2017; Nevill & Rhodes, 2004). These studies generally confirm that both social integration and academic integration are associated with students' dropout intentions.

While these studies provide important insights about the association between students' integration and their pathway through higher education, they are limited in two regards which we attempt to address with this study. First, their purely observational research designs inhibit the identification of the causal ordering of integration and dropout. Is it *because* of failed social and academic integration that students decide to leave their higher education program? Or do students with a higher propensity for dropping out show less motivation and effort to integrate from the beginning, which would point to *reversed causal order*? Observational designs cannot separate these causal mechanisms adequately. We utilize a factorial survey design to experimentally test the predictions of Tinto's model and overcome the methodological limitations outlined above. This design allows us to describe differences in students' decision-making process at given levels of integration with a clear ordering of cause and effect. This is achieved by presenting respondents with different hypothetical scenarios, randomly varying their degree of academic and social integration and asking them about their intention to quit their study program under the described conditions.

As the second main contribution, our study theorizes and tests for social differences in students' dropout decisions. Since dropout in higher education is stratified along students' academic family background, examining whether students' decision-making process is socially stratified adds to a more comprehensive understanding of how these differences emerge (Contini et al., 2018; Herbaut, 2020). Specifically, we assess whether students from academic and non-academic family backgrounds show different dropout risks at the same level of achieved integration. This expectation is in line with the "compensatory advantage" mechanism which predicts that students from academic family backgrounds are sheltered from negative educational experiences (Bernardi, 2014). Our analysis thus sheds light on whether under the same conditions, some students choose to drop out, while others would persist.

4.3 Theoretical Framework and Previous Empirical Evidence

In this section, we first review Tinto's (1975) student dropout model and the related empirical dropout literature to derive testable hypotheses from them. Afterwards, we lay out common limitations of these studies and outline how our factorial research design addresses them. Finally, we develop hypotheses pertaining to the moderating effect of academic family background.

4.3.1 Student Dropout and Students' Social and Academic Integration

Tinto (1975) describes student dropout as the result of a "longitudinal process of interactions between the individual and the academic and social systems of the college" (p. 94). In his student integration model, he asserts that, beyond individual pre-entry and background characteristics, students' experiences with the higher education institution are central predictors of their dropout decision. He distinguishes between students' *social* and *academic* integration, suggesting that students who are not sufficiently integrated into the social and academic system of their university will choose to quit their educational pathway. Tinto distinguishes between two sub-dimensions of social integration: students' relationship with the faculty and students' interaction with fellow students. A high degree of social integration is conditional upon the quality and the quantity of these relationships. Faculty members provide students with educational and learning contexts that are supportive and create meaningful academic experiences, resulting in higher attachment and reduced dropout (Pascarella & Terenzini, 1977). Previous studies have found a positive association between (formal and informal) teacher-student relationships and students' academic motivation, achievement and persistence (Umbach & Wawrzynski, 2005; Walsh et al., 2009). In their literature review, Pascarella & Terenzini (2005) conclude that more favorable contact between students and faculty enhances students' academic development and educational attainment. We thus expect that quantity and quality of faculty support affects students' intentions to drop out:

(H1): A low degree of faculty support increases dropout intention.

Within the social system of higher education, fellow students provide opportunities of informal academic collaboration and sources of emotional and social support. Friendships guide students in navigating the transition into their institution and enhance students' sense of belonging (Meeuwisse et al., 2010). A lack of social contact is cited as one of the most important reasons for dropout in Germany (Isleib et al., 2019). The second prediction derived from Tinto's model is thus:

(H2) A low degree of social interaction with fellow students increases dropout intentions.

Regarding students' academic integration, Tinto again distinguishes between two sub-dimensions: students' academic *grade performance* and students' *intellectual development*. Tinto (1975) understands students' grade performance as an indicator of "(…) meeting of certain explicit standards of the academic system" (p. 104). Performance deficits are interpreted as a mismatch between students' abilities and the requirements of their study program. Across many higher education systems, student performance is one of the most cited reasons for student dropout in Germany (Heublein et al.). We understand the meeting of the academic standards in terms of students' perceived academic difficulty of the study program, which we term "academic challenge". This measure captures the external conditions of academic integration imposed on the student by the study program. This approach also circumvents the problem that academic grades significantly vary across fields of study and thus convey a different meaning to each student. For example, the same absolute grade may be considered a good or a bad performance when comparing different fields of study and educational institutions (Müller-Benedict & Tsarouha, 2011). Previous research has found that academic difficulties and a lack of academic control increase the likelihood of dropping out (Respondek et al., 2020). Consequently, our third hypothesis is:

(H3): A high degree of academic challenge increases dropout intentions.

The second sub-dimension of academic integration refers to students' *intellectual development*, which Tinto describes as "an integral part of the person's personality development and as a reflection of his intellectual integration" (p. 105). In line with previous operationalizations (Dahm & Lauterbach, 2016), we understand this dimension as students' commitment to the academic content of the study program. Again, departing from an institutional perspective, we understand this commitment as being shaped by the curriculum and content of the study program. Using the Konstanz Student Survey, Georg (2009) finds that commitment to their field of study was the primary reason for students' dropout decision, also in comparison with academic difficulties and performance. Furthermore, a lack of interest in the field of study is cited as one of the most prevalent reasons for why students chose to abandon their studies in Germany (Heublein et al.). The last hypothesis derived from Tinto's model thus is:

(H4): A low degree of academic interest in the field of study increases dropout intentions.

Tinto's integration model has inspired many empirical studies which tend to confirm the postulated relationships. Early applications of Tinto's model from the U.S. compared students who dropped out with those who persisted and found that the former possessed lower levels of social and academic integration (e.g. Bers & Smith, 1991; Stage, 1989, for a review see Pascarella & Terenzini, 2005). Empirical evidence for Europe is less extensive, but it also generally supports the role of social and academic integration for student dropout decision (e.g. Bernardo et al., 2016; Chrysikos et al., 2017). Some studies support partial aspects of the model, such as engagement with peer and faculty (Bank et al., 1990; Schudde, 2019) or perceived academic control (Respondek et al., 2020). In Germany, which constitutes the context for our study, Klein (2019) and Klein, Schwabe & Stocké (2019) have tested the role of social and academic integration for student dropout in Bachelor and Master programs using a nationally representative sample. They find that all four sub-dimensions of student integration are associated with dropout intentions. Klein (2019) uses data from the Konstanz Student Survey to test, for the first time, predictions of Tinto's model in Germany. He finds that all four dimensions of integration are associated with the intention to dropout from higher education. The strongest predictor in his model was students' "extrinsic academic integration", which was operationalized – similarly to our approach – as perceived academic difficulties. As postulated by the model itself, the author also shows that social and academic integration are correlated with each other and are thus interdependent. The apparently strong interrelatedness of the integration dimensions, which has also been confirmed in other studies (Dahm & Lauterbach, 2016) underlines the difficulty to establish the unique and independent contribution of each integration dimension based on observational research designs.

4.3.2 Limitations of Previous Empirical Evidence and Contribution of the Present Study

A common limitation of the presented empirical evidence refers to the studies' inability to test for *causal* associations because these studies apply observational research designs which are limited in their ability to investigate the association between integration and dropout from a causal perspective. This is a clear deficiency when considering that only causal evidence can effectively guide measures to reduce student dropout. If the association between (academic and/or social) integration and dropout is a spurious one, measures targeted at improving students' integration would be misplaced. There are at least two alternative explanations for an apparent association between integration and dropout. First, the association could be driven by (unobserved) omitted variables which are correlated with both integration and dropout. If study programs with overall high dropout rates at the same time provide limited opportunities for social or academic interaction (e.g., due to size of the student body, teacher-student ratio or instructional arrangements), the association between student integration and dropout would be the result of common causes rather than causal influence. Since survey data is often limited regarding the availability of characteristics of the higher education institution, previous findings are prone to unobserved confounding. A second problem that arises with observational study designs is that the causal order cannot be established. For example, if students with a high dropout propensity are less motivated and engaged from the beginning and correspondingly show reduced efforts to integrate in the higher education environment, reverse causality could explain the association between integration and dropout (see e.g. Kim & Sax, 2009; Noyens et al., 2019). Our study improves upon these methodological limitations by employing a factorial survey which has not been applied before in the context of Tinto's integration model. A factorial survey design can establish the association between each dimension of student integration and dropout, ruling out possible confounding from omitted variable bias and reverse causality (this method will be described in detail in the methods section).

4.3.3 Student Integration and Academic Family Background

Tinto's model acknowledges the independent contribution of students' demographic and pre-entry characteristics to dropout. However, Tinto primarily understands individual characteristics in terms of additive effects, and he does not provide any theoretical predictions about how individual characteristics interact with students' experiences in higher education in predicting dropout. Although Tinto stresses that "it is the perceptions of the individual that are important" (Tinto, 1975, p. 98) when students form their dropout decision, his framework does not acknowledge the possibility that individuals may systematically differ in the thresholds at which they perceive their level of (academic or social) integration as detrimental. In our study, we extend the model by testing whether the effect of students' social and academic integration is conditional on students' academic family background, postulating that the same achieved level of academic and social integration does not lead to the same dropout decision for all students. Although early studies have emphasized the relevance of interactive effects (Pascarella & Terenzini, 1979), the empirical basis is rather limited (for recent endeavors see Kim & Sax, 2009).

In general, students from non-academic family backgrounds show higher dropout rates in many European countries (Contini et al., 2018; Herbaut, 2020). In Germany, the evidence is more mixed, with some studies finding no overall differences (Heublein & Wolter, 2011; Isphording & Wozny, 2018), while others find that students from non-academic backgrounds have higher dropout risks at university but not at applied sciences institutions (Müller & Schneider, 2013). The reasons for these social differences in dropout patterns are not yet well understood. First, social differences in dropout may stem from social differences in the *achieved* level of social or academic integration. For example, students from non-academic family backgrounds may have more difficulties when trying to integrate into academic institutions because they experience a lower congruency between their family habitus and the institutional habitus of the higher education system (Atkinson, 2011; Reay et al., 2010). Students from academic families possess relevant social and cultural capital through their families which facilitates their integration into the social and academic higher education system (Pascarella et al., 2004). For example, evidence suggests that students from non-academic family backgrounds are less well socially integrated (M. Rubin, 2012) and achieve lower grades (Hansen, 2006; Rodríguez-Hernández et al., 2020) than their peers. A second explanation for socially stratified dropout behavior relates to social differences in the *susceptibility* to dropout given the same level of integration. In other words, students from academic and non-academic households may systematically differ in their dropout behavior, despite having achieved the same level of integration. If students from academic and non-academic family backgrounds make different dropout decisions although their level of achieved integration is the same, they might be subject to a "second disadvantage" beyond that of social differences in achieved integration. The "compensatory advantage" hypothesis supports this view and suggests that students from academic family backgrounds are sheltered from negative experiences during (higher) education (Bernardi, 2014). Students from non-academic families are expected to be more heavily discouraged by experiencing academic difficulties, since they do not possess compensatory resources and support from academically oriented families and significant others outside of the higher education institution. Similarly, when experiencing a lack of social integration within their higher education institution, students from academic families can rely on academically oriented social contacts outside of their higher education institution. Herbaut (2020) supports the "compensatory advantage" expectation in the context of French higher education and shows that students from advantaged backgrounds are less likely to drop out after academic failure than disadvantaged students. Against this backdrop, we thus expect that:

(H5): Students from non-academic family backgrounds rely more strongly on their academic and social integration in their dropout decision.

In other words, we expect students from non-academic households to adjust their dropout intention more strongly to shifts in their academic and social integration compared to students from academic family backgrounds. More specifically, a compensatory advantage mechanism entails that a shift from high to low levels of integration will be associated with a stronger increase in dropout intention for students from non-academic family backgrounds than students from academic family backgrounds.

4.4 Method

4.4.1 Factorial Survey Design

To assess the importance of academic and social integration for students' dropout intention, judge its causality and determine whether the effect of integration on dropout varies by students' academic family background we conducted a factorial survey. Factorial surveys are experiments embedded within traditional surveys where each respondent judges a randomly allocated set (deck) of fictitious situations (vignettes) that vary according to pre-specified factors (dimensions). In our case, these vignettes provide a description of the respondents' study situation, which randomly differs along four factors capturing students' academic and social integration. This design enables us to identify both the impact of each factor independently as well as their relative effect when compared to the other factors.

Before presenting the vignettes, respondents were asked to imagine being in the third semester of their study program. The vignettes themselves are short texts describing students' academic and social integration at the start of their third semester. After reading through a vignette, students were asked to judge how likely they would drop out of the study program under the conditions presented, the answer to which constitutes our dependent variable. Within the factorial survey, four dimensions with three levels each (see Table 4.1) were varied. The complete vignette universe consequently consists of 81 possible combinations (3x3x3x3). A D-optimal sample (D-Efficiency = 97.4192) of 72 vignettes, with the aim to orthogonalize all main effects and two-way interactions (Atzmüller & Steiner, 2010; Auspurg & Hinz, 2015; Su & Steiner, 2020) while also keeping the set size low, was drawn from the vignette universe and assigned to 18 decks with four vignettes each. All vignettes within the vignette universe where plausible cases and thus retained. Each respondent received one randomly allocated deck, containing four vignettes. The number of vignettes per respondent was kept low to minimize fatigue and learning effects (Auspurg et al., 2009; Sauer et al., 2011). To reduce potential order effects within decks, the sequence in which the four vignettes were presented was also randomized. The order at which the dimensions were presented within each vignette was fixed to keep the text flow natural. Research on order effects of dimensions within vignettes suggests that such effects only occur when either the number of dimensions and vignettes per person is very high, the complexity of the task is great or the cognitive ability of respondents is impaired (Auspurg & Jäckle, 2017). In our research design, none of the factors contributing to order effects is present. Additionally, an experiment conducted by Düval & Hinz (2020) found no strong evidence for order effects in factorial surveys in general.

Table 4.1. Vignette dimensions and levels

	Dimensions	Levels		
		low	*medium*	*high*
1	Social integration: Faculty	In case of problems, teachers are **hard to reach** and they **do not adequately answer your questions**.	In case of problems, teachers are **available most of the time**, but they **only give short and not always satisfactory answers**.	In case of problems, teachers are **always available** and they **take the time to answer your questions thoroughly**.
2	Social integration: Fellow students	Among your fellow students, you have found **no friends** and **feel left alone when you have questions**.	Among your fellow students, you have found **some friends** who are **sometimes there for you if you have questions**.	Among your fellow students, you have found **multiple good friends** who are **always there for you if you have questions**.
3	Academic integration: Interest	The contents of your study program are **not interesting or fun**.	You occasionally enjoy your study program and you find the contents **interesting from time to time**.	The contents of your study program are **always interesting and a lot of fun**.
4	Academic integration: Challenge[1]	You can only succeed in your studies with **a lot of effort**.	You can succeed in your studies with **moderate** effort.	You can succeed in your studies with **little** effort.

[1] To keep the coding and visualization between dimensions consistent, i.e., that "high" always represents high amounts of integration in the respective subdimension, "high" academic integration: challenge refers to high ease, whereas "low" academic integration: challenge signifies low ease.

4.4.2 Sample

The factorial survey was embedded in the third wave of the "PraeventAbb" (Early prevention of dropout from higher education) panel study. This survey was originally set up to evaluate the impact of a student guidance workshop on students' entry into and persistence throughout higher education. For this purpose, high school students in their second to last year of high school, set to graduate in 2019, self-registered for the study. Participating students were mainly recruited from the surrounding area of two large German universities. They were surveyed at four waves throughout their transition from school to university.

In this study, we draw on the third wave of the panel study which was administered in the fourth quarter of 2019 (N=567 respondents). At this time, the majority of participants had graduated with a higher education entrance diploma (97.17% graduated with the German "Abitur"). We restricted the sample to students who had started their first semester in a higher education institution (N=343, 60.49% of wave 3 respondents), thus excluding students who did not immediately enroll into higher education after high school or chose an alternative educational pathway, such as vocational training. The restrictions to the sample were done in order to achieve high congruency between the vignette situation and students' real life situation, which in turn reduces artificiality in the judgment process (Auspurg & Hinz, 2015).

4.4.3 Variables and Estimation

The main outcome of our study are the vignette ratings of the factorial survey. After reading each vignette, students were asked to indicate how likely they would drop out of the study program under the circumstances described in the vignette (ranging from 1 "not likely at all" to 10 "very likely"). Although the vignette explicitly asked whether respondents intended to "drop out", it was left to the interpretation of the respondent whether they understood the question as leaving tertiary education completely or switching to another study program. Hence, our study does not explicitly differentiate between the destination states following students' decision to quit their study program, which may entail complete dropout or switching to an alternative study program (see e.g. Tieben, 2020).

The central independent variables are the four vignette dimensions, namely social integration, consisting of students' (1) integration within the academic faculty, (2) integration with fellow students, and academic integration, consisting of (3) academic interest and (4) academic challenge, each with three levels "low", "medium" and "high". Table 4.1 depicts the translated content of each dimension-level combination and Figure A4.1 (see appendix) shows a translated sample vignette from the survey.

Students' academic family background, needed for the assessment of H5, is a dichotomous variable which takes the value 1 if at least one parent has a university degree and 0 otherwise. Additionally, we included multiple covariates which have been discussed as relevant predictors for dropout decisions: age in months, gender, household composition, migration background, type of higher education institution, high school GPA, respondents' confidence in completing their study program and the average hours respondents spent on their studies per week (Heublein, 2014). Table A4.1 (see appendix) provides a detailed description and summary statistics of all covariates and how they are coded. The factorial survey variables, i.e., academic and social integration with their two subdimensions, are subsequently referred to and labeled as level-1 variables and respondent variables as level-2 variables.

To estimate the effect of each integration dimension on dropout, we employ multilevel models. Each respondent evaluated four vignettes, resulting in a hierarchical data structure where vignette evaluations are nested within respondents. To account for the multilevel structure, hierarchical linear models, more specifically random effect models are estimated for the main effects (for H1-H4) and cross level interactions (for H5) (Auspurg & Hinz, 2015; Raudenbush & Bryk, 2002). We also calculate fixed effect models (Allison, 2009) for our main effects estimation, which by design controls for all (observed and unobserved) time invariant between person differences, leaving only within person variation (responses to the vignettes). To test for effect heterogeneities (H5), cross-level interaction terms between academic background and each dimension were included into the random effect models.

4.5 Results

We test H1-H4 using random and fixed effect models to evaluate the extent to which each of our four integration dimensions influences dropout intention. Table 4.2 depicts the dimensions' main effects under four different model specifications (M1-M4). M1 to M3 are random intercept models that include different sets of level 1 and 2 covariates and M4 is a fixed effect model. M1, our baseline, is a random effects model containing only our main independent variables. M2-M4 are robustness checks that include different sets of control variables and estimation techniques to show that the main effects do not depend on respondent characteristics and design factors. M2 includes controls for potential deck effects, also referred to as set effects (Atzmüller & Steiner, 2010; Su & Steiner, 2020), which tell us whether respondent specific differences in response patterns are attributable to the deck the respondent was allocated to. M3 additionally includes observed respondent-level characteristics (level-2 covariates), such as gender, age and academic family background, describing the effect respondent level variables have on dropout intention. The model also serves as a randomization check by uncovering whether respondent level variables are correlated with the four vignette dimensions. Finally, the fixed effect estimation in M4 controls for both unobserved and observed between-person differences, leaving only variation within individuals, i.e., the effect of our four dimensions. There are only marginal differences between the effects of each dimension across M1-M4, which shows that randomization was successful (i.e., there is no systematic association between respondent characteristics and vignette dimensions) and that potential deck-effects do not change the main effect estimates substantially.

Table 4.2. Factorial survey analysis – effect of dimensions on dropout intention

	(M1) RE-Vign	(M2) RE-Vign-Deck[1]	(M3) RE-Vign-L2-Deck[1]	(M4) FE-Vign
Level 1 (vignettes)				
Social integration - fellow students (ref: high)				
low	1.618***	1.653***	1.652***	1.650***
	(11.90)	(11.91)	(11.90)	(11.94)
medium	0.849***	0.850***	0.852***	0.828***
	(6.24)	(6.12)	(6.14)	(5.99)
Social integration – faculty (ref: high)				
low	0.346*	0.373**	0.371**	0.377**
	(2.47)	(2.62)	(2.61)	(2.66)
medium	0.158	0.172	0.172	0.164
	(1.17)	(1.24)	(1.25)	(1.19)
Academic integration – challenge (ref: high)				
low	0.851***	0.896***	0.896***	0.897***
	(6.25)	(6.46)	(6.46)	(6.50)
medium	0.224	0.259	0.260	0.242
	(1.66)	(1.88)	(1.89)	(1.76)
Academic integration – interest (ref: high)				
low	4.088***	4.024***	4.025***	4.030***
	(29.93)	(28.94)	(28.93)	(29.11)
medium	1.582***	1.540***	1.539***	1.547***
	(11.59)	(11.08)	(11.07)	(11.18)
Level 2 (respondents)				
Gender			0.0533	
			(0.33)	
Academic background			0.114	
			(0.76)	
Migration background			-0.435**	
			(-2.98)	
Type of higher education institution			0.514**	
			(2.72)	
Living with parents			-0.0995	
			(-0.68)	
Age in months			-0.00807*	
			(-2.07)	
High school GPA			-0.0647	
			(-0.51)	
Confidence in completion			-0.206**	
			(-2.78)	
Average study hours per week			-0.00112	
			(-0.25)	
Constant	1.597***	1.059**	3.708***	1.596***
	(9.35)	(3.06)	(3.50)	(9.62)
Number of vignettes	1275	1275	1275	1275
Number of respondents	321	321	321	321
Std Dev u_j	0.870	0.797	0.710	1.336
Std Dev e_{ij}	1.958	1.958	1.958	1.958
Interclass corr. p	0.165	0.142	0.116	0.318

z statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

[1] Models 2 and 3 include dummy variables for each deck (k-1 Dummy Variables) to control for deck-effects (coefficients not shown).

To answer H1-H4, whether each integration dimension has an effect on dropout intention, we focus on the most saturated model (M3). The four integration dimensions are ordinally scaled, which is why they are included as dummy variables. The reference category of each dimension is "high integration", consequently the coefficients of each dimension convey the extent to which dropout intention changes when integration shifts from high to medium and from high to low respectively.

The results show that a reduction in any type of social or academic integration increases students' dropout intention, although the effect sizes vary greatly. The dimension with the largest impact on dropout intention is Ai-interest (Academic integration: interest), which increases dropout intention by 4.025 ($p < 0.001$) scale points when it is low (vs. high). Si-fellow students (Social integration: fellow students) has the second largest effect (1.652, $p < 0.001$, low vs. high), followed by Ai-challenge (Academic integration: challenge) (0.896, $p < 0.001$, low vs. high), and lastly Si-faculty (Social integration: faculty) (0.371, $p < 0.01$, low vs. high). To more adequately evaluate the impact each dimension has on dropout intention, we can y-standardize these effects, leaving us with the following coefficients: Ai-interest 1.427, Si-fellow students 0.586, Ai-challenge 0.318 and Si-faculty 0.132. For example, the y-standardized coefficient for Ai-interest tells us that a shift from high to low integration increases dropout intention by 1.427 standard deviations in the dependent variable. When comparing each effect with the weakest (Si-faculty), the difference in magnitude becomes clear. The effect of Ai-interest is approximately 13 times larger, Si-fellow still 5 times larger and Ai-challenge only 2 times larger than Si-faculty. Clearly, Ai-interest has a profound impact (both in relative and absolute terms) on dropout intention and Si-fellow students still a substantial one, while Ai-challenge has only a moderate effect and Si-faculty a weak one. When comparing the intermediate steps of each dimensions, we see that the results are consistent with the ordinal scaling of the independent variable, i.e., the effect size of the medium level always lies between the effect sizes of high and low integration. However, as is the case for Ai-challenge and Si-faculty, effects do not always reach statistical significance in comparison to the reference category, which could be the consequence of power limitations. The effects also do not appear to be linear for Ai-interest and Ai-challenge, where the difference between high vs. low and high vs. medium is more than double. Because the dimensions are ordinally scaled, such differences between levels are not unexpected.

In sum, our analyses show that both social and academic integration and their respective sub-dimensions have an independent and statistically significant impact on dropout intention, thus corroborating H1-H4. Furthermore, our analyses reveal that not all sub-dimensions are equally important, academic interest and social integration with fellow students are far more relevant for the formation of dropout intentions than academic challenge and social integration with the faculty.

To test hypothesis H5, whether the effect of academic and social integration and their subdimensions varies according to academic family background, cross-level interactions between each dimension and academic background were included (see Table A4.2 in the appendix for the full interaction model). The predicted margins of each interaction are shown in Figure 1 (with the corresponding point estimates in Table A4.3 in the appendix). Contrast effects, i.e., the difference in the difference of the effects, and their corresponding statistical significance are depicted in Table 4.3.

Figure 4.1. Predicted values of dropout intentions – cross level interactions

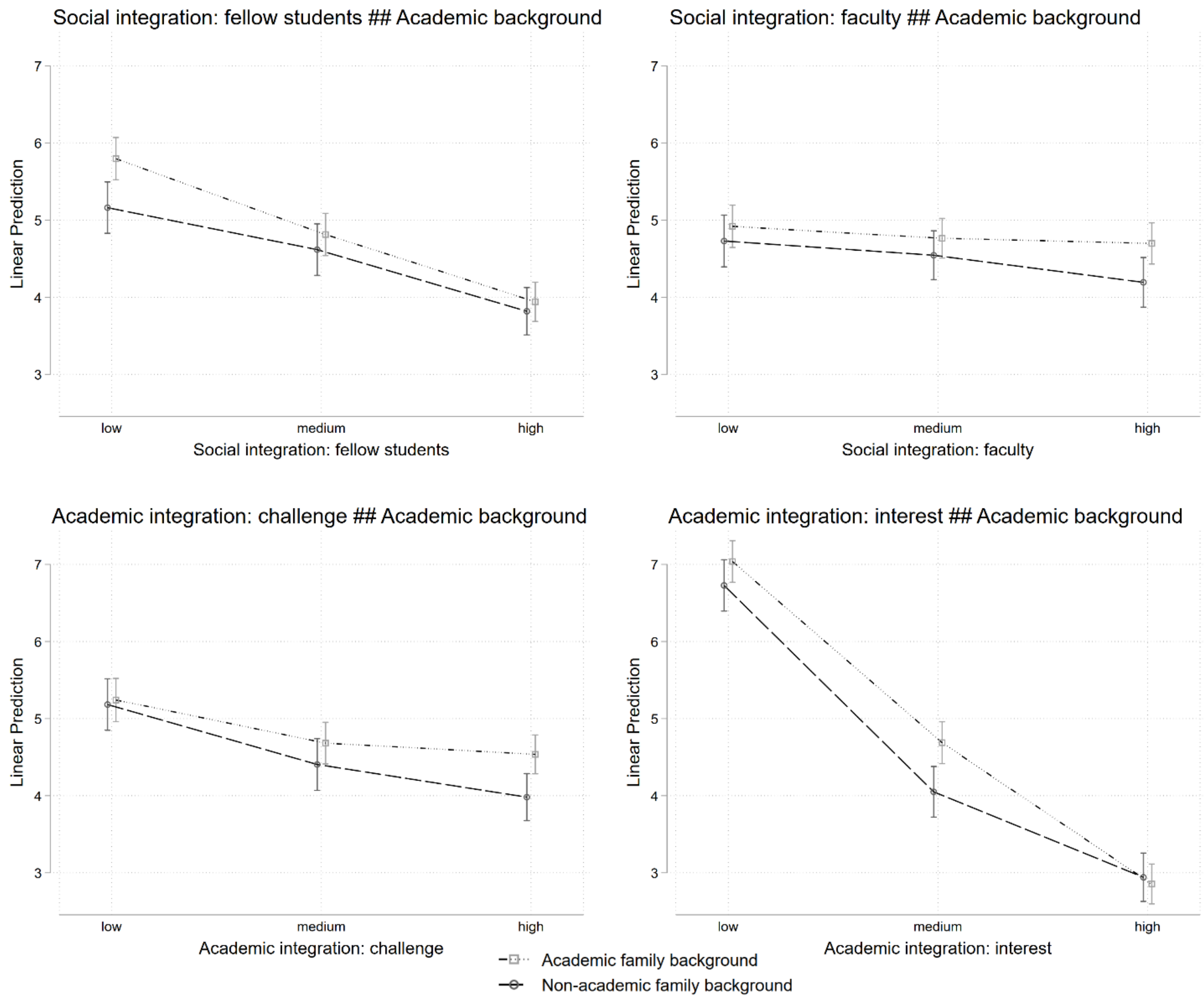


Table 4.3. Contrasts of cross-level interactions

	(1) Social integration - fellow students	(2) Social integration – faculty	(3) Academic integration – challenge	(4) Academic integration – interest
Academic background *low vs high* # dimension *low vs high*	0.513 (3.46)	-0.314 (1.22)	-0.497 (3.23)	0.397 (2.04)
Academic background *low vs high* # dimension *medium vs high*	0.0738 (0.07)	-0.284 (1.07)	-0.278 (1.03)	0.724 (6.80)**
Joint test	(3.88)	(1.53)	(3.29)	(6.85)*

All contrasts were calculated from Table A4.2. Chi^2 statistics in parentheses. Joint test has two degrees of freedom, the others one
* $p < 0.05$, ** $p < 0.01$

Visual inspection of the predicted margins in Figure 1 indicates whether students from academic and non-academic backgrounds differ in their dropout intention depending on the presented level of achieved integration. If students from academic and non-academic family backgrounds react differently to given levels of integration, we would expect the slopes to diverge from one another. As suggested by H5, we expect that students from non-academic family backgrounds will adjust their dropout intention more strongly than students from academic family backgrounds given shifts in their academic and social integration. Figure 1 first reveals a difference in intercepts, showing that students from non-academic family backgrounds express lower dropout intentions in general. Inspecting the slopes, we find that in every integration dimension the slopes of students from non-academic backgrounds and academic backgrounds diverge from one another either at high or low amounts of integration.

Focusing on Ai-challenge and Si-faculty, where the slopes diverge at high amounts of integration, we see that an increase in Ai-challenge and Si-faculty leads to a steeper decrease in dropout intention for non-academic students than for academic students. The contrasts in Table 4.3 for Ai-challenge and Si-faculty reveals the same pattern, e.g., the effect on dropout intention of receiving a low compared to a high Ai-challenge vignette is 0.497 points greater for students from non-academic backgrounds than students from academic backgrounds. Hence, non-academic students profit slightly more from high levels of integration than academic students. These differences, although small, are generally in line with our expectation that non-academic students are more responsive to shifts in integration (H5). Interestingly, slope divergence occurs at high levels rather than low levels of integration as suggested by compensatory advantage theory. However, both interaction effects fail to reach statistical significance.

Turning to the remaining two integration dimensions, Si-fellow students and Ai-interest, we find the opposite pattern, namely that lower levels of integration have a less pronounced effect on dropout intention for students from non-academic backgrounds than students from academic backgrounds. The effect of low vs. high Si-fellow students is 0.513 points lower for students from non-academic background than students from academic backgrounds, although the effect is not statistically significant (chi^2 3.46). The only statistically significant cross-level interaction is between academic background and Ai-interest (joint significance of $p < 0.05$). The effect of high integration is very similar for both students from non-academic backgrounds as well as academic backgrounds, but lower levels of integration increase dropout intention more for students from academic backgrounds than for students from non-academic backgrounds (0.724 for mediums vs. high and 0.397 for low vs. high).

Overall, our results do not support H5. The small differences we observe between the two groups for Ai-challenge and Si-faculty, while in line with H5, are not statistically significant. Moreover, the group differences of Ai-interest and Si-fellow student depict a pattern opposite to the one suggested by H5, with only Ai-interest reaching statistical significance.

4.6 Discussion and Conclusion

This study tested the relevance of academic and social integration for students' higher education dropout intentions, motivated by Tinto's student integration model. Going beyond previous research, we applied a factorial survey design with the aim to uncover students' actual decision-making process. From a methodological perspective, this study advances previous observational research which was unable to account for bias from reverse causality or omitted variables in the association between integration and dropout intentions. Additionally, we advance theory and empirical research on social inequalities in higher education by testing for social differences in students' dropout propensity given different levels of achieved academic and social integration. Our findings generally support the predictions derived from Tinto's integration model in that all four dimensions of integration are related to students' dropout intentions. Students actively employ criteria tied to their academic and social integration when deciding whether to drop out of or continue their chosen higher education program. We thus conclude that the association between (academic and social) integration is not purely driven by selection or reverse causality processes, lending legitimacy to observational research designs.

Our study also revealed that not all dimensions of integration are equally important. The strongest effect on students' dropout intention was found for academic interest in their field of study, followed by social integration with fellow students. Perceived difficulty of the study program and students' social integration with faculty, on the other hand, were only moderately to weakly related to students' dropout intention. The important role of academic interest supports career theories which place an emphasis on the decisive role of the match between students' interests and their educational field or occupation for their satisfaction and retention (Holland, 1959). At the same time, our study contrasts findings that posit that difficulties with the study content are the most important dropout reasons in the German context (Heublein, 2014). But the rank order of our effect sizes comes with two caveats. First, although each integration dimension has the same three levels, the variables are only ordinally scaled because there are no universally valid metric definitions of social and academic integration. As such, differences between low and high integration in one dimension cannot be guaranteed to correspond exactly to such a difference in another dimension. Second, the effect of academic interest and social integration with fellow students could partly be driven by primacy and recency effects since these dimensions were always presented first and last in each vignette. However, this concern may be alleviated by the fact that our factorial survey was designed to prevent order effects (Auspurg & Jäckle, 2017) and experimental research on factorial survey design found no strong evidence for the occurrence of order effects in general (Düval & Hinz, 2020).

Regarding social stratification in students' decision-process, we expected students from non-academic family backgrounds to adjust their dropout intention more strongly to shifts in academic and social integration than students from academic family backgrounds. The "compensatory advantage" theory more specifically suggests that students from academic family backgrounds would be less responsive to low levels of integration (Bernardi, 2014). Generally, we found little evidence supporting this expectation. The only statistically significant difference between the two groups showed the opposite pattern, namely that students from academic family backgrounds relied more strongly on their academic interest in their dropout intention than students from non-academic family backgrounds. The absence of pronounced differences between the two social groups implies that students from academic and non-academic backgrounds react similarly to the same levels of integration. Our study suggests that students from academic family backgrounds are not generally sheltered from experiencing low levels of social or academic integration as previous research suggests (Herbaut, 2020). Hence, although students from non-academic family backgrounds may face difficulties integrating into the academic and social system of higher education in the first place, they do not experience a "second" disadvantage resulting from more negative reactions to social and academic integration.

There are several potential explanations for the absence of a "compensatory advantage" effect in our study. First, the dropout definition in our study is different from the one used in other studies which found such an effect. For example, Herbaut (2020) defined dropout as permanently leaving higher education, while in our study, dropout intentions may encompass leaving higher education or changing to a different field of study. Second, the absence of pronounced social differences could also be explained by the fact that students from non-academic family backgrounds studying at university already surpassed the threshold of entering university, which is marked by pronounced social inequalities (Schindler & Reimer, 2011). They therefore constitute a highly selective group of students who are potentially more resilient to negative experiences. Furthermore, given that students from non-academic family backgrounds are often in a disadvantaged financial situation, these students have higher financial and opportunity costs when dropping out of their study program or switching to another field of study, possibly making them less responsive to negative integration experiences. This line of argumentation could also explain why our study revealed that students from academic family backgrounds expressed higher dropout intentions when their interest in the field of study was low than students from non-academic family backgrounds. Students from academic family backgrounds may have less financial and opportunity costs associated with switching to a more interest driven educational alternative and thus consider the idea of dropping out more freely. Finally, students from academic family backgrounds may have stronger expectations of attaining high levels of integration at university given their family habitus. When these expectations are not met, they may be even more discouraged from detrimental levels of integration than their non-academic peers, resulting in higher dropout rates. Ultimately, the effects discussed so far may also cancel each other out on the aggregate level, which could explain why we did not find any significant social differences in students' dropout propensity given their achieved level of integration. To advance our understanding about socially stratified dropout patterns, future research should scrutinize the precise reasons for why students drop out and whether these reasons are socially stratified.

On a more general level, we want to put our findings into a wider context. Our results stem from a non-representative sample, consisting of first-year university students of a limited regional scope, which is not representative for the whole German population. Generalizations of this study should therefore be context-specific as they apply primarily to university students in the region of North-Rhine Westphalia in Germany. This region of Germany is characterized by a relatively dense landscape of universities compared to other German states and other countries. Students' dropout decision may be different when the overall supply of alternative study programs is more limited. For example, if the supply of universities is very dense, students may face lower costs of changing their higher education institution. On the other hand, if the supply of alternative fields is more scattered, students may decide to continue with their chosen study program even in light of negative experiences, anticipating higher costs due to needing to change their place of residence. Future research is needed to study and compare influences on students' dropout behavior in varying regional and national contexts.

A general limitation of this study relates to the artificial nature of factorial survey designs. The use of hypothetical scenarios, aiming to represent real-world situations as close as possible, is common practice in factorial survey designs. To reduce artificiality as much as possible, we restricted our sample to enrolled university students who are very close to the judgement task. Still, under real-world conditions students may react differently than under these hypothetical presentations. An alternative causal approach to a factorial survey design would consist in randomly varying levels of integration in the "real world", for example, by implementing interventions increasing students' academic or social integration at university. However, since such external manipulations of students' higher education experiences are complicated from an ethical and logistic perspective, we believe that our study constitutes an important contribution to advancing causal claims in research on the association between student integration and dropout.

4.7 Appendix

Table A4.1. Descriptive statistics

Variable	Coding	Mean	SD	Min	Max
Dependent variable					
Dropout Intention	1: very unlikely, 10: very likely	4.67	2.811	1	10
Level 1 (vignettes)					
Social integration - fellow students					
low		0.311		0	1
medium		0.312		0	1
high		0.377		0	1
Social integration - faculty					
low		0.308		0	1
medium		0.360		0	1
high		0.332		0	1
Academic integration - challenge					
low		0.300		0	1
medium		0.318		0	1
high		0.382		0	1
Academic integration – interest					
low		0.319		0	1
medium		0.320		0	1
high		0.361		0	1
Level 2 (respondents)					
Gender	0: male, 1: female	0.729		0	1
Academic family background	0: parents do not have a university degree, 1: at least one parent has a university degree	0.598		0	1
Migration background	0: no migration background, 1: migration background	0.416		0	1
Type of higher education institution	0: university of applied sciences, 1: university	0.828		0	1
Living with parents	0: no, 1: yes	0.540		0	1
Age in months		213.8	18.771	192	409
High school GPA	1.0 (very good) - 4.0 (sufficient)	1.942	0.586	1	3.4
Confidence in completion	1: very unlikely, 5: very likely	3.928	0.968	1	5
Average study hours per week		25.685	16.168	0	80

N(respondents) = 321
N(vignettes) = 1275

Table A4.2. Factorial survey with cross level interactions – effect on dropout intention

	Cross Level Interaction Model
Level 1 (vignettes)	
Social integration - fellow students (ref: high)	
low	1.344*** (6.25)
medium	0.799*** (3.71)
Social integration – faculty (ref: high)	
low	0.536* (2.42)
medium	0.351 (1.64)
Academic integration – challenge (ref: high)	
low	1.201*** (5.61)
medium	0.423* (1.97)
Academic integration – interest (ref: high)	
low	3.786*** (17.47)
medium	1.108*** (5.13)
Cross Level Interactions	
Academic background ## Si – fellow students *low*	0.513 (1.86)
Academic background ## Si – fellow students *medium*	0.0738 (0.27)
Academic background ## Si – faculty *low*	-0.314 (-1.10)
Academic background ## Si – faculty *medium*	-0.284 (-1.03)
Academic background ## Ai – challenge *low*	-0.497 (-1.80)
Academic background ## Ai – challenge *medium*	-0.278 (-1.01)
Academic background ## Ai – interest *low*	0.397 (1.43)
Academic background ## Ai – interest *medium*	0.724** (2.61)
Level 2 (respondents)	
Academic background	0.00658 (0.02)
Gender	0.0564 (0.35)
Migration background	-0.432** (-2.95)
Type of higher education institution	0.516** (2.73)
Living with parents	-0.112 (-0.76)
Age in months	-0.00823* (-2.10)
High school GPA	-0.0691 (-0.55)
Confidence in completion	-0.205** (-2.76)
Average study hours per week	-0.00138 (-0.31)
Constant	3.812*** (3.52)
Number of vignettes	1275
Number of respondents	321
Std Dev u_j	0.722
Std Dev e_{ij}	1.950
Interclass corr. p	0.121

z statistics in parentheses. Dummy variables for each deck are included to control for set-effects (coefficients not shown).
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4.3. Predicted margins of cross-level interactions

	(1) Social integration - fellow students	(2) Social integration – faculty	(3) Academic integration – challenge	(4) Academic integration – interest
Academic background *low* ## dimension *low*	5.162 [4.829,5.495]	4.729 [4.394,5.065]	5.183 [4.849,5.517]	6.728 [6.395,7.060]
Academic background *low* ## dimension *medium*	4.618 [4.283,4.952]	4.545 [4.227,4.862]	4.405 [4.070,4.741]	4.050 [3.722,4.379]
Academic background *low* ## dimension *high*	3.818 [3.511,4.125]	4.193 [3.871,4.516]	3.982 [3.677,4.286]	2.942 [2.628,3.256]
Academic background *high* ## dimension *low*	5.798 [5.523,6.072]	4.920 [4.645,5.195]	5.241 [4.961,5.521]	7.038 [6.769,7.307]
Academic background *high* ## dimension *medium*	4.814 [4.540,5.087]	4.766 [4.509,5.022]	4.682 [4.412,4.952]	4.688 [4.417,4.960]
Academic background *high* ## dimension *high*	3.941 [3.687,4.194]	4.698 [4.430,4.966]	4.536 [4.285,4.788]	2.855 [2.597,3.114]
N	1275	1275	1275	1275

Predicted margins were calculated from Table A4.2. 95% confidence intervals in brackets.

Figure A4.1. Translated sample vignette

Imagine, at the start of the second study year, your situation has developed as follows (Scenario C):

Among your fellow students, you have found some friends who are sometimes there for you if you have questions. In case of problems, teachers are available most of the time, but they only give short and not always satisfactory answers. You can succeed in your studies with little effort. You occasionally enjoy your study program and you find the contents interesting from time to time.

Under these conditions, how likely would you decide to drop out of your study program?

1 very unlikely
2
3
4
5
6
7
8
9
10 very likely

Chapter 5: Discussion

In the following discussion, I focus on the central findings, limitations and extensions as well as implications for theory, research and practice for all papers presented in my dissertation. The first part of this discussion focusses on Chapters 2 and 3, while the second part focusses on Chapter 4. I complement and extend the discussion presented within each paper by incorporating aspects of the larger theoretical framework into the discussion and doing supplementary analyses that are not part of the papers themselves.

One key insight the first two papers of my dissertation (Chapters 2 and 3) delivered was that the information treatment, in the form of a single day university guidance workshop, influenced *field of study considerations* but did not affect *field of study choices*. Hence, the effect of information on study program decisions seems to be short lived. The second paper, presented in Chapter 3, directly showed this by providing evidence that the information treatment only affected considered field of study homogeneity and not chosen field of study homogeneity. The first paper, presented in Chapter 2, was written before access to the third wave of the study was available, which is why no long-term treatment effects on gender-atypical and non-beaten path program choices where examined. A natural question thus is whether the same lack of long-term effects can be observed for these outcomes. Supplementary analyses (see Table A6.1 in the appendix) show that there are no statistically significant long-term effects of the information treatment on choosing a gender-atypical or non-beaten path study program in wave 3, corroborating the overall conclusion that the counseling workshop did not produce long-term effects on field of study choices. Incorporating these findings into the larger theoretical framework, the results of the first two papers indicate that the information intervention may have been successful in increasing the set size of feasible alternatives (F), most likely by increasing the set of alternatives students have information on (I) and the set of study programs that are accessible (A). Similarly, the treatment may have changed the values of the parameters for the utility functions of some study programs that lead to a new utility rank order of study programs within F. But the increase in *considered* alternatives F or new rank orders for the elements of F did not yield a $\max_{i \in F} U_i$ that reflected the *choice* of a gender-atypical or non-beaten study program or resulted in less field of study homogeneity in the close friend network in the long run.

There are multiple potential explanations for the lack of long-term effects, three of which I want to highlight here: (1) information saturation, (2) information disregard and (3) independent search for information. In Chapter 3, I already introduced information saturation as a potential explanation for the reduction or lack of long-term effects. The core idea is that all students reach a maximally saturated information level prior to making the study program decision. Students of the treatment group just reached this information level sooner by virtue of participating in the workshop than students in the control group. For this reason, short-term effects but no long-term effects could be observed. Of course, the information composition of the maximally saturated information level prior to an educational decision can be far from optimal or accurate. For this reason, we thought that the workshop provided an *additional* information benefit, that is not naturally gained through other means. However, the information saturation argument suggests that the information gained through the workshop did not yield an information benefit in the long run for the treatment group, and students in the control group were able to reach the same information level through alternative sources of information. If this is the case, long-term effects may be achieved by increasing the amount of hard to get or unique information the workshop provides.

The second reason for the lack of long-term effects is related to the finding that people tend to disregard information disconfirming their beliefs (Andrew & Hauser, 2011; Einhorn & Hogarth, 1986). In Chapter 1.1.1, I incorporated social psychological research on dual system modes of cognition (Chaiken & Trope, 1999) to describe under which circumstances new information should and when it should not influence decision making. Given the many factors that contribute to a reasoned decision-making mode for this type of educational choice and on account of finding short-term effects, it seems implausible that the reason for the lack of long-term effects is information disregard alone.

The last potential reason I want to highlight is independent search for information in the post treatment phase. This explanation is motivated by the moderator we analyzed in Chapter 2, namely information level prior to the workshop. Our analyses revealed that prior level of information only had a statistically significant interaction with consideration of non-beaten path study programs but not with the consideration of a gender-atypical study programs. We suggested that one potential reason for the lack of an interaction effect between prior level of information and consideration of gender-atypical study programs was that when students *independently* search for information, they may be less likely to seek out information on gender-atypical study programs themselves. However, when confronted with gender-atypical study programs through an external source of information, such as the information treatment, students are more likely to give these study programs their due diligence. This argument can be extended and combined with the finding that independent search for information is not bias free. Since people tend to prefer information confirming their believes rather than information opposing them (Jonas et al., 2001), the lack of long-term effects may also be due to post treatment independent information search. Within the post treatment information gathering phase, students may reaffirm mostly believes and considerations they held prior to the workshop rather than strengthen new considerations the workshop made them realize. Consequently, as time progresses, the effect of the workshop diminishes. It thus also stands to reason that long-term effects on study program decisions may be achieved with stronger stimuli, such as multiple counseling sessions where newly realized beliefs can be reaffirmed and nurtured, and more individualized counseling sessions where newly realized beliefs are engrained more deeply.

In the end, more research is needed to assess why information treatments for high school students loose effectiveness in the long run and whether it is connected to information saturation, information disregard or independent information search.

Given the mixed evidence on the effectiveness of the counseling workshop one may ask: "Should these types of guidance programs still be pursued?". Answering this question is difficult and it should not be done hastily for numerous reasons. Although information treatments, such as one day counseling workshops, may not be very effective in changing students' field of study choices regarding the outcomes presented in this dissertation, research shows that information treatments can influence study program decisions in other ways. For example, Barone et al. (2019) found that providing students with information on monetary returns to field of studies increased the likelihood that women, especially undecided ones, chose more lucrative majors. Furthermore, information treatments in the form financial advice on financing higher education have been shown to increase other tertiary education related outcomes, such as enrollment rates into higher education (Loyalka et al., 2013; Peter et al., 2018). However, many information treatment evaluations only look at short-term impacts, which tend to be positive, but they lack the long-term perspective. It remains unclear whether the positive effects found in some interventions (e.g., on enrollment rates) are sustainable and lead to higher completion rates. Studies with longer duration often only find short- but no long-term effects of counseling on educational outcomes (Daniel et al., 2018; Wang et al., 2016). These findings are in line with the ones presented in my dissertation and a follow up paper we wrote, where we found that the workshop did not have a statistically significant effect on university enrollment rates, students' choice of and satisfaction with study programs or dropout behavior (Fervers et al., Forthcoming).

There are also several limitations to our study, which should be considered when evaluating whether these types of guidance programs benefit students. Limitations have been discussed at different points throughout Chapters 2-4, and I only want to highlight two here relating to our sample. First, we utilized a voluntary sample of motivated high school students, which are not representative of high school students in Germany. Our sample was particularly motivated and already exhibited an affinity for information acquisition by virtue of registering for our study. This illustrates one central limitation inherent in most student counseling programs: its voluntary character. Those who might profit most from counseling are not the same students who will voluntarily partake in counseling. For this reason, the information intervention treatment effect could be different for a representative sample of German high school students. However, most counseling programs are voluntary and as such our findings provide evidence for the real-world effectiveness of these types of high school counseling workshops. Second, the control group of our study could not be prohibited from gathering information or applying for a counseling workshop elsewhere. Students in the control group compensated to some degree by taking more advantage of other counseling opportunities, which once again suggests that the treatment effects where subject to ceiling effects. Nevertheless, it is implausible to assume that students interested in enrolling into higher education do not gather any information at all, as such the question is whether the counseling intervention we provided offered a tangible additional information benefit compared to other sources of information - which it did, albeit only in the short-term.

In the end, most papers - and the papers presented here are no exception - only show the effectiveness of a single intervention concept on a limited range of outcomes. Truth is not uncovered by a single study, it is more adequately viewed as a cumulative or generative process (Goldthorpe, 2001) where each study contributes to its discovery. So, before the question whether counseling workshops are useful in general can be answered, more experimental research on the effectiveness of information treatments on students' field of study choice in particular and other educational outcomes in general is needed.

The last paper of my dissertation, presented in Chapter 4, provided causal evidence for the relevance of students' social and academic integration into the higher education system for their drop out decision. Our vignette experiment revealed that all subdimensions of academic and social integration had a significant effect on dropout intention, thus corroborating Tinto's (1975) model of student dropout from higher education. Interestingly, students' academic family background did not have a significant interaction with most dimensions of academic and social integration and in general affected dropout intention contrary to our expectations. I want to highlight and further discuss this finding as it stands in contrast to most research on educational inequality, which posits that students from disadvantaged social backgrounds act and react differently to educational parameters (Barone et al., 2018; Boudon, 1974; Breen & Goldthorpe, 1997; Erikson & Jonsson, 1996; Stocke, 2007). For example, in Breen and Goldthorpe's (1997) educational decision making model, students from high social classes are said to have a strong status maintenance motive. The result of this motive is that they value the acquisition of a degree more than students of lower social classes, which is reflected in their high subjective assessments of the benefit (B) of completing a study program (U_c). Consequently, U_c for students of high social origins should on average be higher than U_c for students from low social origins. Similarly, the utility of dropping out of a study program (U_g) should be on average lower for students from high social origins compared to students from low social origins, since the subjective costs (C) (e.g., loss of status) of dropping out is higher for students from high social origins. Therefore, the likelihood that $\max_{i \in D} U_i = U_c$ should be on average higher for students from high compared to students from low social origins. Provided that dropout intention reflects the difference in utility between U_c and U_g, such that the greater $U_c > U_g$ the lower the dropout intention and the greater $U_c < U_g$ the higher the dropout intention, we would have expected an intercept shift between students from academic and non-academic family backgrounds, where students from academic family backgrounds should have exhibited lower dropout intentions in general. Instead, we found the opposite pattern, namely that students from non-academic backgrounds in general were less likely to express high dropout intentions. Similarly, Breen and Goldthorpe's (1997) model suggests that given the high utility of U_c for students from academic family backgrounds these students could also be less responsive to changes in their level of academic and social integration, an argument complementing the compensatory advantage theory (Bernardi, 2014) we focus on in the paper. In other words, students form academic family backgrounds should be less dependent on their level of academic and social integration for their dropout intention when compared to students from non-academic family backgrounds. But once again, we found no evidence for this effect as most interactions between integration dimensions and academic family background were statistically insignificant. The only significant interaction effect (academic interest and academic family background) displayed the opposite pattern, namely that for students from non-academic family backgrounds the effect of changes to levels of academic interest was lower than for students from academic family backgrounds. Our results suggest that, although the choices to enroll into higher education may be stratified along class lines, once the hurdle of enrollment into higher education is overcome, the dropout propensity of students from high and low social origins are not very different from each other given changes in academic and social integration. This finding is encouraging and signals that policies aiming to reduce educational inequality stemming from differences in social origin could focus on helping students from non-academic family backgrounds into higher education and do not have to allocate as many resources into supporting these students after they enrolled into higher education. But before such a suggestion is considered for implementation, more research on this topic is needed that corroborates these findings.

A central limitation of our analysis lies within the artificial character of vignette studies. Do vignette ratings really transfer to real world decisions? To assess this question, I ran supplementary analyses (see Table A6.2 in the appendix) in which I modeled the effect of each integration dimension as measured via a single Likert scale (see Figure A6.1 in the appendix for the question text) in wave 3 of the study on a dichotomous dropout indicator (Table A6.2 M2 OLS & M3 Logit) and a dropout likelihood indicator (Table A6.2 M1 OLS) measured in wave 4. The dichotomous dropout indicator was coded as "1" if students had changed or dropped out of their study program and "0" if they stayed. The dropout likelihood indicator consists of the mean of two 5-point Likert scales on the likelihood of dropping out of university and the likelihood of changing study programs (ranging from 1 "low chance" to 5 "high chance"). That is to say, I show whether the degree of social and academic integration in the first semester predicts dropout decisions within the first three semesters (Table A6.2 M2 & M3) and dropout intention for those who did not drop out until the third semester (Table A6.2 M1). The results for actual dropout behavior show that only academic interest, the dimension with the highest impact in the vignette study, still remains a very potent predictor of dropout likelihood. The OLS regression (Table A6.2 M2) reveals that an additional point of interest in the study program reduces dropout likelihood by 9.57 percentage points ($p < 0.01$), a finding corroborated by the logit model (Table A6.2 M3). All other dimensions of academic and social integration do not reach statistical significance and are substantively smaller. The effect of wave 3 academic and social integration on dropout intention for those who are still studying in wave 4 depicts a similar pattern, namely that academic interest is still the strongest predictor of dropout intention ($\beta = -0.148$), although the effect is not as statistically significant ($t = -1.94$). These supplementary analyses suggest that academic and social integration, at least as far as the academic interest is concerned, do not only affect dropout intention within vignettes, but also dropout behavior in real life. Likewise, they corroborate the finding that academic interest is a central predictor for dropout decisions, and keeping students engaged and interested in their chosen study program may reduce dropout behavior.

Chapter 6: Summary and Conclusion

The pursuit of knowledge is a lifelong endeavor that can be as rewarding as it can be frustrating. To minimize the amount of frustration and maximize the joy, personal and societal growth education can promote, choosing an educational path that one can flourish in is imperative. For this to happen it is essential to develop and evaluate measures that can assist people in making the best educational decision they can.

My dissertation contributed theoretically and empirically to this important subject matter in multiple ways. Theory driven, I described educational decision-making within the RC framework and showed how the study program decision can be influenced by information. Additionally, I presented how the study program decision and the dropout decision are connected according to RC by emphasizing that the study program decision is a direct precursor of the decision to stay in the higher education. Empirically, the dissertation corroborated the theoretical expectations on multiple accounts. Chapters 2 and 3 showed that information, provided by means of a counseling workshop within the context of an experiment, indeed influences study program considerations of high school students. The two questions asked within the introduction, namely (1) “Are information deficits partly to blame for the large focus students put on well-known and gender typical study programs?” and (2) “Do information deficits provide an explanation for homogeneity formation in small and dense networks?”, can both be answered with a cautious: “Yes, but there are more factors at play and further research is needed”. The caveat to this answer is that no information effects on study program choices were found. This result highlights an important future research avenue that should focuses on why short-term effect may not translate into the long-term and what can be done to increase the long-term effectiveness of information treatments. Chapter 4 focused on the factors within higher education that influence dropout. Based on the theoretical framework the question I posed was, why the utility of the study program decision is not equal to the utility of completing said study program when enrolled in higher education ($\max_{i \in F} U_i \; != U_c$). To answer this question, Chapter 4 of my dissertation tested Tinto's (1975) integration model by means of a vignette experiment. The results revealed how important the factors *within* higher education, such as academic and social integration (especially academic interest), are for students' dropout decision. These findings thus show how $\max_{i \in F} U_i$ may diverge from U_c once students are enrolled in higher education. Everything taken together, my dissertation provides a comprehensive account of two decisive and intertwined educational decisions: the study program decision and the dropout decision. Hopefully, the insights of this dissertation can prove helpful for devising new measures that help high school students choose appropriate study programs and improve their likelihood of succeeding in them.

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Appendix

Table A6.1. Treatment effect on field of study *choices* in w3

	(1) non-beaten path	(2) gender-atypical study choice
Treatment	**-0.0143**	**-0.0362**
	(-0.22)	**(-0.76)**
Pre-treatment outcome	0.349***	0.331***
	(6.20)	(4.89)
Female	-0.00672	-0.0915
	(-0.12)	(-1.66)
University degree	-0.0351	-0.0707
	(-0.68)	(-1.86)
Household: siblings	-0.0690	0.0165
	(-1.25)	(0.45)
Household: both parents	-0.0283	-0.000744
	(-0.46)	(-0.02)
Parental study aspiration	-0.0506	-0.0418
	(-0.87)	(-0.88)
Start of information gathering: during this school year/not yet		
during the last school year	0.0571	0.0257
	(1.05)	(0.60)
before upper-secondary education	-0.0638	-0.0168
	(-0.83)	(-0.35)
Age in months	0.00260*	0.0000910
	(2.03)	(0.07)
Grade school report 2017 (mean)	0.177***	-0.0215
	(4.54)	(-0.70)
Study aspiration of schoolmates	-0.0285	-0.0110
	(-1.00)	(-0.51)
Constant	-0.332	0.290
	(-0.96)	(0.83)
N	328	328

IV regressions with robust standard errors. z statistics in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A6.2. Effect of social and academic integration on dropout *behavior* and dropout *intention*

	(M1)[1] Dropout intention	(M2)[1] Dropout behavior OLS	(M3)[2] Dropout behavior Logit
Social integration - fellow students	-0.0214	0.00396	0.0599
	(-0.35)	(0.19)	(0.38)
Social integration – faculty	0.0269	0.0182	0.132
	(0.41)	(0.74)	(0.69)
Academic integration – challenge	-0.0591	-0.0179	-0.205
	(-0.85)	(-0.57)	(-0.92)
Academic integration – interest	-0.148	-0.0957^{**}	-0.634^{**}
	(-1.94)	(-3.15)	(-2.81)
Gender	0.117	0.0318	0.182
	(1.05)	(0.62)	(0.41)
Academic background	-0.115	0.0221	0.174
	(-1.11)	(0.46)	(0.46)
Migration background	-0.0462	-0.0678	-0.480
	(-0.46)	(-1.50)	(-1.24)
Type of higher education institution	0.176	0.102^{*}	1.024
	(1.51)	(2.04)	(1.67)
Living with parents	-0.224^{*}	-0.0157	-0.137
	(-2.16)	(-0.33)	(-0.36)
Age in months	0.00564	-0.00135	-0.0128
	(1.10)	(-1.37)	(-0.93)
High school GPA	0.160	0.0440	0.381
	(1.55)	(1.07)	(1.21)
Confidence in completion	-0.275^{***}	-0.0982^{**}	-0.565^{**}
	(-3.61)	(-3.05)	(-2.64)
Average study hours per week	0.0000817	-0.00159	-0.0127
	(0.02)	(-1.03)	(-1.01)
Constant	2.065	1.098^{***}	4.492
	(1.62)	(3.58)	(1.39)
N	215	265	265
R^2	0.200	0.183	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[1] t statistics in parentheses, robust standard errors

[2] z statistics in parentheses, robust standard errors

Figure A6.1. Question text for academic and social integration in w3. Answers ranging from (1) strongly disagree to (5) strongly agree

Social integration - fellow students:
I was able to make contact with other students while studying.

Social integration – faculty:
I feel supported by the faculty of my study program.

Academic integration – challenge:
I am able to meet the demands of my study program.

Academic integration – interest:
I have great interest in the content of my study program.