

Peers Affect Personality Development*

Xiaoyue Shan

(National University of Singapore)

Ulf Zölitz

(University of Zurich)

August 2023

Abstract

Do people around us influence our personality? We study this question with a field experiment where we randomly assign university students to study groups. We find personality spillovers along three dimensions: students become more conscientious when assigned to conscientious peers, more open-minded when assigned to open-minded peers, and more competitive when assigned to competitive peers. We find no effects for peers' extraversion, agreeableness, or neuroticism. Our findings are consistent with students adopting peer traits predictive of academic achievement. Our paper provides novel evidence on spillovers in noncognitive skills and establishes that socialization with peers affects personality development.

Keywords: personality, spillovers, field experiment, peer effects

JEL classification: I21, I24, J24

* This experiment received IRB approval from the University of Zurich (OEC IRB #2018-021) and was registered at the American Economic Association's registry for randomized controlled trials before the start of the experiment in 2018 (AEARCTR-0003255). In a previous version of this paper ([IZA Discussion Paper #15257](#)) we estimated personality spillovers using data from the first three waves of the experiment in 2018/19, 2019/20 and 2020/21. This version uses additional data from replications of the experiment in 2021/22 and 2022/23 and confirms previous results. We received helpful comments from Jan Bietenbeck, Alexandra de Gendre, Bart Golsteyn, Jan Feld, Edwin Leuven, Corinne Low, Nicolás Salamanca, and participants of the CESifo Area Conference on Economics of Education, SOLE 2022, AFE 2022, and the World ESA Meeting 2022, and seminar participants at the University of Zurich and the University of Pennsylvania. We thank Anna Valyogos, Matthew Bonci, and Timo Haller for outstanding research assistance. Xiaoyue Shan: National University of Singapore (email: x.shan@nus.edu.sg). Ulf Zölitz: University of Zurich, IZA, CESifo and CEPR (email: ulf.zoelitz@econ.uzh.ch).

1. Introduction

Personality predicts many important outcomes, including education, income, job satisfaction, health, risky behaviors, successful relations, and divorce (Heckman and Kautz 2012; Roberts et al. 2007). Personality is also a key element of human capital that the labor market increasingly values (Deming 2017; Edin et al. 2022). Given the importance of personality for individuals and society, it is surprising how little we know about causal determinants of personality.

In this paper, we study how peers shape personality. The omnipresence of peers makes it easy to imagine that they influence who we are. This idea is captured by group socialization theory stating that our personality is formed through efforts of fitting into a group and competing with others (Harris 1995). Although peers are promising and seemingly obvious candidates for explaining personality development, causal evidence on their influence is absent. The large literature on peer effects that is devoted to studying social spillovers has never directly investigated this question.

To study the impact of peers on personality development, we conduct a field experiment with 963 undergraduate students who we randomly assign to small study groups of four. In these groups, students solve problem sets, prepare tutorial sessions, discuss lectures, as well as meet for different social events. These social interactions take place during the first year at university, a formative period in which students adjust to a new environment, make new friends, and form new habits. The students in our sample are 18-22 years old, an age period where personality still displays substantial malleability (Robins et al. 2001; Caspi and Roberts 2001; Borghuis et al. 2017). We measure students' personality traits (openness, conscientiousness, extraversion, agreeableness, and neuroticism), with the commonly used Big Five taxonomy. We also measure students' competitiveness, which has recently emerged as an important predictor of education and labor market outcomes (Buser, Niederle, and Oosterbeek 2021). We measure these six traits at the start of the course before students were assigned to their study groups (baseline) and the end of the course, just before their final exams (endline). We then estimate how the personality of randomly assigned peers measured at the baseline affects student personality at the endline.

Our results show that students become more similar to their peers along several, but not all, personality dimensions. Being randomly assigned to peers who are one standard deviation (SD) more conscientious raises a student's own conscientiousness by 0.046 to 0.060 SD, depending on the specification. Being assigned to peers who are one SD more competitive

makes students approximately 0.055 SD more competitive. We also see that being assigned to peers who are one SD more open to new experiences raises a student's own openness by about 0.065 SD. By contrast, peer extraversion, agreeableness, or neuroticism do not statistically significantly affect a student's own level of the trait. We also find no evidence that peers' math ability affects any of the six personality traits, suggesting that peers' cognitive skills do not influence noncognitive skill development.

Are the personality spillovers we document driven by the personality of peers or other characteristics correlated with peer personality? It is hard to make this distinction because peer personality cannot be independently randomized from other peer characteristics. From a policy perspective, this distinction is less important. Knowing that exposure to conscientious peers increases students' conscientiousness is policy relevant, regardless of what drives these effects. In practice, we cannot assign students to more-conscientious peers without changing peer gender, achievement, and other unobserved peer characteristics correlated with conscientiousness. However, to be able to better place our findings in the academic literature, it is important to know whether peer personality is merely a proxy for other peer characteristics that have been shown to predict students' outcomes. We thus test whether controlling for peer gender, achievement, and a large set of other peer characteristics affects our results. It does not. Having peers with different personalities generates distinct social spillovers.

Our results raise the question of how persistent peer-induced personality changes are. We conduct follow-up surveys and measure personality traits one to four years after the end of the experiment. We find that the spillover effects for conscientiousness and competitiveness remain visible up to four years after the initial peer group assignment. The spillover for openness, however, appears to fade out. The persistent impact of peer conscientiousness and competitiveness suggests that spillovers in these traits go beyond short-term behavioral changes and represent longer-lasting trait changes.

Having established that peers affect personality development, we next investigate whether the three personality traits generating spillovers affect "hard" academic outcomes of other students. We find that exposure to conscientious and competitive peers improves student performance. A one SD increase in peer competitiveness and conscientiousness raises the obtained exam score by 0.075 and 0.052 SD. In comparison, a one SD increase in peers' math

ability raises exam performance by 0.048 SD. These findings suggest that peers with productive noncognitive skills can be as valuable as high-achieving peers.¹

Why do peers affect only some personality traits but leave others unaffected? We first test whether peer personality affects the frequency of peer-to-peer interactions. We find no evidence that peers' competitiveness, openness, or conscientiousness affects the intensity of academic or social interaction. This suggests that the trait-specific personality spillovers are unlikely driven by the intensity of exposure itself. Next, we test how personality traits relate to academic achievement and find that personality spillovers are only visible for traits that predict educational success. This is consistent with the idea that people engage in self-regulated, effortful personality change (Hennecke et al. 2014; Stieger et al. 2021), and that students only adopt peer traits that are productive in the university context.²

The large literature on peer effects has studied how peers' gender, race, or achievement affect performance and educational choices.³ Only a few recent papers have explored peer personality as an input in the education production function. These studies show that peer personality affects students' performance. Shure (2021) shows that having more conscientious peers raises math and language performance in high school. Hancock and Hill (2021) show that teammate conscientiousness raises team performance in university study groups. Golsteyn, Non, and Zölitz (2021) show that exposure to peers who are more persistent raises university performance. Only one other peer effects paper looks at an outcome related to personality. Using the project STAR data, Bietenbeck (2021) finds that having more motivated peers, while increasing reading test scores, has no significant impact on own motivation. Bietenbeck (2021) studies these effects in the primary school classroom. In contrast, we study peer effects in small university peer groups using six validated personality measures.

Our work relates to several studies that also conduct experiments to study peer effects (Booij, Leuven, and Oosterbeek 2017; Carrell, Sacerdote, and West 2013; Duflo, Dupas, and

¹ We also find that conscientious peers lower students' self-reported anxiety levels at the end of the semester. A one SD increase in peer conscientiousness lowers anxiety by 0.075 SD. This suggests that the effect of peer personality may go beyond personality and academic performance.

² We formalize this idea with a simple theoretical framework in Appendix B. In our framework, students adopt traits when the academic returns exceed the costs. The costs of personality change depend on peers who act as role models or apply social pressure.

³ For example, Hoxby (2000) shows that having more female peers raises both boys' and girls' test scores. Carrell, Sacerdote, and West (2013) highlight that low-achieving students perform worse when medium-achieving peers are replaced with high-achieving peers. Consistent with these results, Booij, Leuven, and Oosterbeek (2017) show that low- and medium-achieving students benefit from tracking of the same type of students. Figlio (2007) shows that boys with female-sounding names have more behavioral problems and a negative impact on their peers' test scores. Carrell, Hoekstra, and Kuka (2018) show that having disruptive peers reduces earnings by 3–4%. Sacerdote (2014) provides an excellent review of the existing literature on peer effects.

[Kremer 2011](#); [Oosterbeek and Van Ewijk 2014](#)). While these studies provide important insights into the nature of peer effects, they focus on performance and do not consider personality as an input or output.

This paper underlines the malleability of personality in adolescence and therefore also relates to a series of recent papers showing that targeted interventions can impact socioemotional skills ([Alan, Boneva, and Ertac 2019](#); [Abeler, Falk, and Kosse 2021](#); [Alan et al., 2021](#); [Kosse et al. 2020](#); [Sorrenti et al. 2020](#); [Roberts et al. 2017](#)). Our paper contributes to this literature by highlighting that natural exposure to peers can have lasting effects on noncognitive skills. This finding suggests that interventions improving noncognitive skills may have a multiplier effect through peer-to-peer spillovers. [List, Momeni, and Zenou \(2020\)](#) provide evidence along these lines and show that an early childhood intervention affects both treated and untreated children's noncognitive skills. Given these findings and our results, we note that policymakers and researchers might be underestimating the social returns of programs that boost noncognitive skills.

Our paper also has implications for group composition, neighborhood sorting, and peer choice in general. Prior studies have recognized peer effects as underlying mechanisms of the impacts of schools and neighborhoods on socioeconomic outcomes ([Jackson 2010](#); [Deming et al. 2014](#); [Chetty, Hendren, and Katz 2016](#); [Deutscher 2020](#)). Peers are also an important factor in families' school and neighborhood choices ([Nechyba 2006](#); [Barseghyan, Clark, and Coate 2019](#)). Compared to cognitive ability, demographics, and socioeconomic status, noncognitive skills have been largely neglected. We highlight the value of peers who are equipped with better noncognitive skills and establish a novel fact: peers influence students' personality development.

2. What is Personality and How Much Does It Change?

The American Psychological Association defines personality as “individual differences in characteristic patterns of thinking, feeling and behaving.” [Heckman and Kautz \(2014, page 346\)](#) define personality traits as “personal attributes not thought to be measured by IQ tests or achievement tests.” Both of these definitions are broad and include socioemotional skills, soft skills, and what economists refer to as “noncognitive” skills. Differentiating personality traits from behaviors, habits, or feelings is empirically not possible ([Borghans et al., 2011](#)). Because traits can only be inferred from their consequences such as observable behavior, there can be no direct measurement of an abstract trait. In this paper, we adopt the pragmatic definition by

[Pervin \(1994\)](#), who defines personality traits as *observable* patterns of habits and behaviors that make a person unique. Because personality is always measured through questions about behavior, lasting changes in these behaviors represent changes in personality by definition.

What do we know about changes in personality? While there is a misconception among some economists that personality is fixed, a large literature in psychology has shown that personality is both malleable and reasonably stable over time ([Roberts et al. 2001](#); [Roberts et al. 2006](#); [Borghuis et al. 2017](#)). Within economics, a series of recent intervention studies has provided evidence on the malleability of personality during childhood. [Kosse et al. \(2020\)](#) show that children participating in the German “Balu und Du” mentoring program become more prosocial. [Abeler, Falk, and Kosse \(2021\)](#) show that these children also become more honest, and [Boneva et al. \(2021\)](#) show they become more competitive. In related work, [Alan and Ertac \(2018\)](#) show that children receiving a classroom-based intervention become more patient, and [Alan, Boneva, and Ertac \(2019\)](#) show that grit can be fostered through interventions. [Alan et al. \(2021\)](#) show that an intervention in perspective-taking increases prosocial behavior. [Cappelen et al. \(2020\)](#) show that early childhood education affects children’s social preferences for fairness and the importance children place on efficiency relative to fairness. [Sorrenti et al. \(2020\)](#) show that a socioemotional skills intervention persistently reduces children’s impulsiveness and disruptiveness. In related work, [Fryer, Levitt, and List \(2015\)](#) show that incentivized parental engagement can improve children’s cognitive and noncognitive scores.

We know substantially less about factors that affect personality in adolescence and adulthood. [Dahmann and Anger \(2018\)](#) study a German schooling reform and find that a one-year reduction in years of schooling increases openness and decreases emotional stability. Only two studies have investigated how schools and teachers affect outcomes related to personality. [Jackson \(2018\)](#) and [Jackson et al. \(2020\)](#) show that both schools and teachers shape socio-emotional development and noncognitive outcomes like absences, suspensions, and on-time grade progression. A more recent work by [Joensen et al. \(2022\)](#) shows that targeted interventions in high school can affect both the levels of skill and the technology of skill formation. These results suggest that adolescence can be a formative period for socio-emotional skills.

Recent evidence further suggests that people can change their own personality. [Stieger et al. \(2021\)](#) show that access to a virtual personality coach can help people to persistently change their personality in the desired direction. Experimental participants who had access to such a coach became more conscientious, less neurotic, and more extraverted. [Hennecke et al. \(2014\)](#) propose a framework for self-directed personality development and three necessary

conditions to change personality. Based on this framework, people can change their personality if they (1) feel such a change is desirable or necessary, (2) consider the change to be feasible, and (3) make a habit of the initial changes.

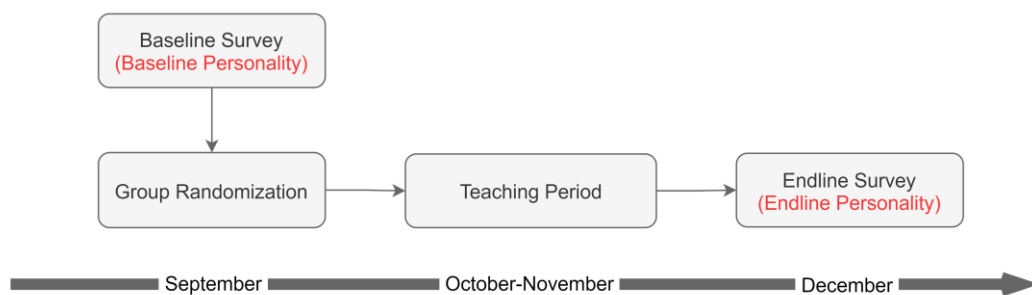
3. Experimental Design

3.1. The Experiment

Our experiment took place in an introductory economics course that is mandatory for all students enrolled in a major or minor in economics, business, or informatics. We conducted the same experiment in the Fall semester for five academic years, from 2018/2019 to 2022/2023. Students who participated in our experiment were mostly in their first semester and aged 18 to 22 years—a formative period for personality changes (Robins et al. 2001; Caspi and Roberts 2001; Borghuis et al. 2017).

The structure of the course was identical for each of the five student cohorts. Students attended two lectures and one tutorial session per week and completed weekly ungraded problem sets. To pass the course, students needed to pass their final exam, which was the only determinant of their grade. The stakes for this exam were high. If a student failed the exam, it could only be retaken once. Failing the exam for a second time meant the student would have to change majors or drop out of the university.

Figure 1: Timeline of the Experiment



Notes: We conducted the same experiment among five cohorts from 2018/19 to 2022/23. The experiment timeline has remained the same across the years.

Figure 1 shows the timeline of our experiment. Before the start of the term, students received an invitation to complete a baseline survey. This survey contained measures of students' personality as well as other demographic and background questions. In the first lecture, we informed students about the possibility of signing up for study groups. While signing up had no direct effect on students' grades, we suggested that they might enjoy studying

with other students. Overall, 42% of all students in the course signed up for study groups. We think these students, who are willing to interact with new peers are arguably a more relevant sample to examine the effect of peer environment on personality development (relative to students who prefer to study by themselves or interact with people out of the social environment being examined). While voluntary participation does not affect our internal validity, it might affect the generalizability of our results. Because we also surveyed students not registered for study groups, we are able to compare the baseline characteristics by the group registration status. As Table A1 in the Appendix shows, students who signed up were slightly more open to new experiences and were less conscientious. Along all other dimensions (competitiveness, agreeableness, neuroticism, high school grades, and gender), participating students were not significantly different from other students (p -value > 0.05). In Section 7 we provide a thorough discussion on the external validity of our results following the framework of [List \(2020\)](#). Altogether, the evidence suggests that our findings apply to a more general student population.

We assigned students to study groups as follows. In each year, we grouped students into three study programs depending on whether they were enrolled in: 1) a business or economics major, 2) an informatics major, or 3) any other major with a business or economics minor.⁴ Students who are enrolled in the same broad program typically follow the same curriculum. Within the program, we randomly assigned each student to a study group consisting of four students. Our study group assignment is therefore stratified at the program–cohort level.

We informed students about their study group via email. This email included the names, email addresses, and mobile phone numbers of their study group peers, invited them to create a WhatsApp group, and suggested that they schedule their first group meeting.⁵ To foster social interactions, we also offered each group a \$20 voucher for drinks at the local university bar. Once students were assigned to groups, participation in group activities was voluntary. Although study groups were designed for studying economics, group members could also study together for other common courses or organize social activities. At the end of the semester, before the final exam, students received the endline survey. This survey elicited students' endline personality traits as well as information about academic and social interactions with their peers.

⁴ 65.3% were majoring in business or economics, 15.7% were majoring in informatics, and the remaining 19% were majoring in another subject but were enrolled in a business or economics minor.

⁵ According to our pre-experiment survey, more than 90% of students use WhatsApp to communicate with friends, which makes it a convenient tool to facilitate peer group interaction.

3.2. Data

We measure students' personality in the baseline survey and in the endline survey. Table 1 provides an overview of the included personality traits, the items used to measure them, and their answer scales.

We measure the Big Five personality traits (openness, conscientiousness, extraversion, agreeableness, and neuroticism) with the 15-item Big Five Inventory (BFI-S), which consists of three items per trait (Gerlitz and Schupp 2005). This inventory is a short version of the original 44-item Big Five Inventory (John et al. 1991) and has been validated and used in different settings (Specht et al. 2011; Lang et al. 2011; Hahn et al. 2012). Students rate each item on a 7-point scale ranging from 1 = “does not apply to me at all” to 7 = “applies to me perfectly.” Following Gerlitz and Schupp (2005), we measure each personality trait as the average of students' ratings on the three designated items. To measure students' competitiveness, we use the one-item measure proposed and validated by Buser, Niederle, and Oosterbeek (2021): “In general, how competitive do you consider yourself?” Students answer this question on a 10-point scale ranging from 1 = “not competitive at all” to 10 = “very competitive.”

Table 1: Measurement of Personality Traits (C-OCEAN)

Personality Trait	Question and Scale
Competitiveness	In general, how competitive do you consider yourself? <i>Scale: 0 (not competitive at all) to 10 (very competitive)</i>
	I see myself as someone who . . . <i>Scale: 1 (does not apply to me at all) to 7 (applies to me perfectly)</i>
Openness	is original, comes up with new ideas values artistic, aesthetic experiences has an active imagination
Conscientiousness	does a thorough job tends to be lazy [reversed] does things effectively and efficiently
Extraversion	is communicative, talkative is outgoing, sociable is reserved [reversed]
Agreeableness	is sometimes somewhat rude to others [reversed] has a forgiving nature is considerate and kind to others
Neuroticism	worries a lot gets nervous easily is relaxed, handles stress well [reversed]

Notes: Students could choose to fill out the survey in German or English. More than 80% of students answered the survey in German. The German version of the 15-item Big Five personality traits was taken from the German

Socio-Economic Panel (G-SOEP). The single-item competitiveness scale is based on an early version of [Buser, Niederle, and Oosterbeek \(2021\)](#).

Our analyses focus on 340 study groups for which we observe all group members' baseline personality traits.⁶ Among the 1,360 students in these groups, 963 students (71%) completed the endline survey and make up our final estimation sample. Appendix Table A2, Panel B, shows that peer personality at the baseline does not significantly affect the response rate of the endline survey. Appendix Table A8 further shows that baseline characteristics, including gender, high school grades, and personality traits, are balanced between the baseline sample of 1,360 students and the final estimation sample of 963 students.

Table 2, Panel A, shows descriptive statistics for our estimation sample. 46.5% of the 963 students are female, approximately 10% of the students are taking the course for the second time, and 86% attended a high school with German as the instruction language.⁷ Students also reported their high school final grades in math and the first language using the typical Swiss grading scale, from point 1 to point 6 (higher values indicate better performance).

Panel B shows the summary statistics of personality traits at baseline and endline. From the beginning to the end of the first semester, we only see small changes in students' personality: the average student in our sample becomes more open to new experiences, slightly more extraverted, less competitive, less conscientious, less agreeable, and more neurotic. Appendix Figure A1 plots the distribution of the six personality traits at the baseline and endline.

All six personality traits are empirically distinct from each other. Appendix Table A3 shows that the pairwise correlations between personality traits at the baseline never exceed 0.3 in absolute terms. Table A4 provides evidence on the reliability of our Big Five measures. The Cronbach's Alpha coefficients range from 0.46 to 0.85 and largely support the internal consistency of our personality measures. Table A5 provides evidence on the stability of personality over time. The within-trait correlations over time range between 0.62 and 0.80, which is reasonably stable and close to what typically is found in other studies over similar time horizons ([Robins et al. 2001](#); [Terracciano, McCrae, and Costa 2010](#)).

Table 2, Panel C, shows two indicators of student performance: exam attendance and exam scores. The final grade for the course is entirely based on student performance in the final exam. 97.2% of students attended the final exam, and conditional on taking the exam, the

⁶ We do not include study groups with incomplete baseline personality measures because missing peer data can lead to a large bias in peer-effect estimates ([Sojourner 2013](#)).

⁷ The instruction language in our setting is German. We coded all our survey questions in both German and English so that students could freely choose the language throughout the survey.

average score obtained was 60 on a scale of 0 to 120. Considering the disruptive and stressful impacts of the COVID-19 pandemic, we also elicited students' mental health in the endline survey from year 2020/21. The anxiety score summarized in Panel C is based on students' answers to two questions from the Patient Health Questionnaire: *“Over the last two weeks, how often have you been bothered by the following problems? (1) Feeling nervous, anxious or on edge; (2) Not being able to stop or control worrying.”* Each question is answered on the following scale: 0 (“Not at all”), 1 (“Several days”), 2 (“More than half of the days”), and 3 (“Nearly every day”). Adding together the two answers gives the total anxiety score. The average anxiety score is 2.6, and strikingly, 45% of the students reported a score of at least 3, which suggests anxiety.

Table 2: Descriptive Statistics

	(1) N	(2) mean	(3) sd	(4) min	(5) max
Panel A: Background Characteristics					
Female	963	0.465	0.499	0	1
Retaking the Course	963	0.100	0.300	0	1
High School Math Grade	963	4.606	0.795	1	6
High School Language Grade	963	4.749	0.600	1	6
High School First Language Was German	963	0.864	0.343	0	1
Panel B: Personality Traits					
Baseline Competitiveness	963	6.896	1.893	1	10
Endline Competitiveness	963	6.603	2.077	1	10
Baseline Openness	963	4.842	1.116	1.333	7
Endline Openness	963	4.904	1.159	1.333	7
Baseline Conscientiousness	963	4.826	0.971	1.333	7
Endline Conscientiousness	963	4.643	1.045	1.667	7
Baseline Extraversion	963	4.769	1.222	1	7
Endline Extraversion	963	4.782	1.254	1	7
Baseline Agreeableness	963	5.530	0.862	2.667	7
Endline Agreeableness	963	5.341	0.913	2.333	7
Baseline Neuroticism	963	4.071	1.300	1	7
Endline Neuroticism	963	4.213	1.344	1	7
Panel C: Performance Indicators					
Exam Attendance	963	0.972	0.165	0	1
Exam Score	936	60.21	20.76	1	118
Anxiety Score	696	2.636	1.715	0	6

Notes: This table is based on our estimation sample. The standard deviation is denoted by “sd.” The sample size is smaller for exam score and anxiety score in Panel C because the exam score is conditional on taking the final exam, and the anxiety score is available for the 2020/21 to 2022/23 cohorts.

4. Empirical Strategy

Our aim is to estimate how peer personality measured at the baseline affects students' own personality at the endline. We estimate the following model separately for each personality trait T :

$$T_{ig,t} = \alpha + \beta \bar{T}_{-ig,t-1} + \gamma X_{ig,t-1} + \varepsilon_{igt}, \quad (1)$$

where $T_{ig,t}$ is the trait level of student i in study group g measured in the endline survey at time t . $\bar{T}_{-ig,t-1}$ is the average trait level of students in group g (excluding student i) measured in the baseline survey at time $t-1$. In our main analysis, we estimate Equation (1) separately for each personality trait. For example, we estimate the effect of having peers who are more open on students' own openness.

The vector $X_{ig,t-1}$ includes control variables that differ by specification. In all specifications, $X_{ig,t-1}$ includes *randomization controls*, which are study program dummies (business or economics majors, informatics majors, base group: other majors), student cohort dummies, and for the 2020/2021 cohort, nine dummies for the last digit of students' ID numbers. The latter set of dummies accounts for the fact that for this cohort, the university assigned students to online or in-person lectures based on their student ID to facilitate social distancing during the COVID-19 pandemic. In all specifications, $X_{ig,t-1}$ also includes the baseline level of the personality trait in question. For example, we control for students' own openness at the baseline when our dependent variable is students' openness at the endline. This control implies that our estimates are econometrically equivalent to using the change in personality between the baseline and endline as a dependent variable.

We estimate additional specifications where $X_{ig,t-1}$ includes students' other personality traits measured at the baseline (*other own personality traits*), student's gender, age, fixed effects for business-economics majors, high school math and language grades, and an indicator for whether German was the instruction language in high school (*other own characteristics*), as well as peer averages of these variables (*other peer personality traits* and *other peer characteristics*). To facilitate the interpretation of our estimates, we standardize each personality trait to have a mean of zero and a standard deviation of one across the estimation sample. The peer averages reported in our regression tables are based on those standardized personality measures.

The key identifying assumption for our approach is that the randomization of students into study groups was successful. To confirm that this is the case, we test how peer personality

measured at the baseline relates to students' own personality measured at the *baseline*, that is, before peer composition it could have affected it. We implement this test by estimating Equation (1) but use as the dependent variable students' baseline personality instead of their endline personality. Besides randomization controls, these regressions include a leave-out-mean of each personality trait at the program-by-cohort level to account for the mechanical relationship between own and peer characteristics (see [Guryan, Kroft, and Notowidigdo 2009](#)).

Table 3 shows that peer personality at the baseline does not significantly predict students' own personality at baseline. All six estimates of interest are small in magnitude, and none are statistically significant. Table A6 in the Appendix provides an alternative balancing check in which we test whether study group dummies jointly predict students' baseline characteristics. This is not the case. Both tests confirm that our randomization of students into study groups was successful.

Table 3: Randomization Test

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Own Personality Traits at Baseline					
	Competitiveness	Openness	Conscientiousness	Extraversion	Agreeableness	Neuroticism
Peer Competitiveness	-0.012 (0.043)					
Peer Openness		-0.026 (0.050)				
Peer Conscientiousness			-0.081 (0.050)			
Peer Extraversion				-0.033 (0.049)		
Peer Agreeableness					0.002 (0.045)	
Peer Neuroticism						0.015 (0.050)
Observations	963	963	963	963	963	963
<i>R</i> -squared	0.444	0.450	0.497	0.497	0.526	0.447

Notes: Each column represents one OLS regression. All regressions control for study program-by-cohort fixed effects and the leave-out-mean of each personality trait at the program-by-cohort level. All dependent and independent variables are standardized. Robust standard errors clustered at the study group level are in parentheses. Panel A of Appendix Table A2 shows the balancing results using the full baseline sample (1,360 students in 340 groups).

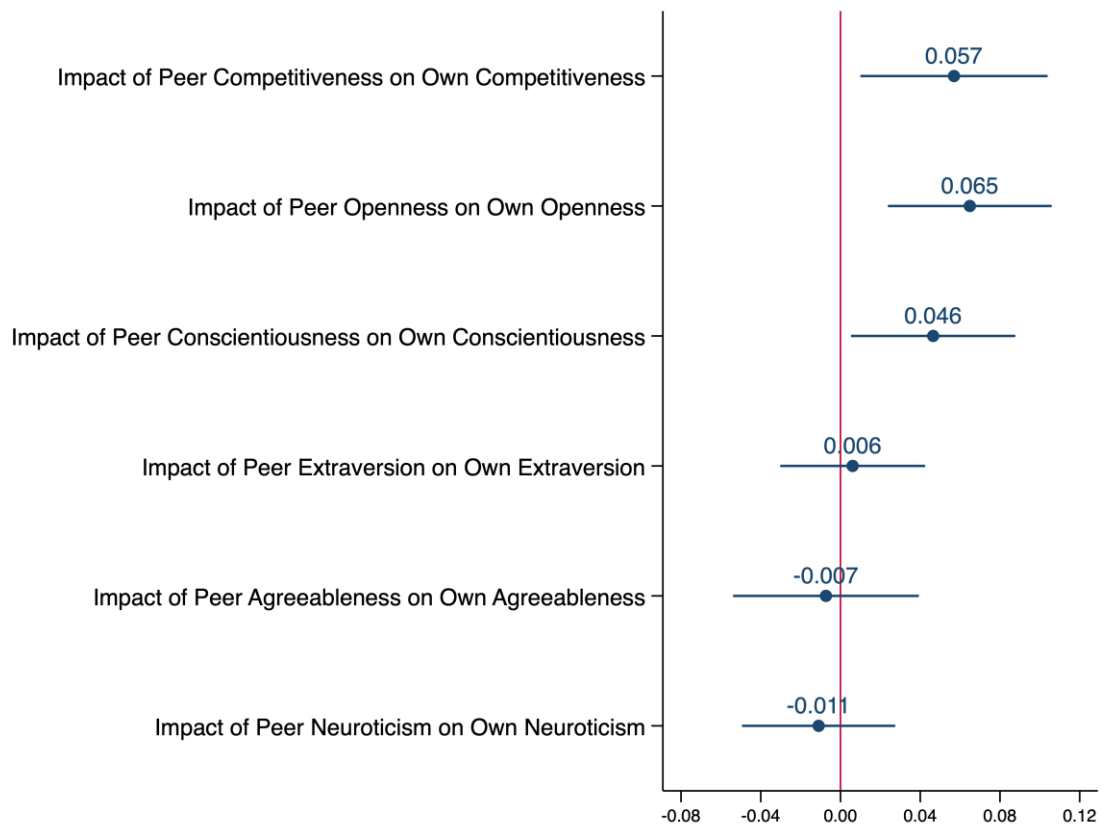
5. Results

5.1. Personality Spillovers

Figure 2 shows that students become more similar to their randomly assigned peers for three out of six personality traits. Being assigned to peers who are one SD more competitive increases students' own competitiveness by 0.057 SD (p -value = 0.018). Being assigned to

peers who are one SD more open to new experiences increases students' own openness by 0.065 SD (p -value = 0.002). Similarly, being assigned to peers who are one SD more conscientious increases students' own conscientiousness by 0.046 SD (p -value = 0.028). We find no evidence that being assigned to more extraverted, agreeable, or neurotic peers affects students' own personality in these dimensions.

Figure 2: The Impact of Peer Personality at Baseline on Own Personality at Endline



Notes: Each point estimate represents one OLS regression as specified in column (1) of Table 4. For each regression, the outcome variable is a standardized own personality trait at the endline, the independent variable of interest is a standardized peer personality trait at the baseline, and the control variables include randomization controls as defined in the empirical strategy and students' own personality trait at the baseline (i.e., the baseline level of the dependent variable). Standard errors are clustered at the study group level. Error bars indicate 95% confidence intervals.

How large are these effects in their original scales? In terms of the 10-point scale used to measure competitiveness, being assigned to peers who are, on average, one point more competitive increases students' own competitiveness by 0.106 points. In terms of the 7-point scale used to measure the Big Five personality traits, we see that being assigned to three peers who are one point more open increases own openness by 0.121 points, and being assigned to

peers who are one point more conscientious increases own conscientiousness by 0.091 points. These effects are modest but economically meaningful.

We assess the robustness of these results by gradually adding student- and peer-level control variables. Column (1) of Table 4 shows our baseline estimates from Figure 2 for reference. Column (2) shows estimates from regressions in which we include all own personality variables in the model, and in column (3), we additionally include all student background variables. As expected under random assignment, including these variables does not affect our point estimates in any meaningful way.

Table 4: Peer Personality and Own Personality

	(1)	(2)	(3)	(4)	(5)
Panel A: DV = Std. Own Competitiveness at Endline					
Std. Peer Competitiveness	0.057** (0.024) [0.018]	0.056** (0.023) [0.017]	0.052** (0.023) [0.026]	0.053** (0.025) [0.030]	0.056** (0.025) [0.024]
Panel B: DV = Std. Own Openness at Endline					
Std. Peer Openness	0.065*** (0.021) [0.002]	0.067*** (0.021) [0.001]	0.067*** (0.021) [0.002]	0.072*** (0.022) [0.001]	0.068*** (0.022) [0.002]
Panel C: DV = Std. Own Conscientiousness at Endline					
Std. Peer Conscientiousness	0.046** (0.021) [0.027]	0.043** (0.021) [0.042]	0.046** (0.021) [0.029]	0.059** (0.023) [0.012]	0.060** (0.025) [0.018]
Panel D: DV = Std. Own Extraversion at Endline					
Std. Peer Extraversion	0.006 (0.018) [0.741]	0.005 (0.019) [0.803]	0.002 (0.018) [0.902]	0.005 (0.020) [0.808]	0.006 (0.020) [0.771]
Panel E: DV = Std. Own Agreeableness at Endline					
Std. Peer Agreeableness	-0.007 (0.024) [0.758]	-0.007 (0.024) [0.764]	-0.002 (0.024) [0.922]	0.006 (0.024) [0.789]	0.006 (0.024) [0.797]
Panel F: DV = Std. Own Neuroticism at Endline					
Std. Peer Neuroticism	-0.011 (0.020) [0.575]	-0.011 (0.020) [0.572]	-0.011 (0.020) [0.563]	-0.018 (0.021) [0.379]	-0.021 (0.021) [0.335]
Observations	963	963	963	963	963
Control Variables:					
Other own personality traits	N	Y	Y	Y	Y
Other own characteristics	N	N	Y	Y	Y
Other peer personality traits	N	N	N	Y	Y
Other peer characteristics	N	N	N	N	Y

Notes: Each column in each panel shows one coefficient from a separate OLS regression. All regressions include the baseline level of the dependent variable as well as randomization controls as defined in the empirical strategy. *Other own personality traits* include five other personality traits at the baseline, except for the one trait being

examined in each panel. *Other own characteristics* include gender, business-economics major fixed effects, high school math and language grades, an indicator for German as the high school instruction language, course-retaking status, and age fixed effects. *Other peer personality traits* are five other peer personality traits at the baseline, except for the one being analyzed. *Other peer characteristics* include peers' gender, high school math and language grades, and whether German was the instruction language in high school. All dependent and independent variables are standardized. Robust standard errors clustered at the study group level are in parentheses. p -values are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Are these effects driven by peer personality? Or, do they show effects of characteristics correlated with peer personality? These questions are difficult to answer because we cannot randomly assign peer personality independent of other peer attributes. For policymakers, understanding what drives our observed effects is also less important. They can, for example, increase students' competitiveness by assigning them to more competitive peers *regardless of what drives these effects*.

To be able to better place our results in the literature, it is nevertheless important to know whether our peer personality spillovers merely capture effects associated with other peer variables, for example, peer gender and peer achievement, which are the two peer characteristics most frequently studied in the literature. We therefore test how our point estimates are affected by including other peer variables. In column (4) of Table 4, we include all peer personality variables in the model, and in column (5), we include peer gender, high school math and language grades, and whether German was the high school instruction language. Our results remain very similar after including these controls. This suggests that our measures of peer personality capture distinct components of students' personality traits. Column (5) shows that peer conscientiousness increases own conscientiousness (p -value = 0.018), peer competitiveness increases own competitiveness (p -value = 0.024), and peer openness increases own openness (p -value = 0.002). Taken together, our estimates in Table 4 show that the impact of peer personality remains remarkably robust and is not sensitive to the inclusion of student- or peer-level control variables.

While our conclusions remain the same, it is not clear whether controlling for other peer characteristics is a good robustness check. The estimates from specifications that include these controls do not deliver policy-relevant parameters. In practice, we cannot assign students to more-conscientious peers without changing peer gender, achievement, or other correlated unobserved peer characteristics.

Although this paper focuses on within-trait personality spillovers, for completeness, we also test whether spillovers exist across traits. Appendix Table A9 provides a "full spillover matrix" and shows estimates for how each peer trait affects the six own traits. We find that

personality spillovers are highly concentrated on the diagonal line, where a given own trait is regressed on the same peer trait. Only two out of the other 30 coefficients reach significance at the 5% level, which could represent chance findings. Table A9 also shows that peers' math achievement does not significantly affect any of the six personality traits, suggesting that peers' cognitive skills do not create spillover effects on students' own noncognitive skills.

Measurement Error: One might be concerned that measurement error in the peer personality variables drives our results. If measurement error is random, this should not be a concern. [Feld and Zölitz \(2017\)](#) show that random measurement error will attenuate peer effect estimates in settings with random assignment. If peer personality measures true personality with random error, we would therefore expect our results to be a lower bound of the true effect.

To quantify bias from measurement error, we use the *obviously related instrumental variables* (ORIV) approach by [Gillen, Snowberg, and Yariv \(2019\)](#) and exploit the fact that we have three items for each Big Five personality trait. Because competitiveness is measured with only one question, we cannot apply the ORIV approach to correct for measurement errors in competitiveness. For all other measures, we use each item as an instrumental variable for the other two items and re-estimate the effect of peer personality on own personality using two-stage least-squares (2SLS) regressions. As expected, this procedure leads to larger point estimates in absolute terms (see Appendix Figure A3). The estimated effect of a one SD increase in peer openness on students' own openness is 0.118 SD (compared to a 0.065 SD in our main specification). The estimated effect of a one SD increase in peer conscientiousness on own conscientiousness is 0.103 SD (compared to 0.046 SD in our main specification). The estimates are statistically significant at the 1% and 5% levels, respectively.

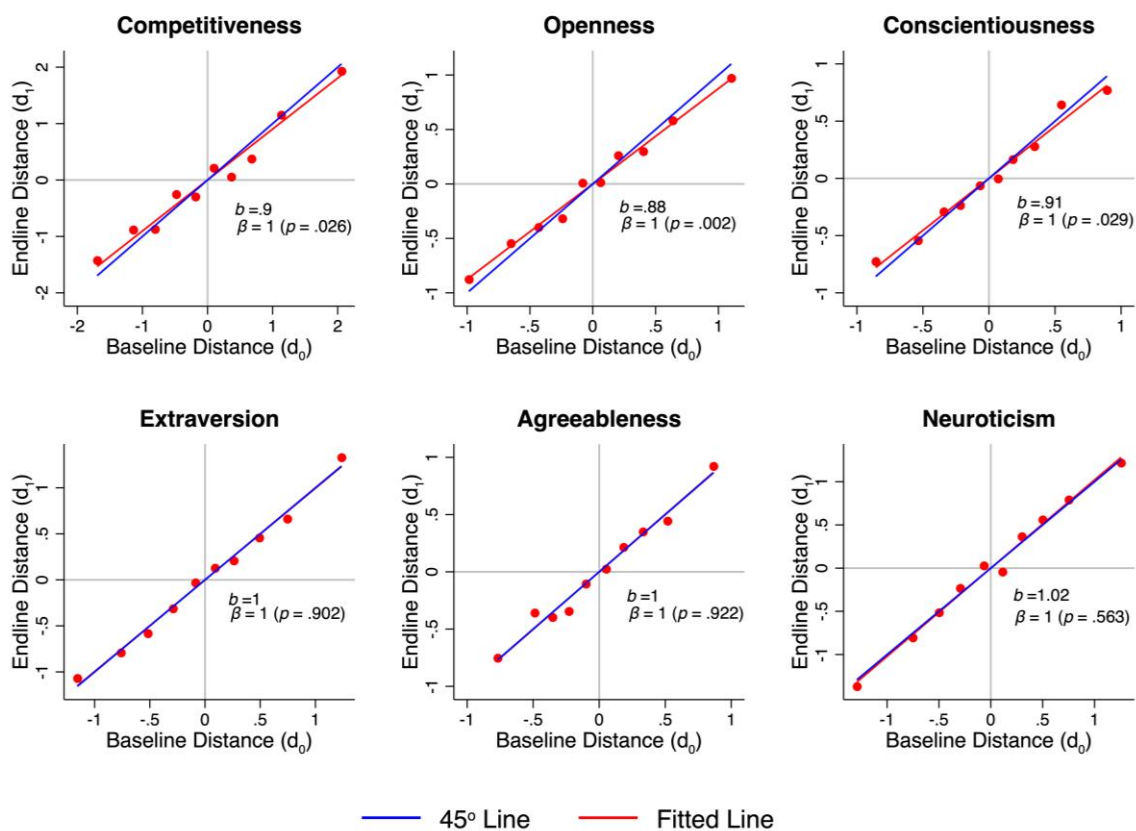
5.2 Convergence of Personality

Our analyses reveal spillovers in competitiveness, openness, and conscientiousness: when exposed to peers with higher (lower) levels of a trait, own levels of the trait at the endline increase (decrease). An alternative interpretation of these positive spillover effects is that students' own personality converges to peer personality. Put differently, students become more similar to their peers along these dimensions of personality.

To better illustrate the patterns of personality convergence, we calculate the distance between own personality T_0^o and peer personality T_0^p at baseline for each trait ($d_0 = T_0^o - T_0^p$), and similarly, the distance between own personality at endline T_1^o and peers' baseline personality T_0^p ($d_1 = T_1^o - T_0^p$). If students do converge to peers in terms of a trait, the endline

distance should move closer to zero compared to the baseline distance. Except in the case of overshooting, the two distances should also have the same sign. In Figure 3, we plot d_1 against d_0 separately for each trait. The results show that along the dimensions of competitiveness, openness, and conscientiousness, students do move closer to T_0^p , the reference points set by their peers at baseline. For these three traits, the endline distance is significantly smaller than the baseline distance ($d_1 < d_0$). By contrast, for the other three traits, the endline and baseline distances are not significantly different. If anything, we find suggestive evidence of divergence along the dimension of neuroticism ($d_1 > d_0$).⁸

Figure 3: Convergence of Personality



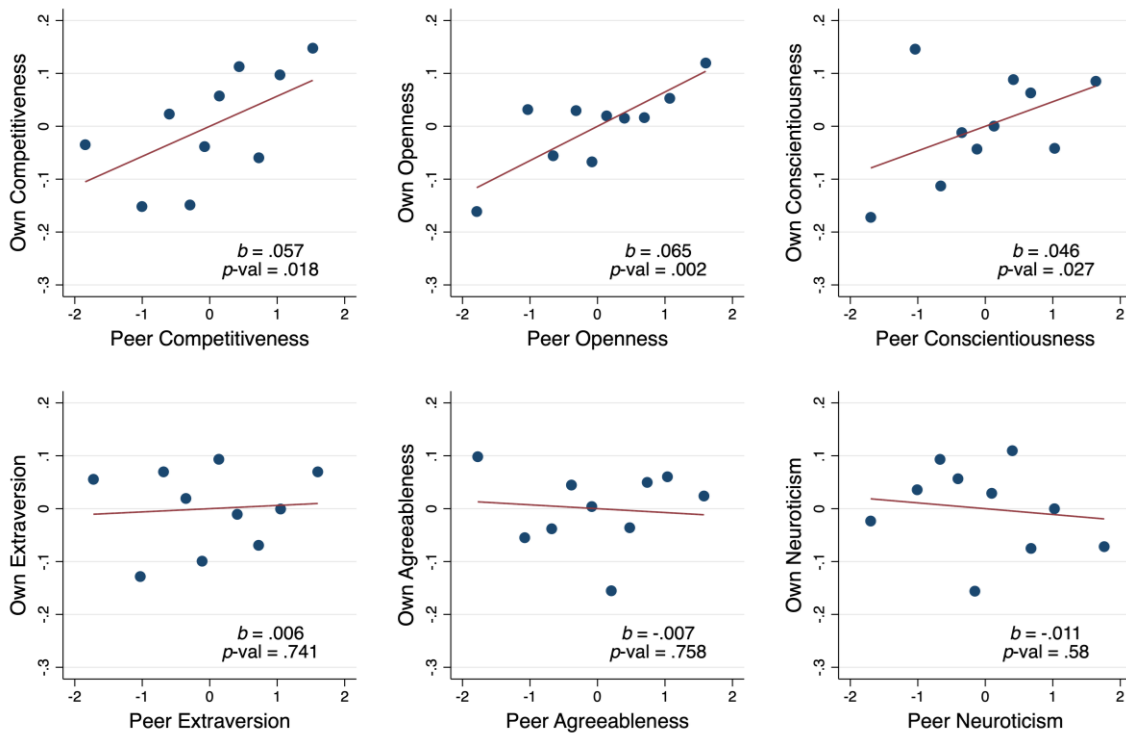
Notes: Binned scatter plots showing the relationship between the endline and baseline distance. The baseline distance (d_0) is the difference between a student's own level of a personality trait and peers' level of this trait at the baseline. The endline distance (d_1) is the difference between a student's own level at the endline and peers' level at the baseline. All baseline and endline distances are the residuals after controlling for individual characteristics observed at the baseline. The 45° line is a reference line representing the case of no convergence to peer personality: $d_1 = d_0$. The graph also plots the fitted linear lines and the estimated slopes (b). We also test whether the estimated slopes are significantly different from one ($H_0: \beta = 1$) and show the p -values for the tests.

⁸ The test of convergence is in essence equal to the test of spillover effects. Specifically, we estimate the spillover effect with the following specification: $T_1^o = \beta_0 + \beta_1 T_0^p + \beta_2 T_0^o + \varepsilon$, which is a linear transformation of the alternative specification: $T_1^o - T_0^p = \delta_0 + \delta_1 (T_0^o - T_0^p) + \delta_2 T_0^o + \eta$. This means that identifying $\beta_1 > 0$ (positive spillover) is equal to identifying $\delta_1 < 1$ (convergence).

5.3 Heterogeneity in Spillover Effects

In this section, we examine whether the spillovers are driven by certain types of students. We first use binned scatter plots to show how own personality varies with peer personality in Figure 4. The figure shows that spillover effects in competitiveness, openness, and conscientiousness are fairly linear. Furthermore, we separately estimate the impact of peer personality for students with above- and below-median values of a given trait at baseline. Results in Appendix Figure A4 show that the impact does not significantly vary with students' own personality. Given the lack of evidence on heterogeneity, we think the linear-in-means model is a good approximation to describe spillovers in our setting.

Figure 4: Non-Linear Spillovers?



Notes: Binned scatter plots showing the relationship between own personality traits at endline (y-axis) and peers' personality traits at baseline (x-axis). All measurements of own and peer personality traits are standardized. Each plot uses the same specification as in column (3) of Table 4.

5.4 Persistence of Personality Changes

We have shown that peers affected students' personality at the time of the endline survey, which was three months after the study group assignment. It is not clear what happens after the course is over. Personality peer effects may fade out and students may revert to their old selves once they are no longer exposed to their study group peers, or peer effects may persist if

students have formed new habits or continue to interact with their peers after the end of the course.

To provide evidence on the persistence of peer personality effects, we conducted two follow-up surveys in the summer of 2021 and 2022 to measure the personality of students from the first four cohorts (2018/19 to 2021/22). Depending on their cohort, students were surveyed one to four years after the end of the experiment. Of the 748 students from these four cohorts, 487 students (65%) completed the follow-up survey. Reassuringly, peer traits generating spillovers are unrelated to the probability of responding to this survey.⁹

Figure 5 shows how peer personality at baseline relates to own personality at the baseline, endline, and follow-up. For ease of comparison, we use raw scores of personality outcomes (instead of standardized measures) on a scale of 1 to 7 or 1 to 10. The baseline estimates in Figure 5 restate that peers' and own traits are not significantly correlated, confirming that peers are randomly assigned to study groups. We then re-estimate our main results from column (5) of Table 4 for the sample of 748 students. The point estimates are different because we use raw outcome measures—when using standardized measures, the estimates of endline spillovers are comparable to the overall results in Table 4 but slightly larger in magnitude.

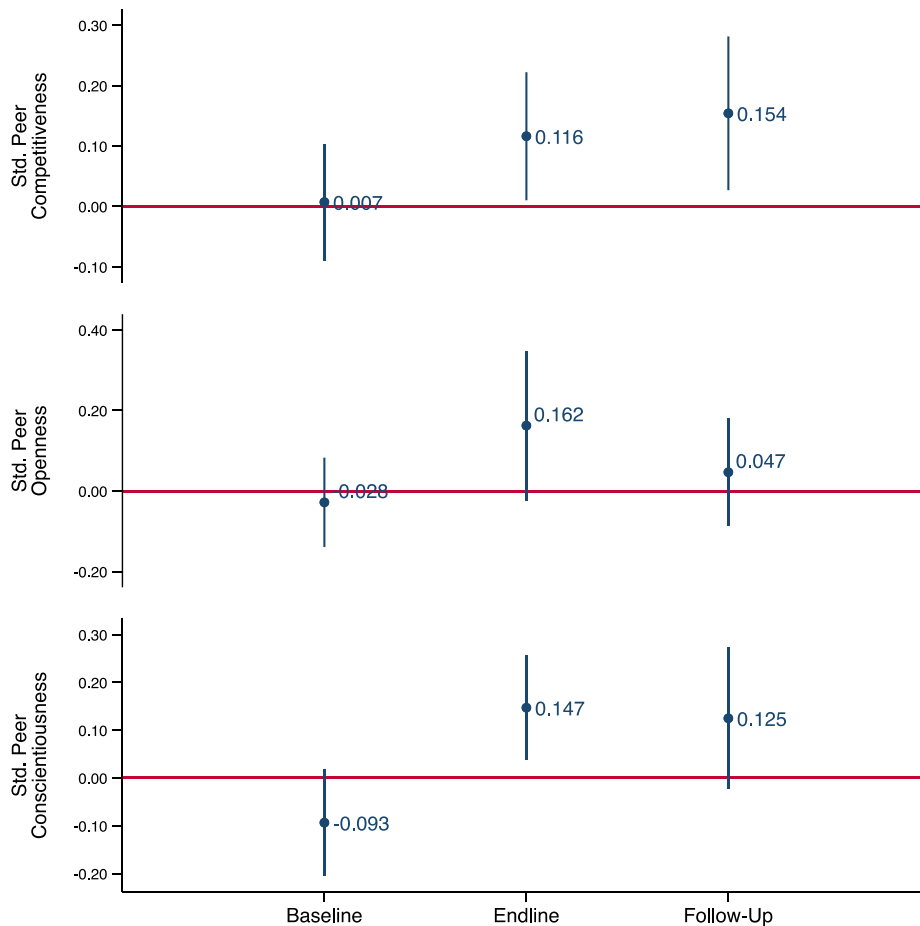
Finally, we examine how personality traits measured years later respond to peer personality measured at baseline and plot the estimates in the last column of Figure 5. Overall, we find suggestive evidence of persistent personality spillovers. For competitiveness and conscientiousness, the follow-up spillovers are less precisely estimated but have very similar effect sizes compared to the endline spillovers. Up to four years after being assigned to a study group, students who had peers who were one SD more competitive are still 0.154 points more competitive (compared to 0.116 points at the endline). Similarly, students who had peers who were one SD more conscientious are 0.125 points more conscientious (compared to 0.147 points at the endline). For openness, we see lower point estimates at the follow-up than at the endline (0.047 vs. 0.162 points), but the 95% confidence interval includes the endline effect size. Taken together, these results suggest that spillovers in conscientiousness and competitiveness are more persistent, while spillovers in openness are relatively short lived.

Although the follow-up effects are less precisely estimated and therefore more suggestive, they give us confidence that our main results are not driven by short-term

⁹ Table A7 in the Appendix tests whether baseline peer personality predicts follow-up survey participation. We find no evidence that any of the six peer personality traits significantly predicts the response rate.

behavioral changes. Our results suggest that peers can induce lasting changes in behavior that remain visible in personality measures.

Figure 5: Balance, Short- and Longer-Term Effects of Peer Personality



Notes: The figure shows estimated effects of peer personality on students' own personality measured at the baseline, in the endline survey, and in the 2021 & 2022 follow-up surveys—up to four years after the experiment. The estimation sample ($N = 748$) includes participants from the first four cohorts (2018/19 to 2021/22) since we do not yet have follow-up measures for the 2022/23 cohort. Baseline estimates use the same specifications outlined in Table 3. The endline and follow-up estimates are based on the model reported in column (5) of Table 4. To make sure that the outcomes are comparable across time, we use raw scores of personality traits at baseline, endline, and follow-up on a scale of 1 to 10 or 1 to 7. To reduce measurement error, the follow-up outcomes use the average value of personality in two follow-up surveys. Error bars indicate 95% confidence intervals. The p -values for spillover effects on follow-up personality are 0.018 (competitiveness), 0.494 (openness), and 0.097 (conscientiousness), respectively.

5.5 Impact of Peer Personality on Performance and Mental Health

Does peer personality also affect students' academic performance and well-being? Even though peers' academic ability does not affect own personality development (Table A9), peer personality may still affect own academic performance. Such effects could be a result of peer-

induced changes in students' own personality, direct effects of peer personality on performance, or both. Peer personality may also affect students' mental health due to the quality of social interaction itself or indirectly through academic and personality spillovers. In this section, we analyze whether the three traits generating personality spillovers also affect course performance and anxiety levels at the end of the semester.

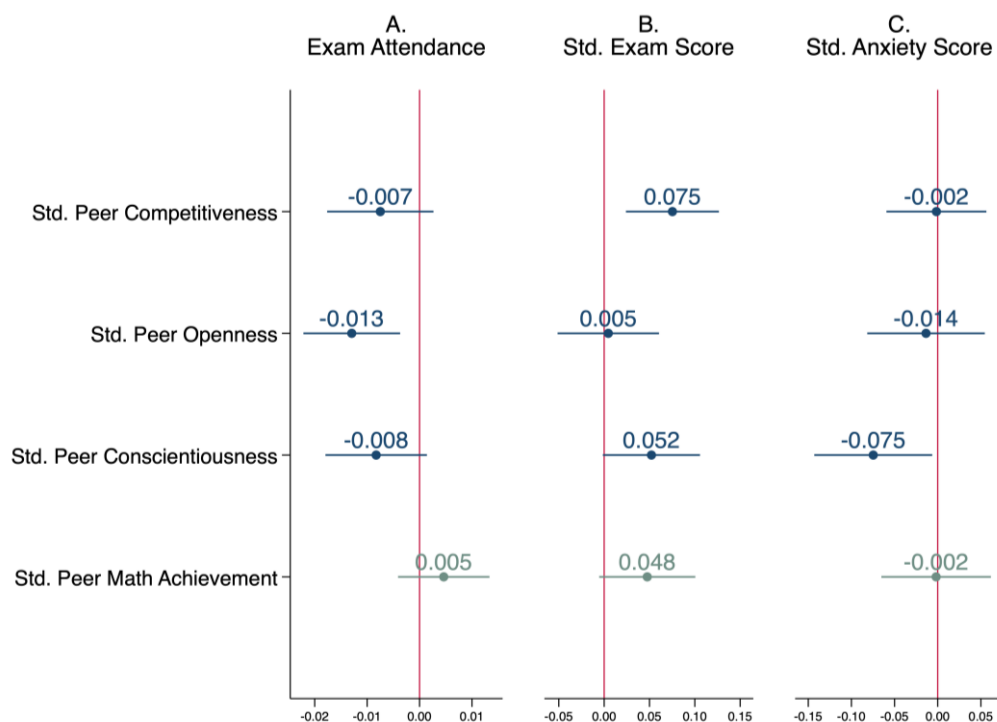
Overall, we see some evidence that peer personality impacts performance. Panels A and B of Figure 6 show that these peer traits largely appear to lower the likelihood that students attend the final exam but raise the obtained exam score. Students exposed to peers who were one SD more open to new experiences become 1.3 percentage points less likely to take the final exam (p -value = 0.006). Students with peers who were one SD more competitive perform 0.075 SD better on the final exam (p -value = 0.004). Similarly, peers who were one SD more conscientious improve exam performance by 0.052 SD (p -value = 0.056). These results provide one possible explanation for why the spillovers in competitiveness and conscientiousness are more long-lasting while the spillover in openness fades away over time. The positive effects on both personality and performance may reinforce each other and lead to longer-term behavioral changes.

Panel C of Figure 6 shows that exposure to more-conscientious peers also improves students' mental health. Specifically, students with peers who were one SD more conscientious reported an anxiety score lower by 0.075 SD, or 0.13 points on a scale of 0 to 6 (p -value = 0.032), at the end of the semester. The result is similar when we use a binary measure of anxiety (the score being at least 3). Peers who were one SD more conscientious lower the occurrence of anxiety by 0.05 percentage points (p -value = 0.013). Conscientious peers tend to be more reliable, organized, and responsible. Having peers with these traits may therefore be beneficial for work collaboration in study groups, leading to lower levels of stress and better academic performance.

As a benchmark, we also estimate the impact of peers' pre-university math achievement on performance and mental health. We find that exposure to high-achieving peers has a marginally significantly positive impact on exam performance but does not significantly impact the probability of exam attendance or anxiety levels. When comparing the effects of peer academic ability and peer personality, we find that peer competitiveness and conscientiousness create even slightly stronger effects on performance (although the differences are not statistically different). Meanwhile, peer conscientiousness has a strong positive effect on mental health, which is statistically significantly different from the effect of peer academic ability.

Taken together, our findings suggest that exposure to peers with productive noncognitive skills is potentially more important than exposure to high-achieving peers. Our finding that peer personality affects own personality and performance, while peer achievement only marginally affects performance, suggests that the peer-to-peer spillovers of cognitive and noncognitive skills on each other are potentially asymmetric. This echoes the skill formation model of [Cunha and Heckman \(2008\)](#), which highlights that noncognitive skills promote the formation of cognitive skills, but cognitive skills mostly do not promote the formation of noncognitive skills.

Figure 6: Impact of Peer Personality and Achievement on Educational Outcomes



Notes: The figure shows how peer personality affects exam attendance and the obtained exam score for the economics course and the reported anxiety score at the end of the semester. The exam score and anxiety score are standardized with a mean of zero and a standard deviation of one. We also examine how peers' math achievement affects the three outcomes. Each point estimate represents one OLS regression. All specifications include the same controls as column (3) of Table 4. Error bars indicate 95% confidence intervals.

6. Mechanisms

Peers impact students' personality development. We find spillovers for conscientiousness and competitiveness that appear to persist and spillovers for openness that seem more short-lived. These results raise the question of why the spillovers are concentrated on certain traits. Given the large literature on socialization and peer effects, we find it unsurprising that peers *can* affect

personality development, especially during a formative period in early adulthood.¹⁰ The finding that seems puzzling to us is that peers induce personality change for some traits, but leave other traits unaffected. To understand the sources of trait-specific spillovers, we explore two possible mechanisms: the intensity of peer-to-peer interaction and the motivation for personality change.

6.1 Peer-to-Peer Interaction

We first examine whether peers with different personality traits induce different degrees of social interaction. If students interact more with peers who are more competitive, open-minded, and conscientious, the personality spillovers may simply be due to more intense exposure to peers with these traits. To test this, we use data on the frequency of interaction measured in the endline survey. More specifically, starting from the year 2019/20, students reported how frequently they had interacted with their study mates to pursue academic or social activities. Answers were recorded on a scale from 0 (“Never”) to 5 (“Multiple times per week”).

Table 5: Peer Personality and Interaction Frequency

	(1) Std. Frequency of Academic Interaction	(2) Std. Frequency of Social Interaction	(3) Std. Overall Frequency of Interaction
Peer Competitiveness	-0.022 (0.058)	-0.024 (0.052)	-0.025 (0.055)
Peer Openness	0.016 (0.056)	-0.040 (0.055)	-0.017 (0.057)
Peer Conscientiousness	0.091 (0.061)	0.079 (0.065)	0.096 (0.064)
Peer Extraversion	-0.049 (0.055)	0.140*** (0.052)	0.058 (0.054)
Peer Agreeableness	-0.033 (0.057)	0.011 (0.057)	-0.011 (0.061)
Peer Neuroticism	0.014 (0.050)	-0.015 (0.049)	-0.000 (0.051)
Observations	868	868	868
R-squared	0.076	0.061	0.069

Notes: We test whether peer personality impacts the frequency of academic interaction with peers (studying together for classes) in column (1), the frequency of social interaction (joint extracurricular activities) in column (2), and the overall frequency of interaction with peers in column (3). The peer-to-peer interaction data were

¹⁰ Broadly classified, the existing literature provides three possible channels for why peers *can* affect personality development. The role model or *social learning channel* captures the idea that students learn from peers’ characteristics, behaviors, and outcomes, and adapt their own personalities (Bandura and Walters 1963; Bandura and McClelland 1977; Moretti 2011, Bursztyn et al. 2014). The *social comparison channel* states that students use peers as reference points to compare themselves to and become more similar to their peers’ personalities (Suls et al. 2002; Rayo and Becker 2007; Chen et al. 2010). The *group socialization channel* states that group members’ personalities become more like each other due to within-group assimilation or the pressure of group norms (Harris 1995; Rubin et al. 2006; Reitz et al. 2014).

collected in the endline survey for cohorts from 2019/20 to 2022/23, so the sample size is smaller. The frequency of academic and social interaction is measured with two survey questions with an answering scale from 0 (“Never”) to 5 (“Multiple times per week”). The overall frequency in column (3) is a normalized measure, with a mean of zero and a standard deviation of one, combining the frequency of academic and social interaction. One column represents one regression, using the same specification as in column (4) of Table 4. All peer traits and interaction frequency variables are standardized among the analysis sample. Standard errors clustered at the group level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 estimates how different peer personality traits affect the frequency of academic and social interactions. The dependent variables are the standardized frequency of academic interaction (Column 1) and social interaction (Column 2) and the overall frequency of interaction (Column 3), which combines the two former measurements. Perhaps unsurprisingly, having more extroverted peers increases the frequency of joint social activities. Apart from this, the frequency of academic, social, or overall interaction does not significantly vary with peers’ personality traits. These results suggest that the spillovers of competitiveness, openness, and conscientiousness are unlikely driven by more frequent peer-to-peer interaction.

6.2 Self-Motivated Personality Change

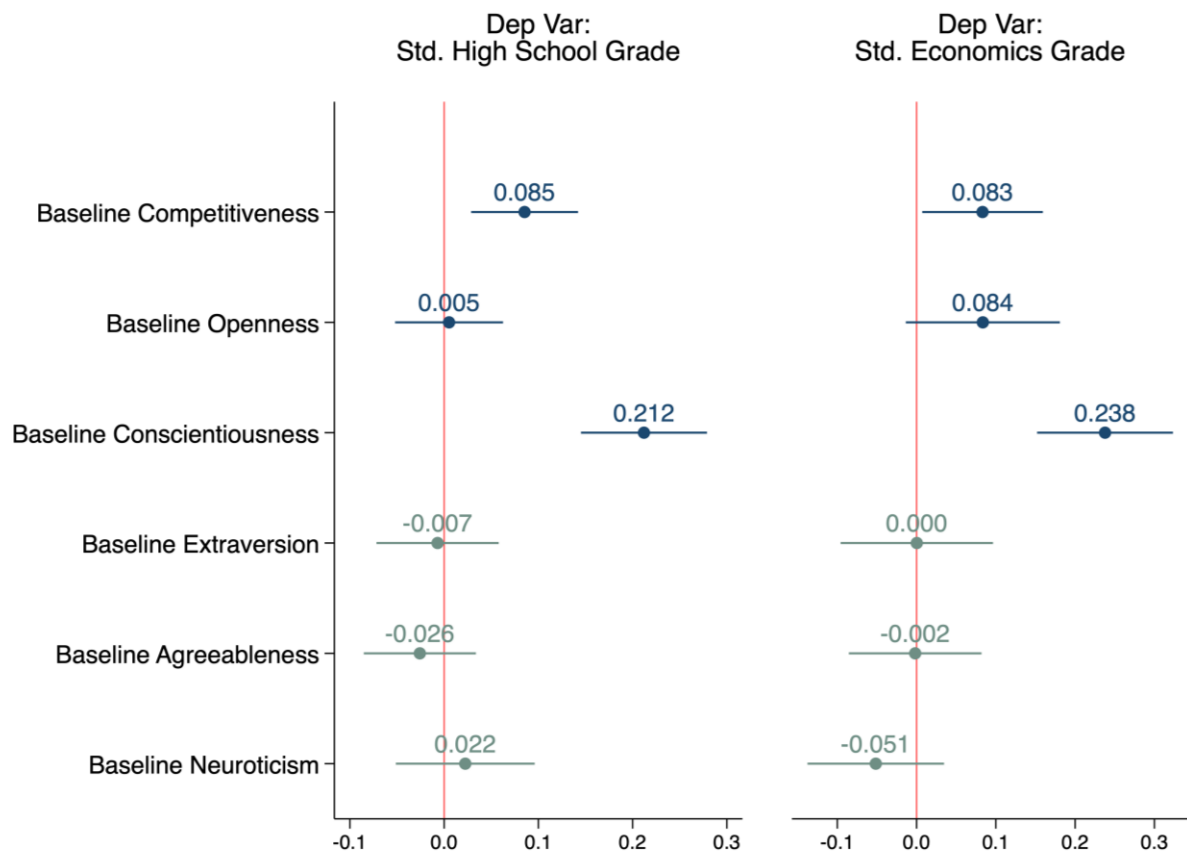
One reason why spillovers are traits specific could come from students having different incentives to change specific personality traits. McCrae and Costa (1994) propose that personality change is costly, and Hennecke et al. (2014) argue that people may only engage in changes that they consider desirable and feasible. Translating these ideas to our context implies that we would expect personality spillovers to be concentrated on traits that are valuable to students in our academic setting.¹¹ Therefore, we analyze the academic returns to different personality traits to understand the *relevance* of each trait. More specifically, we test how different personality traits correlate with two indicators of academic achievement that we observe in our setting: the high school grade and the final grade for the economics course.

Figure 7 shows that conscientiousness and competitiveness significantly predict both past and future achievement with similar effect sizes. A one SD increase in own conscientiousness predicts a 0.2 SD increase in high school and economics grades (p -value < 0.001 for both). Similarly, a one SD increase in competitiveness predicts a 0.085 SD increase in the high school grade (p -value = 0.003) and a 0.083 SD increase in the economics grade (p -

¹¹ We formally illustrate this intuition behind this cost-benefit analysis of personality change with a simple theoretical framework in Appendix B. In this framework, a personality trait may or may not affect academic performance, and changing a trait is costly. The presence of peers affects the cost of personality change due to social learning or pressure. Our key prediction is that students become more like their peers for traits that affect academic performance. For traits that are irrelevant for academic performance, students lack the incentive to change.

value = 0.032). The pattern for openness is less clear. It does not significantly predict high school grade but predicts higher economics grade (p -value = 0.092). Extraversion, agreeableness, and neuroticism do not significantly predict either measure of student performance. Taken together, these results highlight that personality spillover effects (especially the effects that appear long-lasting) are only visible for traits that predict performance. These patterns are consistent with the idea that students engage in self-motivated personality change for traits that are *academically relevant*.

Figure 7: Correlation Between Personality and Academic Achievement



Notes: The figure shows how baseline personality traits predict the high school grade (the average of math and language grades) and the final grade for the economics course where we conducted the experiment. The raw measures of both grades are on a scale of 1 to 6. We use a standardized measure of them (with a mean of zero and a standard deviation of one) in the analysis. Estimates are based on 12 separate regressions controlling for all baseline characteristics except other personality traits. Standard errors are clustered at the study group level. Error bars indicate the 95% confidence intervals.

7. Robustness and External Validity

7.1 Robustness of Results

Extreme Values of Personality: One potential concern for the interpretation of our results are ceiling effects. For example, students with the highest trait levels can only move downward, which may appear like they are becoming more like their peers. To mitigate the concern, we test whether our results are driven by students displaying extremely low or high levels of personality traits at baseline. In Appendix Figure A5, we show spillover effects for the full sample and the sample of students whose baseline personality traits do not display extreme values. We find that the point estimates and confidence intervals remain very similar after dropping the extreme values. These results suggest that ceiling effects do not affect our estimates.

Pre-Registration: Our initial pre-registration (AEARCTR-000325) filed before the start of the experiment in 2018/19 was not very specific about the statistical tests we would perform and did not have a formal pre-analysis plan. In our previous working paper ([Shan and Zölitz 2022](#)), we analyzed personality spillovers using the first three waves of data collection (2018/19 to 2020/21). After replicating the experiment for two additional years (2021/22 and 2022/23), we combine data from all five cohorts and follow the exact empirical specification as in our previous working paper.¹²

Multiple Hypothesis Testing: Even though we conduct the same analyses in our previous working paper, concerns about multiple hypothesis testing remain. Figure 2 and Figure 6 highlight that we look at multiple outcomes and multiple peer characteristics, including the spillovers of six personality traits and how three peer traits affect educational and mental health outcomes. Therefore, we conduct multiple testing corrections *simultaneously* for these 15 estimates, even though this paper focuses on the six estimates of personality spillovers.

We use the approach of [Romano and Wolf \(2005, 2016\)](#) with resampling procedures to adjust the p -values. Table 6 shows that the spillovers of competitiveness, openness, and conscientiousness remain statistically significant at the 5% or 1% level. The impact of peer

¹² In Appendix Figure A2, we show the main results (the six estimates of personality spillover) separately for the sample of students from the first three waves and the sample of students from all five waves. The results appear very similar both qualitatively and quantitatively for the earlier and later waves.

openness on exam attendance and the impact of peer competitiveness on exam performance remain significant at the 1% level. In addition, the effects of peer conscientiousness on performance and anxiety remain statistically significant at the 10% and 5% levels. These results mitigate concerns over multiple hypothesis testing.

Table 6: Multiple Testing Correction for Main Results

	(1)	(2)	(3)	(4)
	Within-Trait Spillover	Impact on Performance and Anxiety		
		Exam Attendance	Std. Exam Score	Std. Anxiety Score
Peer Competitiveness	0.052 (0.0265) {0.0375}	-0.007 (0.1475) {0.3195}	0.075 (0.0041) {0.0042}	-0.002 (0.9582) {0.9996}
Peer Openness	0.067 (0.0016) {0.0018}	-0.013 (0.0061) {0.0072}	0.005 (0.8725) {0.9996}	-0.014 (0.6950) {0.9883}
Peer Conscientiousness	0.046 (0.0292) {0.0417}	-0.008 (0.0935) {0.1813}	0.052 (0.0563) {0.0929}	-0.075 (0.0322) {0.0440}
Peer Extraversion	0.002 (0.9019) {0.9996}			
Peer Agreeableness	-0.002 (0.9996) {0.9670}			
Peer Neuroticism	-0.011 (0.5633) {0.3195}			

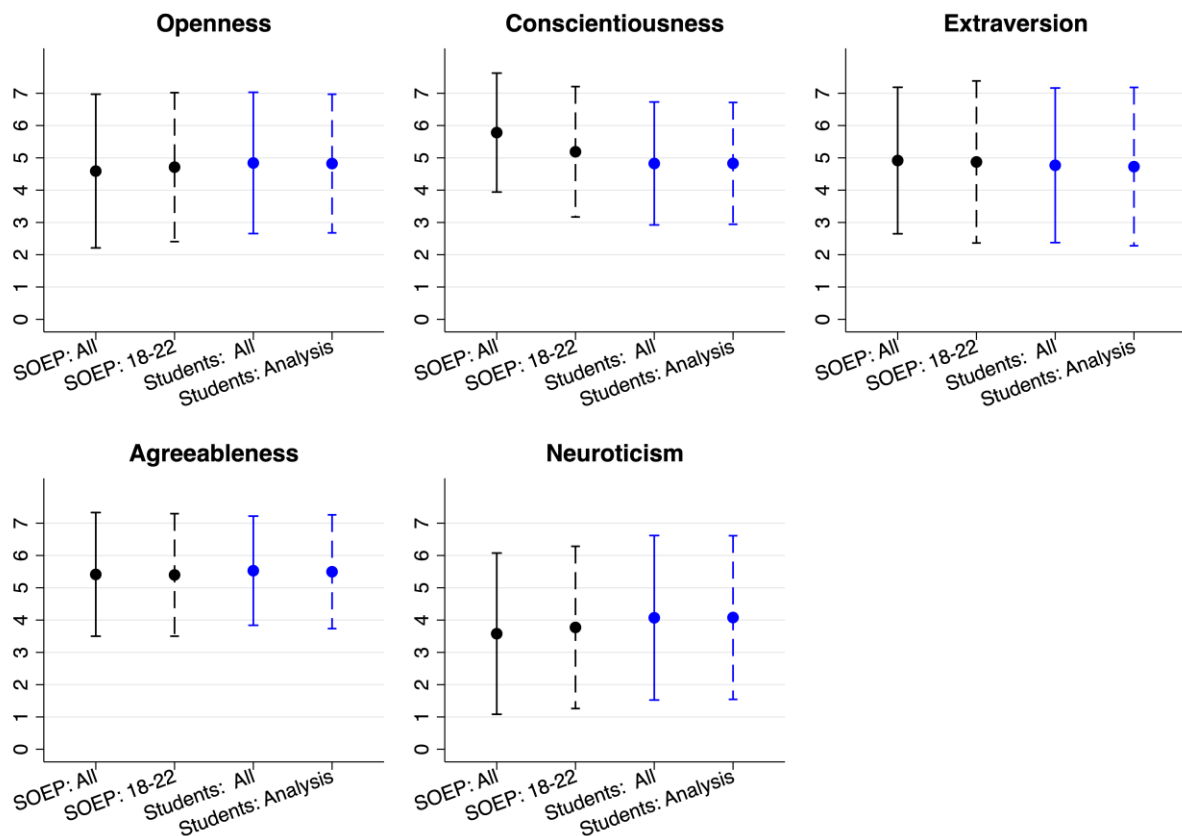
Notes: The table shows the original and corrected p -values for the main results of this paper—the original results are presented in column (3) of Table 4 and Figure 5. The original p -values based on clustered standard errors are in parentheses, and the corrected p -values are in curly braces. We correct the p -values using the approach of Romano and Wolf (2005, 2016) with 10,000 resampling of group clusters within the assignment strata (cohort and study program).

7.2 External Validity

This paper uses a field experiment to examine peer-to-peer personality spillovers among first-year university students. Are our results externally valid beyond our experimental setting? Following the guidelines provided by List (2020), we discuss the external validity of our results from the following perspectives: sample selection, attrition, naturalness, and scaling.

Selection: Our experimental subjects are first-semester university students who are interested in group work and social interactions. As discussed above, students who signed up for study groups in the class were similar to other students with respect to most baseline characteristics including gender, achievement, competitiveness, agreeableness, neuroticism, etc. We only find that students who registered were slightly less conscientious and more open-minded. This suggests that our analysis sample is fairly representative of all students in the setting, except that the nonparticipants seem to have a lower demand for interactions with peers. It is also reassuring that our results are robust to controlling for all baseline personality traits and do not significantly vary with the levels of baseline personality.

Figure 8: Personality Traits of Our Student Sample versus SOEP Respondents



Notes: The figure shows the means and 95% confidence intervals for the Big Five personality traits separately for four samples: (1) the whole sample of respondents of the German Socio-Economic Panel (SOEP) in 2019 who were aged at or above 18 ($N = 25,979$), (2) the young adult sample of the 2019 G-SOEP who were aged 18–22 ($N = 1,920$); (3) the whole sample of students in our setting who answered the baseline survey from 2018/2019 to 2020/2021 ($N = 4,356$); and (4) our final analysis sample of students ($N = 963$). Samples (2) and (4) are subsamples of (1) and (3), respectively. For our student sample, we plot summary statistics of their baseline personality. All respondents in the SOEP and our study reported the Big Five personality traits on the same scale (1–7 point Likert scale), so the raw scores are comparable across samples.

Are students at the Swiss university we study representative of young adults in other contexts and other countries? To answer this question, we compare personality traits of our sample to respondents to the 2019 German Socio-Economic Panel (G-SOEP), aged 18 to 22. The key advantage of this comparison is that the G-SOEP used the exact same items and answering scale to measure the Big Five personality traits in a representative German sample (SOEP 2020). Figure 8 plots the means and 95% confidence intervals of each personality trait separately for the whole sample of G-SOEP respondents ($N = 25,979$), the respondents aged 18 to 22 ($N = 1,920$), all students in our setting who reported their personality at baseline ($N = 4,356$), and our final analysis sample of students who signed up for study groups and satisfied the inclusion criteria mentioned in Section 3.2 ($N = 963$). We find that students in our sample are very similar to the German representative sample of young adults (and even the whole sample) along all the dimensions of personality—both in terms of level and variability.

Attrition: Our analysis focuses on students who reported their personality both at the baseline and endline. Over the period of a semester, the attrition rate was about 30%. However, we find no evidence on selective attrition: the endline response rate does not vary with experimental conditions. Appendix Table A2 shows that peer personality does not affect attrition. To study educational outcomes, we use administrative data and therefore face no attrition issues related to these outcomes. Finally, as Table 5 shows, we find no evidence for selective compliance with the experiment interventions: students' likelihood and intensity of interactions with assigned peers do not vary with peer personality.

Naturalness: Our experiment took place in a first-semester university class, where studying in groups and interacting with new peers were normal. Random (study) group assignment is also a common practice employed in schools and universities (Webb 1982)—for recent examples in higher education see Feld and Zölitz (2017) and Hancock and Hill (2021). The baseline and endline surveys were framed as general course surveys and sent to all students in the class—not only the experimental sample with assigned study groups. Therefore, subjects were not aware that their reported personality scores would be used for analyses on peer effects. The exogenous variations we examine in this paper represent typical natural variations in group composition that would occur in many other settings. In other words, we use a naturally designed field experiment without artificial interventions.

Scaling: Given the high naturalness of our experiment, we think our analyses can be easily replicated in other settings. We study variations in group composition that often occur naturally. Therefore, to examine the scaling feature of our results, the relevant question is not whether the program can be implemented in the general population, but rather whether our estimated spillovers can be detected in other settings. As highlighted in our model, personality spillovers are concentrated in traits that are relevant for individuals' goals in a given context. Thus, we think that personality spillovers might be to some degree context-specific. For example, in art studies, where open-mindedness is more valuable, we may find spillovers in openness. In theater and acting studies where extraversion is more productive, we may detect spillovers in extraversion. We also expect that it will be more difficult to detect personality spillovers in later life stages when personality is less malleable, or in weak or fragmented social networks with less peer-to-peer interaction.

Taken together, our findings are based on a naturally designed field experiment without obvious selection or attrition issues. Our sample is also representative of young adults in a general population regarding personality traits. Therefore, we think that our results have high external validity and can apply to similar youth and adult populations that learn through social interactions. We expect to find different patterns of personality spillovers in other contexts where returns to personality and costs of personality change differ from our setting. Future studies could explore the transmission of personality and noncognitive skills in other contexts to help us better understand personality development under the influence of peers.

8. Conclusion

Attending university is formative for students. They socialize with their peers and adapt to their new environment. This paper represents the first systematic study on how the peer environment shapes students' personality. To identify the causal impact of peer personality, we conduct a field experiment that randomizes undergraduate students into study groups that have frequent social interactions.

We find that the peers students meet at the beginning of their studies have a lasting impact on their personality development. These spillovers are trait specific. Students increase their competitiveness, openness, and conscientiousness if their study group consists of peers who score higher on these traits. Extraversion, agreeableness, and neuroticism appear unaffected by peer personality. Consistent with previous studies, we provide suggestive evidence that peer personality also influences student performance.

We further explore why peers affect some traits but not others. We find that the trait-specific spillovers are not driven by more intensive interactions with peers carrying certain personality traits. Instead, by examining how different traits correlate with academic performance, we find that the traits creating spillovers are more “academically relevant” to students in the setting. Taken together, these results suggest that students adopt peer traits that are valuable to them and help them succeed in the academic environment.

Our paper establishes that personality is malleable and shaped through socialization with peers. It provides the first causal link between the peer environment and personality development. The existence of these personality spillovers is important for policymakers and practitioners in education who assign students to classes or groups, employers who allocate workers to teams, families who choose schools or neighborhoods, and students making choices of peers or friends. Our results suggest that exposure to peers with productive personality traits can be as important as exposure to high-performing peers. Given the growing returns to noncognitive skills in education and the labor market ([Deming 2017](#); [Edin et al. 2022](#)), it is becoming increasingly important to understand the causal determinants of personality change and to consider personality as an explicit policy target.

References

- Abeler, J., Falk, A., & Kosse, F. (2021). Malleability of preferences for honesty. *CESifo Working Paper 9033*.
- Alan, S., Baysan, C., Gumren, M., & Kubilay, E. (2021). Building social cohesion in ethnically mixed schools: An intervention on perspective taking. *The Quarterly Journal of Economics*, *136*(4), 2147-2194.
- Alan, S., Boneva, T., & Ertac, S. (2019). Ever failed, try again, succeed better: Results from a randomized educational intervention on grit. *The Quarterly Journal of Economics*, *134*(3), 1121-1162.
- Alan, S., & Ertac, S. (2018). Fostering patience in the classroom: Results from randomized educational intervention. *Journal of Political Economy*, *126*(5), 1865-1911.
- Bandura, A., & Walters, R.H. (1963). *Social Learning and Personality Development*. Holt Rinehart and Winston: New York.
- Bandura, A., & McClelland, D. C. (1977). *Social Learning Theory* (Vol. 1). Prentice Hall, Englewood Cliffs.
- Barseghyan, L., Clark, D., & Coate, S. (2019). Peer preferences, school competition, and the effects of public school choice. *American Economic Journal: Economic Policy*, *11*(4), 124-58.
- Bietenbeck, J. (2021) Peer Motivation, and Educational Success. *Working Paper*.
- Billings, S. B., & Hoekstra, M. (2019). Schools, neighborhoods, and the long-run effect of crime-prone peers. *NBER Working Paper No. 25730*.
- Boneva, T., Buser, T., Falk, A., & Kosse, F. (2021). The origins of gender differences in competitiveness and earnings expectations: Causal evidence from a mentoring intervention. *IZA Discussion Paper No. 14800*.
- Booij, A. S., Leuven, E., & Oosterbeek, H. (2017). Ability peer effects in university: Evidence from a randomized experiment. *The Review of Economic Studies*, *84*(2), 547-578.
- Borghans, L., Golsteyn, B. H., Heckman, J., & Humphries, J. E. (2011). Identification problems in personality psychology. *Personality and Individual Differences*, *51*(3), 315-320.
- Borghuis, J., Denissen, J. J. A., Oberski, D., Sijtsma, K., Meeus, W. H. J., Branje, S., Koot, H. M., & Bleidorn, W. (2017). Big Five personality stability, change, and codevelopment across adolescence and early adulthood. *Journal of Personality and Social Psychology*, *113*(4), 641-657.

- Bursztyn, L., Ederer, F., Ferman, B., & Yuchtman, N. (2014). Understanding mechanisms underlying peer effects: Evidence from a field experiment on financial decisions. *Econometrica*, 82(4), 1273-1301.
- Buser, T., Niederle, M., & Oosterbeek, H. (2014). Gender, competitiveness, and career choices. *The Quarterly Journal of Economics*, 129(3), 1409-1447.
- Buser, T., Niederle, M., & Oosterbeek, H. (2021). Can competitiveness predict education and labor market outcomes? Evidence from incentivized choice and survey measures. *NBER Working Paper No. w28916*.
- Cappelen, A., List, J., Samek, A., & Tungodden, B. (2020). The effect of early-childhood education on social preferences. *Journal of Political Economy*, 128(7), 2739-2758.
- Caspi, A., & Roberts, B. W. (2001). Personality development across the life course: The argument for change and continuity. *Psychological Inquiry*, 12(2), 49-66.
- Carrell, S. E., Fullerton, R. L., & West, J. E. (2009). Does your cohort matter? Measuring peer effects in college achievement. *Journal of Labor Economics*, 27(3), 439-464.
- Carrell, S. E., & Hoekstra, M. L. (2010). Externalities in the classroom: How children exposed to domestic violence affect everyone's kids. *American Economic Journal: Applied Economics*, 2(1), 211-228.
- Carrell, S. E., Hoekstra, M., & Kuka, E. (2018). The long-run effects of disruptive peers. *American Economic Review*, 108(11), 3377-3415.
- Carrell, S. E., Sacerdote, B. I., & West, J. E. (2013). From natural variation to optimal policy? The importance of endogenous peer group formation. *Econometrica*, 81(3), 855-882.
- Chen, Y., Harper, F. M., Konstan, J., & Li, S. X. (2010). Social comparisons and contributions to online communities: A field experiment on MovieLens. *American Economic Review*, 100(4), 1358-1398.
- Chetty, R., Hendren, N., & Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment. *American Economic Review*, 106(4), 855-902.
- Cunha, F., & Heckman, J. J. (2008). Formulating, identifying and estimating the technology of cognitive and noncognitive skill formation. *Journal of Human Resources*, 43(4), 738-782.
- Deutscher, N. (2020). Place, peers, and the teenage years: Long-run neighborhood effects in Australia. *American Economic Journal: Applied Economics*, 12(2), 220-49.
- Ding, W., & Lehrer, S. F. (2007). Do peers affect student achievement in China's secondary schools? *The Review of Economics and Statistics*, 89(2), 300-312.

- Dahmann, S. C., & Anger, S. (2018). Cross-fertilizing gains or crowding out? Schooling intensity and noncognitive skills. *HCEO Working Paper* 2018-065.
- Deming, D. J., Hastings, J. S., Kane, T. J., & Staiger, D. O. (2014). School choice, school quality, and postsecondary attainment. *American Economic Review*, *104*(3), 991-1013.
- Deming, D. J. (2017). The growing importance of social skills in the labor market. *The Quarterly Journal of Economics*, *132*(4), 1593-1640.
- Duflo, E., Dupas, P., & Kremer, M. (2011). Peer effects, teacher incentives, and the impact of tracking: Evidence from a randomized evaluation in Kenya. *American Economic Review*, *101*(5), 1739-74.
- Edin, P. A., Fredriksson, P., Nybom, M., & Öckert, B. (2022). The rising return to noncognitive skill. *American Economic Journal: Applied Economics*, *14*(2), 78-100.
- Feld, J., & Zölitz, U. (2017). Understanding peer effects – on the nature, estimation and channels of peer effects. *Journal of Labor Economics*, *35*(2), 387–428.
- Festinger, L. (1954). A theory of social comparison processes. *Human relations*, *7*(2), 117-140.
- Figlio, D. N. (2007). Boys named Sue: Disruptive children and their peers. *Education Finance and Policy*, *2*(4), 376-394.
- Fryer, R. G., Levitt, S. D., & List, J. A. (2015). Parental incentives and early childhood achievement: A field experiment in Chicago heights. *NBER Working Paper No. 21477*.
- Gerlitz, J. Y., & Schupp, J. (2005). Zur Erhebung der Big-Five-basierten persönlichkeitsmerkmale im SOEP. *DIW Research Notes*, *4*, 2005.
- Gillen, B., Snowberg, E., & Yariv, L. (2019). Experimenting with measurement error: Techniques with applications to the Caltech cohort study. *Journal of Political Economy*, *127*(4), 1826-1863.
- Golsteyn, B. H., Non, A., & Zölitz, U. (2021). The impact of peer personality on academic achievement. *Journal of Political Economy*, *129*(4), 1052-1099.
- Guryan, J., Kroft, K., & Notowidigdo, M. (2009). Peer effects in the workplace: Evidence from random groupings in professional golf tournaments. *American Economic Journal: Applied Economics*, *1*(4), 34–68.
- Hancock, S., & Hill, A. (2021) The effect of teammate personality on team production. *Working Paper*.
- Hahn, E., Gottschling, J., & Spinath, F. M. (2012). Short measurements of personality – Validity and reliability of the GSOEP Big Five Inventory (BFI-S). *Journal of Research in Personality*, *46*(3), 355-359.

- Harris, J. R. (1995). Where is the child's environment? A group socialization theory of development. *Psychological Review*, *102*(3), 458-489.
- Heckman, J. J., & Kautz, T. (2014). Fostering and measuring skills: Interventions that improve character and cognition. In Heckman, J. J., Humphries, J. E., & Kautz, T. (Eds.). *The myth of achievement tests: The GED and the role of character in American life*. (pp. 341-430). University of Chicago Press.
- Hennecke, M., Bleidorn, W., Denissen, J. J., & Wood, D. (2014). A three-part framework for self-regulated personality development across adulthood. *European Journal of Personality*, *28*(3), 289-299.
- Hoxby, C. M. (2000). Peer effects in the classroom: Learning from gender and race variation. *NBER Working Paper No. 7867*.
- Imberman, S. A., Kugler, A. D., & Sacerdote, B. I. (2012). Katrina's children: Evidence on the structure of peer effects from hurricane evacuees. *American Economic Review*, *102*(5), 2048-82.
- Jackson, C. K. (2010). The effects of an incentive-based high-school intervention on college outcomes. *NBER Working Paper No. 15722*.
- Jackson, C. K. (2018). What do test scores miss? The importance of teacher effects on non-test score outcomes. *Journal of Political Economy*, *126*(5), 2072-2107.
- Jackson, C. K., Porter, S. C., Easton, J. Q., Blanchard, A., & Kiguel, S. (2020). School effects on socioemotional development, school-based arrests, and educational attainment. *American Economic Review: Insights*, *2*(4), 491-508.
- Joensen, J. S., List, J. A., Samek, A., & Uchida, H. (2022). Using a field experiment to understand skill formation in the adolescent years. *Working Paper*.
- John, O. P., Donahue, E. M., & Kentle, R. L. (1991). Big five inventory. *Journal of Personality and Social Psychology*.
- Kosse, F., Deckers, T., Pinger, P., Schildberg-Hörisch, H., & Falk, A. (2020). The formation of prosociality: Causal evidence on the role of social environment. *Journal of Political Economy*, *128*(2), 434-467.
- Lang, F. R., John, D., Lüdtke, O., Schupp, J., & Wagner, G. G. (2011). Short assessment of the Big Five: Robust across survey methods except telephone interviewing. *Behavior Research Methods*, *43*(2), 548-567.
- List, J. A. (2020). Non est disputandum de generalizability? A glimpse into the external validity trial. *NBER Working Paper No. 27535*.

- List, J. A., Momeni, F., & Zenou, Y. (2020). The social side of early human capital formation: Using a field experiment to estimate the causal impact of neighborhoods. *NBER Working Paper No. 28283*.
- McCrae, R. R., & Costa Jr, P. T. (1994). The stability of personality: Observations and evaluations. *Current Directions in Psychological Science*, 3(6), 173-175.
- Moretti, E. (2011). Social learning and peer effects in consumption: Evidence from movie sales. *The Review of Economic Studies*, 78(1), 356-393.
- Nechyba, T. J. (2006). Income and peer quality sorting in public and private schools. *Handbook of the Economics of Education*, 2, 1327-1368.
- Oosterbeek, H., & Van Ewijk, R. (2014). Gender peer effects in university: Evidence from a randomized experiment. *Economics of Education Review*, 38, 51-63.
- Pervin, L. A. (1994). A critical analysis of current trait theory. *Psychological Inquiry*, 5(2), 103-113.
- Rayo, L., & Becker, G. S. (2007). Habits, peers, and happiness: An evolutionary perspective. *American Economic Review*, 97(2), 487-491.
- Reitz, A. K., Zimmermann, J., Hutteman, R., Specht, J., & Neyer, F. J. (2014). How peers make a difference: The role of peer groups and peer relationships in personality development. *European Journal of Personality*, 28(3), 279-288.
- Roberts, B. W., Caspi, A., & Moffitt, T. E. (2001). The kids are alright: Growth and stability in personality development from adolescence to adulthood. *Journal of Personality and Social Psychology*, 81(4), 670-683.
- Roberts, B. W., Walton, K. E., & Viechtbauer, W. (2006). Patterns of mean-level change in personality traits across the life course: A meta-analysis of longitudinal studies. *Psychological Bulletin*, 132(1), 1-25.
- Roberts, B. W., Kuncel, N. R., Shiner, R., Caspi, A., & Goldberg, L. R. (2007). The power of personality: The comparative validity of personality traits, socioeconomic status, and cognitive ability for predicting important life outcomes. *Perspectives on Psychological Science*, 2(4), 313-345.
- Roberts, B. W., Luo, J., Briley, D. A., Chow, P. I., Su, R., & Hill, P. L. (2017). A systematic review of personality trait change through intervention. *Psychological Bulletin*, 143(2), 117-141.
- Robins, R. W., Fraley, R. C., Roberts, B. W., & Trzesniewski, K. H. (2001). A longitudinal study of personality change in young adulthood. *Journal of Personality*, 69(4), 617-640.

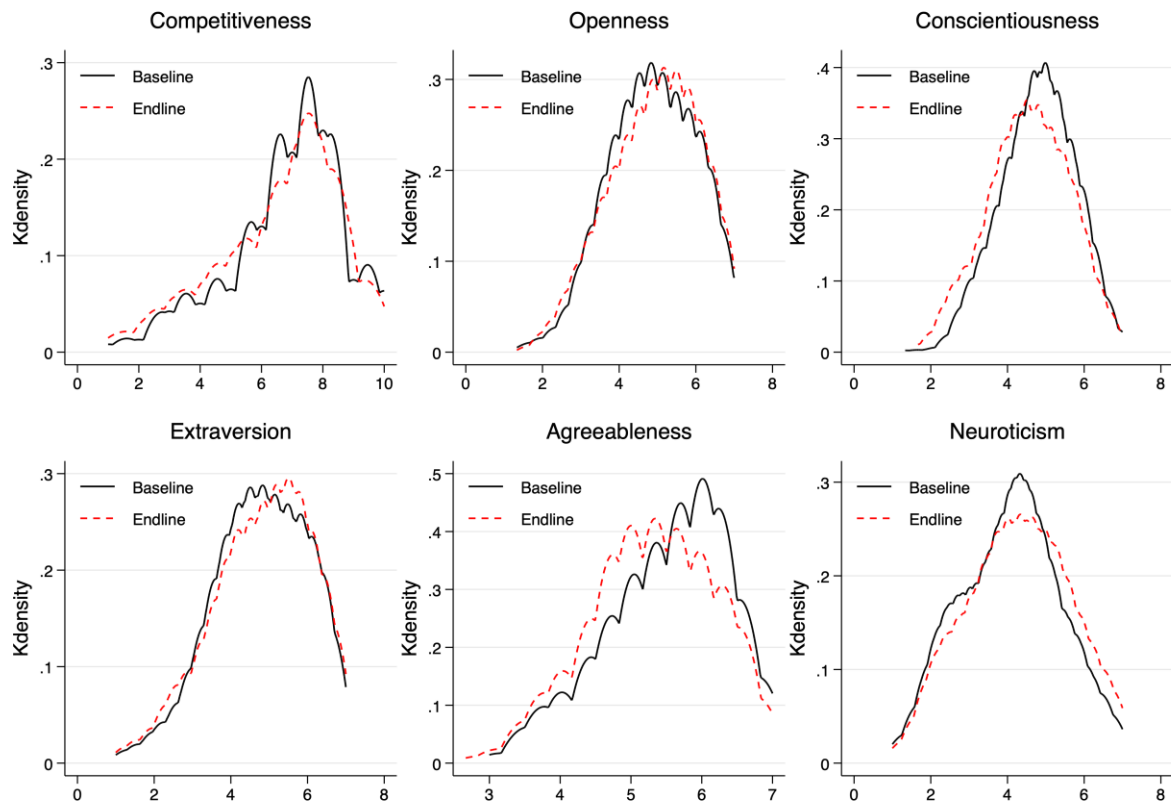
- Romano, J. P., & Wolf, M. (2005). Stepwise multiple testing as formalized data snooping. *Econometrica*, 73(4), 1237-1282.
- Romano, J. P., & Wolf, M. (2016). Efficient computation of adjusted p -values for resampling-based stepdown multiple testing. *Statistics & Probability Letters*, 113, 38-40.
- Rubin, K. H., Bukowski, W. M., & Parker, J. G. (2006). Peer interactions, relationships, and groups. In N. Eisenberg, W. Damon, & R. M. Lerner (Eds.), *Handbook of child psychology: Social, emotional, and personality development* (pp. 571-645). John Wiley & Sons, Inc.
- Sacerdote, B. (2001). Peer effects with random assignment: Results for Dartmouth roommates. *The Quarterly Journal of Economics*, 116(2), 681-704.
- Sacerdote, B. (2014). Experimental and quasi-experimental analysis of peer effects: Two steps forward? *Annual Review of Economics*, 6(1), 253-272.
- Shan, X., & Zölitz, U. (2022). Peers Affect Personality Development. IZA Discussion Paper No. 15257. Available at <https://docs.iza.org/dp15257.pdf>
- Shure, N. (2021). Non-cognitive peer effects in secondary education. *Labour Economics*, 73, 102074.
- SOEP Innovation Sample (SOEP-IS), data from 1998-2019. 2020. DOI: 10.5684/soep.is.2019
- Sojourner, A. (2013). Identification of peer effects with missing peer data: Evidence from Project STAR. *The Economic Journal*, 123(569), 574-605.
- Sorrenti, G., Zölitz, U, Eisner, M. & Ribeaud, D. (2020). The causal impact of socio-emotional skills training on educational success. *IZA Discussion Paper No. 13087*.
- Specht, J., Egloff, B., & Schmukle, S. C. (2011). Stability and change of personality across the life course: The impact of age and major life events on mean-level and rank-order stability of the Big Five. *Journal of Personality and Social Psychology*, 101(4), 862-882.
- Stieger, M., Flückiger, C., Rügger, D., Kowatsch, T., Roberts, B. W., & Allemand, M. (2021). Changing personality traits with the help of a digital personality change intervention. *Proceedings of the National Academy of Sciences*, 118(8) e2017548118.
- Suls, J., Martin, R., & Wheeler, L. (2002). Social comparison: Why, with whom, and with what effect? *Current Directions in Psychological Science*, 11(5), 159-163.

- Terracciano, A., McCrae, R. R., & Costa Jr, P. T. (2010). Intra-individual change in personality stability and age. *Journal of Research in Personality, 44*(1), 31-37.
- Trogon, J. G., Nonnemaker, J., & Pais, J. (2008). Peer effects in adolescent overweight. *Journal of Health Economics, 27*(5), 1388-1399.
- Webb, N. M. (1982). Student interaction and learning in small groups. *Review of Educational Research, 52*(3), 421-445.

ONLINE APPENDIX

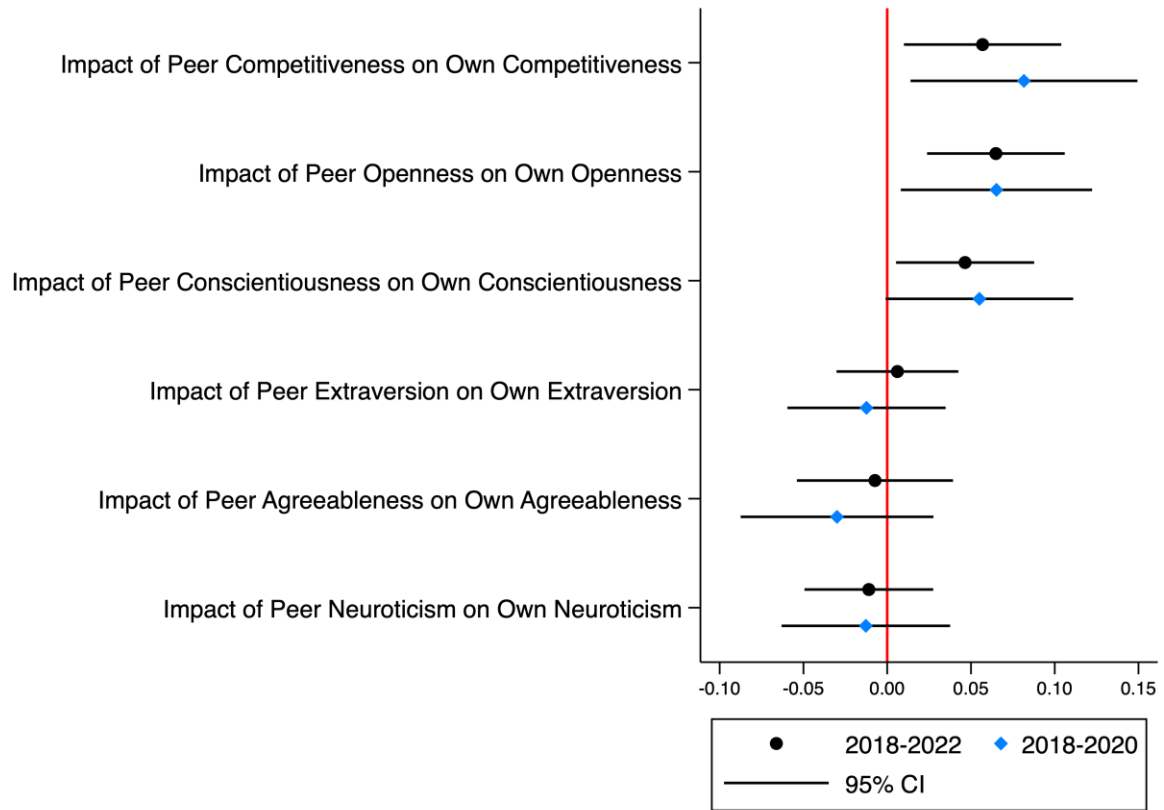
Appendix A: Additional Tables and Figures

Figure A1: The Distribution of Personality Traits at Baseline and Endline



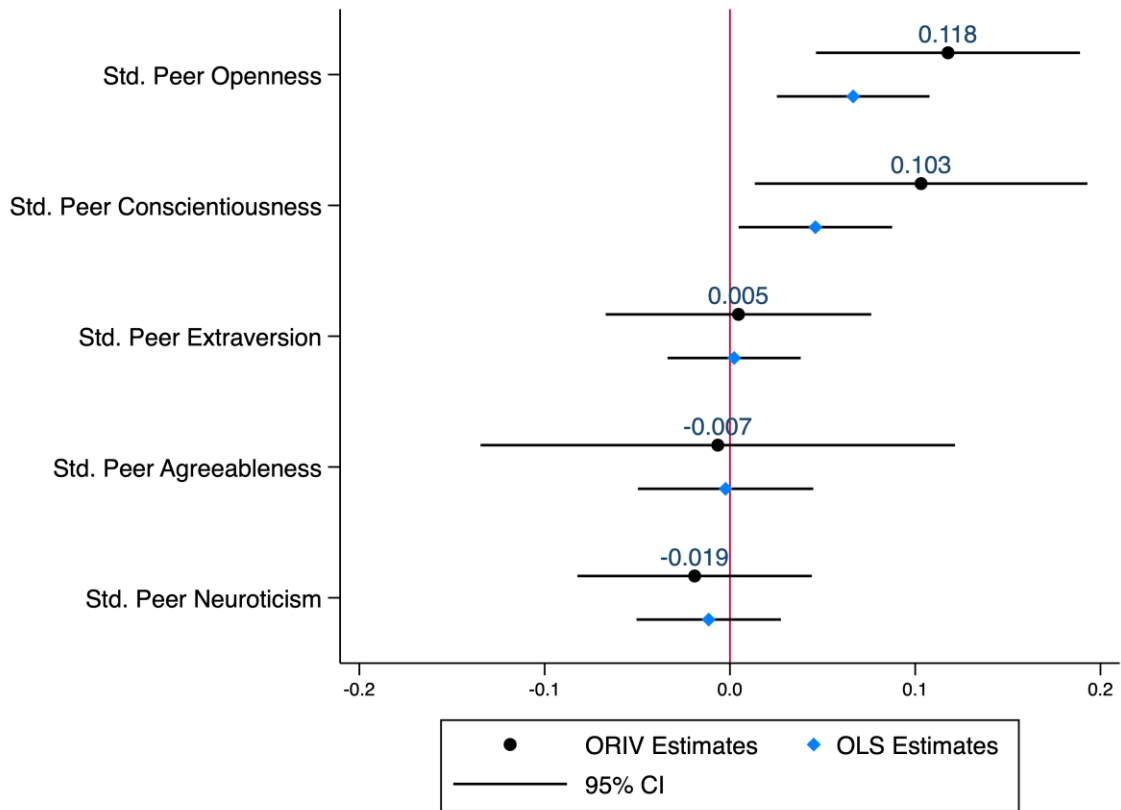
Notes: The figure shows the distribution of six personality traits at the baseline and endline (the raw scores) for the estimation sample (N = 963).

Figure A2: Main Results for the Overall Sample and 2018-2020 Subsample



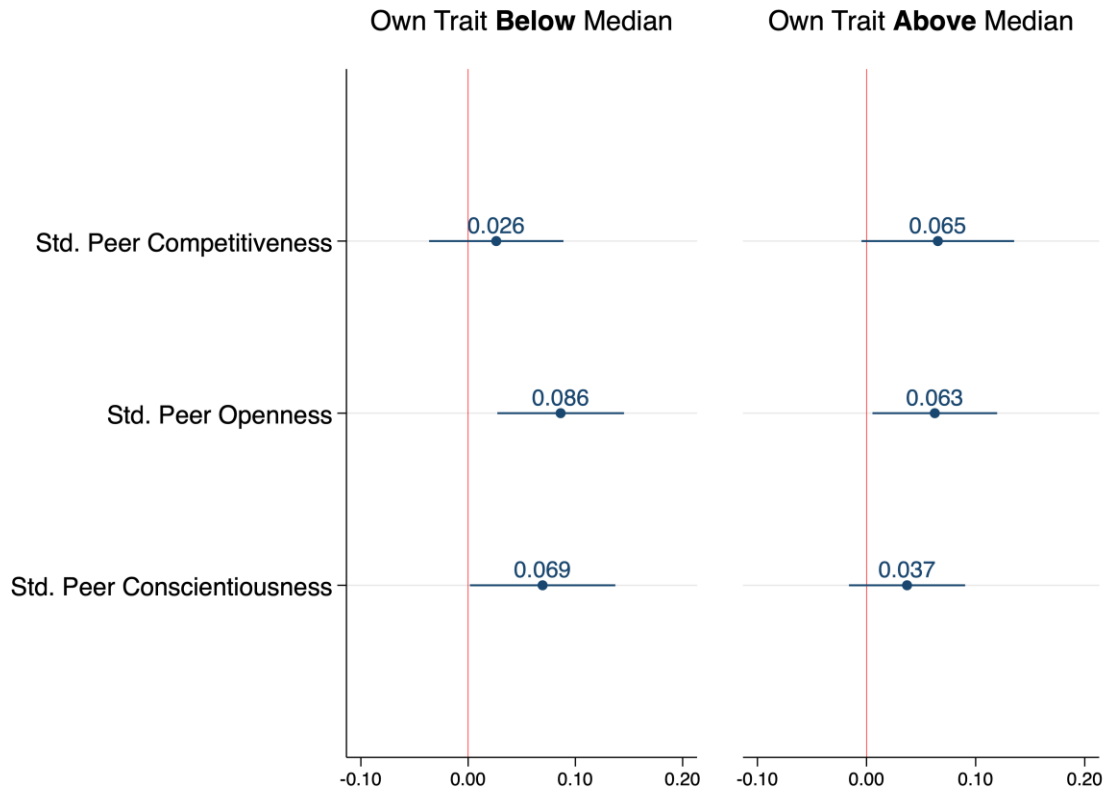
Notes: This figure compares the main results for personality spillovers separately using the full sample of students from cohorts 2018/19 to 2022/23 and using the first three experimental cohorts. Each point estimate is derived from one OLS regression, which uses the same specification as in column (3) of Table 4. Error bars indicate 95% confidence intervals.

Figure A3: Main Results after Correction for Measurement Error Using Obviously Related Instrumental Variables (ORIV)



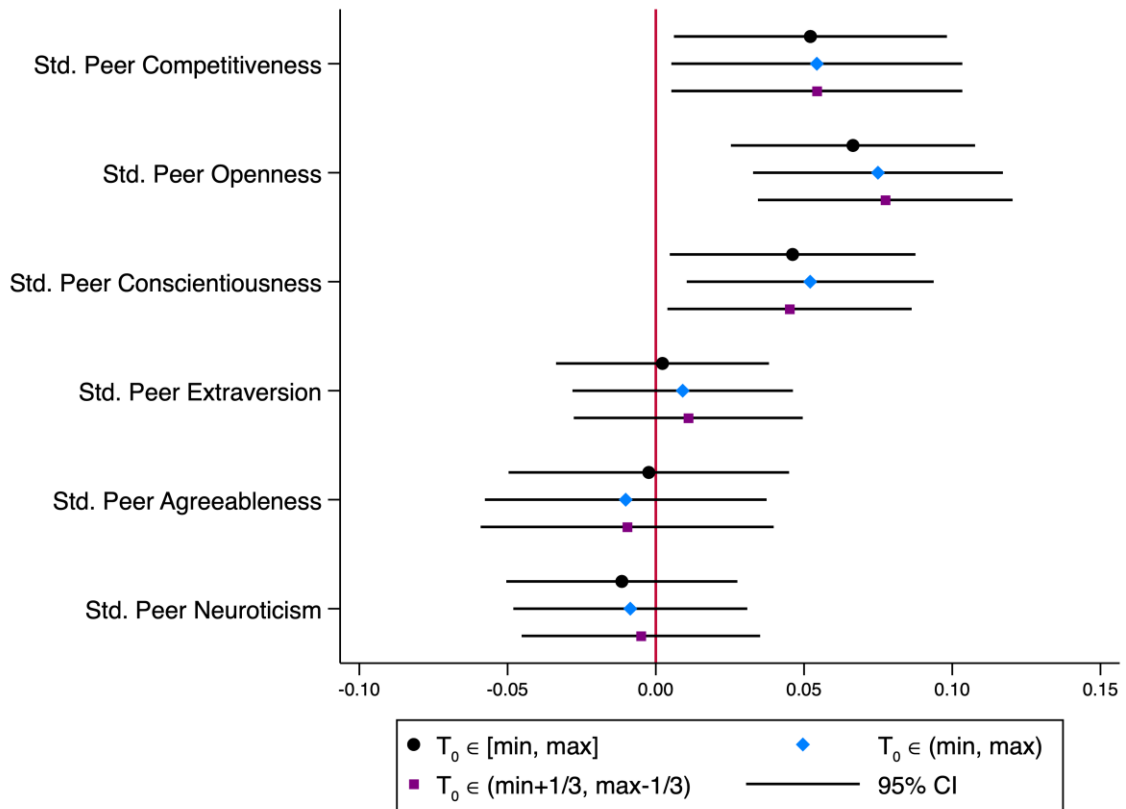
Notes: The figure shows the effects of peer personality on own personality estimated with 2SLS regressions based on the obviously related instrumental variables (ORIV) approach from [Gillen, Snowberg, and Yariv \(2019\)](#). Each of the Big Five personality traits is measured with the mean of three questions. To employ the ORIV method, we duplicate observations six times and use one item as the IV for the other two items. More specifically, suppose we have three items of conscientiousness—consc1, consc2, and consc3. The independent variable is constructed as [consc1, consc1, consc2, consc2, consc3, consc3, ...] and the IV is constructed as [consc2, consc3, consc3, consc1, consc1, consc2, ...]. In the 2SLS regressions, we control for all the characteristics as listed in column (3) of Table 4 and cluster standard errors at the group level. Error bars indicate 95% confidence intervals.

Figure A4: Heterogeneous Effects: Own Trait Below Versus Above Median



Notes: The figure shows heterogeneous effects by the level of own personality at baseline. For each trait, we divide students into two subgroups: those with below-median and above-median levels of the trait. Then we estimate the impact of the peer trait on own trait for the two subgroups, respectively. All regressions control for all individual characteristics at the baseline, as in column (3) of Table 4. None of the between-group differences in impacts reach significance at the 5% level. Error bars indicate 95% confidence intervals based on robust standard errors clustered at the study group level.

Figure A5: Robustness to Dropping Extreme Values of Personality



Notes: The figure shows the estimated spillover effects of six personality traits for the full sample, the sample of students whose baseline level of a trait is in the range of above the minimal value (1) and below the maximum value (7 or 10), and the sample of students whose baseline level of a trait is in the range of above the minimal value plus 1/3 and below the maximum value minus 1/3. Each point estimate is derived from one OLS regression using the same specification as in column (3) of Table 4. Error bars indicate 95% confidence intervals.

Table A1: Baseline Characteristics by Group Registration

	Not Registered (N=2,526)		Registered (N=1,812)		Difference
	mean	<i>sd</i>	mean	<i>sd</i>	<i>p</i> -value
Competitiveness	6.778	1.964	6.859	1.984	0.182
Openness	4.833	1.061	4.923	1.031	0.005
Conscientiousness	4.857	0.954	4.798	0.968	0.047
Extraversion	4.655	1.224	4.724	1.198	0.065
Agreeableness	5.427	0.875	5.478	0.828	0.051
Neuroticism	4.016	1.242	3.966	1.233	0.194
High school math grade	4.549	0.808	4.578	0.790	0.233
High school language grade	4.769	0.547	4.757	0.602	0.498
Female	0.420	0.494	0.441	0.497	0.178

Notes: The table shows the summary statistics of the baseline characteristics by group registration and the *p*-values for tests of differences between students who are registered and students who are not.

Table A2: Test for Balancing and Selective Attrition:**Impact of Baseline Peer Personality on Own Personality and Observing Endline Personality**

	(1)	(2)	(3)	(4)	(5)	(6)
	Competitiveness	Openness	Conscientiousness	Extraversion	Agreeableness	Neuroticism
Panel A: Dependent Variable: Own Personality at Baseline						
Peer Competitiveness	-0.036 (0.038)					
Peer Openness		-0.026 (0.041)				
Peer Conscientiousness			-0.085* (0.045)			
Peer Extraversion				0.010 (0.047)		
Peer Agreeableness					0.012 (0.038)	
Peer Neuroticism						0.042 (0.046)
Observations	1,360	1,360	1,360	1,360	1,360	1,360
R-squared	0.517	0.496	0.547	0.530	0.477	0.445
Panel B: Dependent Variable: Endline Survey Response						
Peer Competitiveness	0.011 (0.013)					
Peer Openness		-0.010 (0.013)				
Peer Conscientiousness			-0.011 (0.013)			
Peer Extraversion				-0.008 (0.012)		
Peer Agreeableness					0.014 (0.013)	
Peer Neuroticism						0.008 (0.012)
Observations	1,360	1,360	1,360	1,360	1,360	1,360
R-squared	0.024	0.024	0.024	0.023	0.024	0.023

Notes: Panel A examines whether peer personality predicts own personality at the baseline; Panel B tests whether peer personality at the baseline affects observing own personality at the endline. Each column represents one OLS regression. The sample used for analysis is 1,360 students in 340 groups, where all students reported their baseline personality traits. Out of the 1,360 students, 963 students (71%) reported their endline personality. The independent variables of interest are standardized peer personality traits at the baseline. All models control for cohort-by-program fixed effects. Results in Panel B are very similar even if we control for all individual characteristics at the baseline. Panel A uses robust standard errors, and Panel B uses clustered standard errors (shown in parentheses).

Table A3: Pairwise Correlations Between Personality Traits

	(1) Competitiveness	(2) Openness	(3) Conscientiousness	(4) Extraversion	(5) Agreeableness	(6) Neuroticism
Competitiveness	1					
Openness	0.050 [0.124]	1				
Conscientiousness	0.294 [0.000]	0.049 [0.126]	1			
Extraversion	0.138 [0.000]	0.235 [0.000]	0.085 [0.009]	1		
Agreeableness	-0.069 [0.032]	0.085 [0.008]	0.162 [0.000]	0.023 [0.480]	1	
Neuroticism	-0.180 [0.000]	0.019 [0.550]	-0.095 [0.003]	-0.234 [0.000]	0.018 [0.569]	1

Notes: Pairwise correlations at the baseline (n = 963). *p*-values in brackets.

Table A4: Cronbach's Alpha Coefficient of Reliability

	(1) Baseline	(2) Endline	(3) Follow-Up
Openness	0.617	0.642	0.747
Conscientiousness	0.601	0.614	0.768
Extraversion	0.807	0.810	0.846
Agreeableness	0.479	0.461	0.550
Neuroticism	0.769	0.794	0.817

Notes: Each cell reports the Cronbach Alpha for each trait (measured with three items) at the baseline, endline, and follow-up survey. In the follow-up survey, we have six items measuring openness and conscientiousness; thus, the alpha coefficients improved substantially. When we use the same three items as in the baseline and endline, the alpha coefficients are 0.624 and 0.589 for openness and conscientiousness. We use the full analysis sample in columns (1) – (2) and the sample from the first three cohorts in column (3).

Table A5: Stability of Personality – Within-Trait Correlation Across Time

	(1)	(2)	(3)	(4)	(5)	(6)
	Competitiveness	Openness	Conscientiousness	Extraversion	Agreeableness	Neuroticism
Baseline & Endline	0.643 [0.000]	0.771 [0.000]	0.691 [0.000]	0.800 [0.000]	0.691 [0.000]	0.755 [0.000]
Baseline & Follow-Up	0.620 [0.000]	0.705 [0.000]	0.634 [0.000]	0.793 [0.000]	0.651 [0.000]	0.731 [0.000]

Notes: The table shows the correlation between a trait’s level at the baseline and its level at the endline or follow-up. *P*-values are in brackets. We use the sample from five cohorts to calculate the correlations between baseline and endline levels and the sample from the first three cohorts to calculate the correlations between baseline and follow-up levels.

Table A6: Edwin Leuven Test of Randomization

	<i>F</i> -statistic	<i>p</i> -value
Competitiveness	1.009	0.456
Openness	0.976	0.602
Conscientiousness	0.768	0.998
Extraversion	1.023	0.391
Agreeableness	0.906	0.862
Neuroticism	1.012	0.441
Female	0.797	0.994
High School Math Grade	1.046	0.302
High School Language Grade	1.093	0.152

Notes: We regress each baseline characteristic on study program and cohort dummies and derive the residuals. Then, we regress the residuals on study group dummies and test the joint significance of group dummies. The table reports the *F*-statistic and *p*-value separately for each baseline characteristic.

Table A7: The Impact of Peer Personality on Follow-Up Survey Response

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Follow-Up Survey Response						
Peer Competitiveness	0.000 (0.015)					
Peer Openness		-0.004 (0.018)				
Peer Conscientiousness			0.001 (0.018)			
Peer Extraversion				-0.023 (0.017)		
Peer Agreeableness					-0.000 (0.017)	
Peer Neuroticism						0.009 (0.018)
Observations	748	748	748	748	748	748
R-squared	0.065	0.066	0.065	0.068	0.065	0.066

Notes: Each column represents one OLS regression in which the dependent variable is an indicator for whether a student reports personality in the follow-up survey. All models use the same specification of column (3) of Table 4. Robust standard errors clustered at the study group level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Balancing Test: Baseline Sample and Estimation Sample

	(1)	(2)	(3)	(4)	(5)
	Baseline Sample (N=1,360)		Estimation Sample (N=963)		Difference (<i>p</i> -value)
	mean	<i>sd</i>	mean	<i>sd</i>	
Competitiveness	6.870	1.964	6.896	1.893	0.747
Openness	4.860	1.095	4.842	1.116	0.708
Conscientiousness	4.789	0.972	4.826	0.971	0.374
Extraversion	4.780	1.235	4.769	1.222	0.835
Agreeableness	5.520	0.872	5.530	0.862	0.790
Neuroticism	4.079	1.286	4.071	1.300	0.887
High School Math Grade	4.585	0.800	4.606	0.795	0.529
High School Language Grade	4.735	0.603	4.749	0.600	0.577
Female	0.439	0.496	0.465	0.499	0.211

Notes: The table reports the summary statistics of the baseline sample (1,360 students in 340 groups) and the final estimation sample (963 students who have also completed the endline survey). *Sd* refers to the standard deviation. Column (5) reports the *p*-values for the test of differences in baseline characteristics between the baseline and analysis sample.

Table A9: The Full Spillover Matrix

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Own Personality Traits at Endline					
	Competitiveness	Openness	Conscientiousness	Extraversion	Agreeableness	Neuroticism
Peer Competitiveness	0.056** (0.025)	-0.002 (0.023)	-0.013 (0.024)	-0.034 (0.021)	0.000 (0.024)	-0.021 (0.021)
Peer Openness	-0.054** (0.026)	0.068*** (0.022)	-0.006 (0.024)	0.014 (0.019)	-0.043* (0.022)	0.016 (0.021)
Peer Conscientiousness	-0.015 (0.025)	0.003 (0.023)	0.060** (0.025)	0.036 (0.023)	-0.016 (0.027)	-0.003 (0.024)
Peer Extraversion	0.035 (0.026)	-0.030 (0.021)	-0.008 (0.026)	0.006 (0.020)	-0.022 (0.025)	-0.020 (0.023)
Peer Agreeableness	0.010 (0.023)	0.022 (0.024)	-0.015 (0.022)	0.027 (0.020)	0.006 (0.024)	0.004 (0.023)
Peer Neuroticism	-0.000 (0.027)	0.016 (0.023)	0.049* (0.025)	0.041** (0.020)	0.028 (0.025)	-0.021 (0.021)
Peer Math Achievement	-0.010 (0.028)	-0.002 (0.023)	0.020 (0.026)	-0.016 (0.022)	-0.019 (0.024)	0.018 (0.023)
Observations	963	963	963	963	963	963
R-squared	0.446	0.615	0.511	0.655	0.505	0.593

Notes: Each column represents one OLS regression in which the dependent variable is one of the six own personality traits at the endline (standardized). The independent variables of interest are six peer personality traits at the baseline (standardized). The specification of each regression is the same as in column (5) of Table 4. Robust standard errors clustered at the study group level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B: A Simple Model on Personality Change under the Influence of Peers

Consider a student who starts university education with a vector of K personality traits $\mathbb{T}_0 = [T_0^1, T_0^2, T_0^3, \dots, T_0^K]$. Personality is malleable and the student can change their traits from the baseline level to a new level $\mathbb{T} = [T^1, T^2, T^3, \dots, T^K]$. For simplicity we assume all trait levels to be strictly positive: $T_0^k, T^k > 0, \forall k \in 1, 2, \dots, K$. Changes in traits are costly and the student obtains utility from academic achievement. Personality traits may affect achievement.

Our key modeling assumption is that peers affect the *costs* of personality change, for example, through social learning or social pressure. In the following, we will derive students' optimal personality vector (\mathbb{T}^*) in the absence of peers. After that, we will derive students' optional personality when exposed to peers who have higher, lower, or the same trait level.

No peers: As a benchmark, we first examine optimal personality development without peer influences. The student chooses their optimal personality vector (\mathbb{T}^*) by solving the following maximization problem:

$$\max_{\mathbb{T}=[T^1, T^2, \dots, T^K]} F(\mathbb{T}) - C(\mathbb{T}; \mathbb{T}_0) = \sum_{k=1}^K f(T^k) - c(T^k; T_0^k), \quad (2)$$

where $f(T^k)$ denotes the production function of academic achievement that depends on trait T^k . $c(T^k; T_0^k)$ denotes the costs of adjusting the trait from the baseline level T_0^k to T^k . $f(T^k)$ allows for personality traits to have different effects on achievement.¹³

For simplicity, we assume that traits (T^k) are substitutes—academic achievement is a linear function of each trait— $F(\mathbb{T}) = \sum_{k=1}^K f(T^k) = \sum_{k=1}^K \alpha^k T^k$, where α^k denotes the marginal benefit of raising T^k . We label traits as *productive* if higher levels of these traits lead to higher achievement: $\alpha^k > 0$. Other traits may have a negative or no impact on academic achievement: $\alpha^k \leq 0$.

We assume that students find changing their personality costly (McCrae and Costa 1994). The costs of changing personality increase with the distance from the baseline level of the given trait:

$$c(T^k; T_0^k) = \begin{cases} (T^k - T_0^k)^\gamma, & \text{if } T^k \geq T_0^k \\ (T_0^k - T^k)^\gamma, & \text{if } T^k < T_0^k, \end{cases} \quad (3)$$

¹³ We can also generalize the utility function by including other outcomes that may depend on personality, such as mental health and social integration. The intuitions of our framework remain the same: some personality traits are overall helpful and generate positive returns, while other traits may generate negative net returns.

with $\gamma > 1$ capturing that it is increasingly costly to move further away from the initial level. Without loss of generality, we assume $\gamma = 2$ and that costs are identical for all traits. These simplifying assumptions about the cost structure and γ are not necessary to arrive at the model's qualitative predictions. In equilibrium, the optimal level of a personality trait is determined by marginal benefit and marginal cost:

$$\alpha^k = 2(T^{k*} - T_0^k), \quad k \in \{1, 2, \dots, K\}. \quad (4)$$

The left-hand side of the equation is the marginal benefit, and the right-hand side is the marginal cost. Equation (4) shows that the optimal level of a productive trait is always above the baseline level: $T^{k*} > T_0^k$. For traits that are not relevant for academic achievement, students' optimal strategy is to avoid any costly changes: $T^{k*} = T_0^k$.

Peer Influences: We next consider how the presence of peers affects personality development. Suppose that the student is exposed to one peer or a group of peers whose baseline personality also consists of a vector of traits $\mathbb{T}_p = [T_p^1, T_p^2, T_p^3, \dots, T_p^K]$. For each trait, peer levels can be lower, higher, or equal to the student's initial level: $T_p^k \gtrless T_0^k$. Peers may affect the costs of molding personality by acting as an example, providing a reference point, or creating peer pressure. Through these mechanisms, it becomes less costly to converge toward, and more costly to deviate from, peer levels.

We assume that the cost function remains centered at T_0^k under the influence of peers—the costs are always the lowest and equal to zero when personality change is not initiated. This assumption highlights that changing personality is difficult and requires effort, and this aversion to change is stronger than the conforming effect of peers. Without loss of generality, we can formalize the cost function in the presence of peers as:

$$c(T^k; T_0^k, T_p^k) = \begin{cases} \frac{T_0^k}{T_p^k} (T^k - T_0^k)^2, & \text{if } T^k \geq T_0^k \\ \frac{T_p^k}{T_0^k} (T_0^k - T^k)^2, & \text{if } T^k < T_0^k. \end{cases} \quad (5)$$

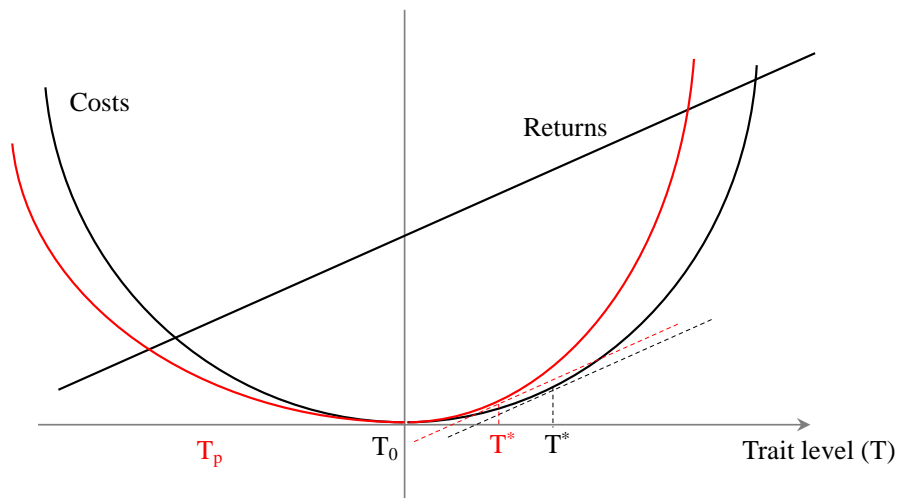
Equation (5) shows that when exposed to peers with the same trait level, $T_p^k = T_0^k$, costs are identical to the scenario without peers. When exposed to peers with higher levels of a trait, $T_p^k > T_0^k$, the costs of increasing (decreasing) own trait level are lower (higher) compared to the case without peers. Finally, we can derive the optimal trait levels as follows:

$$T^{k*}(T_0^k, T_p^k) = \begin{cases} \frac{T_p^k}{2T_0^k} \alpha^k + T_0^k, & \text{if } \alpha^k \geq 0 \\ \frac{T_0^k}{2T_p^k} \alpha^k + T_0^k, & \text{if } \alpha^k < 0. \end{cases} \quad (6)$$

Prediction: The key prediction of this framework is that for personality traits that do not affect achievement, where $\alpha^k = 0$, trait levels at the baseline are optimal (i.e., the student has no incentive to change personality) and there will be no personality spillovers. Peer personality only creates spillover effects for traits that affect academic achievement, that is, when α^k is unequal to zero. Put differently, optimal trait levels increase in peer levels if a trait is relevant for academic performance.

Figure A6 shows a stylized example of how the presence of peers affects the optimal trait level. In this example, the black curves represent the scenario without peers or when peers' level of a trait is equal to own trait level ($T_p^k = T_0^k$). The upward-sloping return curve means that the trait is productive for academic achievement. Thus, the student is incentivized to raise the trait level ($T^{k*} > T_0^k$). This academic motivation is somewhat offset by the presence of peers who have lower levels of this trait ($T_p^k < T_0^k$). Therefore, the optimal trait level decreases but remains above the baseline level.

Figure A6: Peers' Influence on Personality Development—An Example



Notes: T_0 denotes the baseline level of a personality trait, T_p denotes peers' baseline level of this trait, and T^* represents the optimal trait level. The *returns* curve is the academic output function, which increases linearly with the trait level. The *costs* curve is the cost function of personality change. The optimal trait level is arrived when the marginal cost equals the marginal return. The example shows that when exposed to peers with a lower trait level, the optimal own trait level decreases. Similarly, when exposed to peers with a higher trait level, the optimal own trait level increases.