Who Matters in College? Familiarity, Ability Signals, and Student Achievement*

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Abstract

This paper studies how social influences on academic performance evolve during transition to university when students encounter new social environments and publicly observable signals of academic ability. Using administrative data from a large, elite, and linguistically and culturally heterogeneous public university in Uganda and exploiting random assignment of students to dormitories, we examine how exposure to coethnic peers and peers with scholarship status affects academic performance. We find that greater exposure to peers from similar linguistic and cultural backgrounds improves academic achievement early in university, especially for students graduating from homogeneous high schools, but these effects fade over time. In contrast, exposure to scholarship peers generates persistent gains in academic performance that increase in magnitude throughout students' university experience. Our setting allows us to distinguish between peers' underlying academic ability and publicly observable signals of ability since scholarship status is visible, while underlying academic preparation is not. We find that exposure to scholarship peers improves academic performance regardless of those peers' underlying ability, while exposure to high-ability peers without a scholarship signal has no detectable effect. These results suggest that students respond more strongly to observable signals of academic standing than to peers' underlying academic preparation. Taken together, our findings indicate that the sources of academic influence evolve over time. Familiarity appears most important during the initial transition to university, while observable signals of academic standing become increasingly important as students progress through university.

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1 Introduction

Social interactions among college students may shape academic performance, yet the channels through which these influences arise remain poorly understood. Do students benefit from academically strong peers because they learn from or collaborate with them, or because publicly observable indicators of academic standing shape expectations, effort, and study behavior? Existing work highlights two related features of student interactions. One emphasizes that students often form meaningful social and academic relationships with peers who share familiar linguistic, cultural, or social backgrounds (e.g., McPherson et al., 2001; Carrell et al., 2013; Marmaros and Sacerdote, 2006). Another documents that exposure to academically stronger classmates, roommates, or cohorts can influence academic outcomes (e.g., Zimmerman, 2003; Carrell et al., 2009; Garlick, 2018). As Manski (1993) emphasizes, peer interactions may reflect behavioral spillovers, such as learning or effort responses to peers' actions, or contextual effects, whereby individuals respond to the observable characteristics of their peer group rather than peers' behavior. In most settings, separating these channels empirically is difficult because observable peer characteristics typically combine underlying ability with visible signals of ability.

This paper studies social influence in a setting where students enter a large national university with limited information about their new peers, but where familiar linguistic and cultural backgrounds and publicly observable indicators of academic standing are readily apparent. We focus on two dimensions of peer composition: exposure to coethnic peers and exposure to scholarship peers. Throughout the paper, we use the term coethnic to denote peers belonging to the same broad ethnolinguistic community. Specifically, our measure aggregates Uganda's more than 60 ethnic communities into 17 groups sharing common languages, cultural traditions, and historical ties. Coethnic peers may therefore provide an important source of social familiarity for students adjusting to university life. In contrast, scholarship status provides a visible indicator of academic standing. While peers do not directly observe others underlying academic preparation, scholarship status is publicly known within the institution. This setting therefore allows us to examine whether social influence is more closely associated with observable indicators of academic standing than with peers' underlying academic preparation, in the spirit of Manski (1993).

Uganda is relevant for studying coethnic interactions because of its substantial linguistic and cultural heterogeneity: roughly the size of Oregon in the U.S., it is home to more than sixty ethnic groups (Uganda Bureau of Statistics, 2016). Prior work in Sub-Saharan Africa (SSA) has linked ethnic diversity to weaker economic outcomes, such as low public goods provision and low social trust, often driven by strong preferences for interaction with peers from similar backgrounds. The fact that Ugandan ethnic groups are geographically concentrated may further reinforce these tendencies (Alesina and Zhuravskaya, 2011). These differences in social, linguistic, and cultural backgrounds may be particularly relevant in higher education settings that intentionally brings together students from across the country. In this setting, shared linguistic and cultural backgrounds are likely to be especially salient for students arriving on campus and, for the first time, interacting intensively with peers from a wide range of backgrounds.

Makerere University Kampala (MUK), Uganda's flagship public university provides a natural laboratory to study these peer dynamics. MUK admits top-performing students from across the country and strives for representation from all districts, which produces a microcosm of Uganda's ethnic diversity on campus. Having completed high school in their home districts surrounded largely by coethnics, many incoming students arrive having had relatively limited exposure to peers from other regions and ethnolinguistic communities. At the same time, the government-funded merit-based scholarship system, administered through public universities such as MUK introduce a second salient dimension of peer quality. Scholarships at MUK are allocated through two mechanisms. Some are awarded through a nationwide merit competition based solely on examination performance, while others are awarded through district-level competitions that provide geographic representation by allowing students to compete within their home districts for a fixed number of scholarship positions. This structure is conceptually similar to the combination of national and geographically allocated nominations used in admissions to U.S. service academies.

Importantly, because scholarship holders are publicly identifiable through university registration and administrative practices, students can readily observe which of their peers hold a scholarship, but cannot distinguish the specific mechanism through which it was awarded. Lastly, we measure academic performance using course grades. Because graduation rates at MUK are high, selective attrition is low and course grades are observed for nearly all students in our sample, making them a meaningful and well-measured outcome in this setting.

In the empirical analysis, we link administrative records of admissions and post-admission academic performance for students enrolled between 2009 and 2017 across on-campus majors. The administrative data we use in this paper provide students' demographic and academic characteristics, including whether they were admitted on a merit scholarship and the type of the scholarship. These records do not, however, report a student's ethnicity. We overcome this limitation by exploiting linguistic and cultural characteristics common to Uganda and SSA, where surnames reflect one's native languages and, thus, ethnicity. To do so, we apply a machine learning algorithm common in computational linguistics introduced in Cavnar and Trenkle (1994) and recently adapted by Michuda (2021) to the Ugandan context to a confidential national administrative dataset of 2016 voter registrations that includes over 14 million Ugandans. This external data set provides training data we use to build a classification model that produces predicted probabilities of belongingto each ethnic group for every individual. Rather than assigning students to a single ethnic category belonging to the top probability, we use the full predicted distribution of ethnic affiliation to construct our measure of coethnic peer exposure, allowing for uncertainty in surname-based classification.

Our causal identification of peer effects hinges on the random assignment of incoming students into dormitories (dorms), which provides exogenous variation in peer groups[1]. Specifically, a peer group in this analysis consists of students admitted to majors in the same school (e.g., School of Economics) and randomly assigned to live in the same dorm. Upon admission and conditional on gender, dorm assignment is random. Because demand for on-campus housing exceeds available capacity, dorm assignment does not guarantee residence. Indeed, less than half of incoming students ultimately reside in the dorms with the rest securing housing off-campus.

[1] Ricart-Huguet and Paluck (2023) exploit the same random hall assignment at MUK to study the effect of hall cultures on student outcomes, finding no effect on academic performance.

Since we observe assignment rather than actual residence, we interpret our estimates as intent-to-treat effects. In addition to exogenous assignment to peer groups, our econometric strategy exploits idiosyncratic year-to-year variation in coethnic composition. Importantly, dorm assignment occurs prior to students meeting their classmates or dormmates, and students' course sequences are predetermined at the time of admission, limiting scope for endogenous peer selection. We confirm balance on observable characteristics and include dorm, classroom, major, and high-school subject fixed effects to account for correlated shocks.

A number of results emerge from this analysis. A one-standard-deviation increase in coethnic share increases grades by 0.013 standard deviations, while an equivalent increase in scholarship share increases grades by 0.026 standard deviations. Because we observe dorm assignment rather than actual residence, these estimates should be interpreted as assignment-based effects that average across students experiencing different intensities of interaction with their assigned peers.

Dynamic analysis reveals different patterns for different peers over time. While both coethnic share (irrespective of scholarship status) and exposure to scholarship peers (irrespective of ethnicity) lead to higher grades early on, the coethnic peer effect fades with each passing year at MUK. This attenuation is driven primarily by non-scholarship coethnic peers. By the third year, the final year for most majors at MUK, the coethnic effect is statistically insignificant and approximately 68 percent of its first-year magnitude. In contrast, the effect of scholarship share (coethnic and non-coethnic combined) persists and strengthens over time: by the third year, it is roughly 77 percent *larger* than in the first year. Taken together, these patterns reveal a shift in the sources of social influence-from coethnic exposure early in students' university careers to exposure to scholarship peers later on.

Looking beyond average effects, we find that the influence of coethnic peers is strongest for students with limited prior exposure to other ethnic groups in their schooling environment, measured by whether they graduated from high school in their home district or from a district where their own ethnic group dominates. Consistent with the average results, this coethnic effect is concentrated in the first year, a period during which these students also exhibit lower initial academic performance, and largely declines over time.

To separate underlying academic preparation from publicly observable ability signals, we classify peers into four mutually exclusive groups: (A) high-ability students with a scholarship, (B) high-ability students without a scholarship, (C) low-ability students with a scholarship (i.e., district scholarship admits), and (D) low-ability students without a scholarship. While we observe all four types in the data, students themselves cannot directly distinguish between high- and low-ability scholarship holders; they only observe scholarship status. Relative to group (D), exposure to both high-ability (A) and low-ability (C) scholarship peers raises a student's academic performance, with effects that persist and increase over time. In contrast, exposure to high-ability peers without a scholarship signal (B) has no detectable effect on academic performance throughout students' undergraduate training.

The empirical patterns, particularly those related to scholarship peers, are difficult to reconcile with peer influence operating solely through peers' underlying academic ability. When peer ability is measured using national standardized test scores that are not publicly observable to students, we find no detectable effects of exposure to high-ability peers in the absence of a scholarship signal. In contrast, exposure to scholarship peers improves academic performance regardless of whether those peers are drawn from the upper or lower end of the admitted ability distribution. Since scholarship status is publicly visible while underlying academic preparation is not, these findings are more consistent with students responding to observable indicators of academic standing than to peers' underlying academic ability. Although we do not directly observe the behavioral mechanisms through which these responses occur, the results suggest that publicly observable signals of academic achievement may play an important role in shaping academic outcomes during university.

Our results on coethnic peer exposure are consistent with social familiarity channels. In particular, the positive effect of coethnic peers for students with limited prior exposure to non-coethnics suggests that shared linguistic and cultural backgrounds may ease the transition into a large and unfamiliar university environment. Over time, however, the estimated coethnic peer effect attenuates and eventually disappears, a pattern potentially consistent with the diminishing importance of initial social familiarity as students spend more time in a diverse university environment. As students interact repeatedly in classrooms and campus life around shared academic goals, social interactions may shift away from familiarity-based interactions toward academically relevant and observable characteristics, such as institutionally signaled ability.

We contribute to several strands of literature. A large literature documents that exposure to higher-quality peers can influence academic performance, major choice, and social behaviors in college, primarily using random roommate or cohort assignments in Western universities (e.g., Zimmerman, 2003; Sacerdote, 2001; Foster, 2006; Carrell et al., 2009; Mehta et al., 2018). These studies typically measure peer quality using pre-treatment academic characteristics. A small number of recent studies examine college peer effects in Africa, primarily using data from a South African university (Garlick, 2018; Corno et al., 2019), but these papers focus on different questions, such as tracking versus random assignment or changes in racial stereotypes, and do not distinguish between coethnic peer effects, academic ability, and ability signaling in highly heterogeneous settings. Much less is known about social interactions in settings where students arrive from geographically concentrated and socially distinct backgrounds and are suddenly exposed to a substantially broader set of peers.

More importantly, existing work does not distinguish between peers' latent academic ability and publicly observable signals of ability, making it difficult to separate behavioral peer effects from contextual responses to peer composition. Specifically, we exploit the institutional visibility of scholarship status to provide evidence that is more consistent with contextual peer effects in the sense of Manski (1993) than with peer effects operating through underlying academic ability. We show that exposure to scholarship peers improves outcomes even when those peers are drawn from the lower end of the admitted ability distribution, suggesting that publicly observable scholarship status may matter more than peers' underlying academic preparation.

Additionally, our findings on scholarships speak to a growing literature on academic credential labels as informational signals. Barrera-Osorio et al. (2024) study a program where some students receive a merit scholarship label while others receive an identical transfer labeled as need-based aid. Merit-labeled recipients outperform their need-based counterparts, even among students eligible for both programs, pointing to the label rather than student selection channel as the mechanism. Moreira (2019) documents a related pattern in Brazilian secondary schools, where a public math recognition award generates spillovers to classmates: students who observe a winner update their beliefs about what academic goals are within reach. In both cases, the label affects those who directly receive or observe it, in short-run classroom settings. Our setting differs in that students are exposed to scholarship labels through assigned peer groups over an entire undergraduate degree program, allowing us to examine how these influences evolve over time.

Lastly, we document meaningful coethnic peer influence, showing that exposure to coethnic peers improves academic performance regardless of ability initially in students' university undergrad tenure, especially for those with limited prior exposure to students from different linguistic and cultural backgrounds. In doing so, this paper contributes to the broader literature on linguistic and cultural heterogeneity and economic and social outcomes in Sub-Saharan Africa (Easterly and Levine, 1997; Habyarimana et al., 2007; Alesina and Zhuravskaya, 2011; Miguel, 2004; Gisselquist et al., 2016; Alesina and La Ferrara, 2000; Håkansson and Sjöholm, 2007; Hooghe, 2007). While much of this literature studies aggregate outcomes such as public goods provision, social trust, or conflict, our analysis focuses on a distinct but closely related question: how ethnic composition within peer groups shapes academic performance in higher education institutions setting characterized by high ethnic heterogeneity and geographic segregation. Understanding how coethnic social interaction operate in this context is particularly relevant for universities, where admissions, housing, and cohort formation may explicitly or implicitly influence patterns of cross-ethnic interaction among young adults.

2 Institutional Background and Empirical Setting

2.1 Ethnicity in Uganda

Uganda has more than 60 ethnic groups, which are commonly classified into three broad ethnolinguistic families: Bantu, Nilotic, and Central Sudanic (UBOS, 2006). According to the 2002 Uganda Population and Housing Census, the nine largest ethnic groups account for approximately 71% of the national population. Ethnicity is often readily identifiable through language, surnames, accents, and region of origin. This pronounced linguistic and cultural heterogeneity is also characterized by distinct geographic segregation (see Appendix A.1 Figure A.1). Indeed, Alesina and Zhuravskaya (2011) rank Uganda the 4^{th} most segregated country in the world based on a spatial segregation index.

Historic migration and ethnic kingdoms drive these segregated geographic patterns. Bantu-speaking groups are clustered in the country's South, Central, and Western parts, while Nilotic and Central Sudanic peoples are clustered in the Northern and Eastern parts. For purposes of the analysis that follows, we retrace current ethnic borders to historic kingdoms (see Appendix Section A.1). Inter-regional migration is limited except for rural-to-urban migration for economic opportunities into the capital, Kampala. By contrast, rural-to-rural migration across ethnic clusters is relatively uncommon and appears limited by both economic opportunities and cultural ties and barriers.

Although ethnic divisions predate colonial Uganda, some were exacerbated during British colonialism (Tornberg, 2013). The first post-independence government made efforts to reduce the importance of ethnic identities by abolishing historic kingdoms and preaching national unity, an effort that met with resistance from some kingdoms, especially those with economic or political power. The current government has permitted ethnic groups to reestablish their historical kingdoms, and a number have done so. While current inter-ethnic competition and recent historical conflicts can be traced to political and sometimes historical factors (Mamdani, 2001), cross-group tension is generally not as intense as in neighboring countries.

Although English is the official language of Uganda and is intended to be used in public administration and as the primary language of instruction in schools, native languages are widely used in day-to-day lives, especially in regional public offices and schools. Given Uganda's high ethnic diversity, native linguistic diversity is correspondingly high.[2] Differences between native languages are correlated with geographic distance, such that individuals may partially comprehend the languages of neighboring ethnic groups. However, because of long-standing geographic and ethnic segregation, mutual comprehension across ethnic regions is limited and generally confined to neighboring groups. We use this linguistic variation to predict ethnicity in Section 3.2.

2.2 Ugandan Higher Education and Makerere University Kampala

Although Uganda has one of the youngest populations in the world, post-secondary school education is low: the post-secondary enrollment rate for college-age Ugandans was only 6.9% during the 2017/18 academic year (NCHE, 2018). Nine public and 44 private universities offered degree programs during the 2018/19 academic year (NCHE, 2018), of which Makerere University Kampala (MUK) ranks first in quality and size.

MUK is well-known in the SSA as it is one of the oldest universities in the region, established in 1922 as a technical school to facilitate the training of workers for the British colonial government. Centrally located in Kampala, it admits students from all over the country, making the ethnic distribution at MUK largely mirror that reported in the census (some groups are overrepresented as shown in Appendix Figure A.2).

Because many Ugandan students grow up and attend secondary school in geographically concentrated ethnic communities, enrollment at MUK often represents their first sustained interaction with coethnic peers. MUK's centralized peer group assignment mechanisms (discussed in Section 2.3.3 below) further shape these initial interactions, making the university a natural setting in which to study how familiarity with peers from similar backgrounds influences academic adjustment.

2.3 Empirical Setting

The Ugandan public university and pre-collegiate nationwide system offer a unique setting that we exploit to identify peer effects. National pre-collegiate exit examinations and public university admissions are centrally administered. Second, the Uganda Examination Board, an

[2] WorldAtlas reports Uganda's language diversity index of 0.93, which indicates that most Ugandans speak at least one native language.

organization separate from MUK, administers national secondary school exit exams and runs an algorithm for all MUK admissions. Because admission decisions are generated externally, university administrators have no discretion over the academic composition of incoming cohorts.

2.3.1 Admissions, Major Structure, and Peer Exposure

A student's choice of major is determined at the time of offer, typically a few months before enrollment, and depends on the applicant's preference set, exam performance, and program cutoffs. Each major is housed within a school.[3]

Students' course list and sequence are largely predetermined upon admission, leaving little scope for post-enrollment sorting. Moreover, students cannot sort into different sections within the same major, as parallel sections do not exist.[4] Thus, students admitted to the same major take all first-year and most of second and third courses together, following this pre-specified sequence. Students also interact with peers from other majors, especially within the same school. In addition, students within a school routinely share common spaces, including computer laboratories, libraries, study rooms, and food canteens, intensifying repeated peer exposure.

Lastly, university admits come from a secondary school system where students may specialize in different A-level subject combinations, often chosen to align with their intended fields of study and prior academic performance (e.g., Physics-Chemistry-Mathematics History-Economics-Geography). It is common for students admitted to the same major to have taken different subjects in high school (HS). Because subject availability and educational quality also differ across regions and schools, students may arrive at university with heterogeneous academic preparation even within the same cohort.

2.3.2 Admission Schemes and Ability Signals

Students are admitted under three primary funding schemes: the direct National Merit (NM) Scholarship track, District Merit (DM) Scholarship, and the self-funding (private) scheme. NM Merit Scholarships are awarded strictly on the basis of performance in national examinations and cover tuition and living expenses at public universities. These awards are allocated directly to the highest-performing applicants nationwide, independent of region or secondary school attended. As such, NM scholarship status provides a strong ability-based signal at the time of entry.

The DM Scholarship scheme incorporates a region-based policy that adjusts admission cutoffs and criteria for applicants graduating from secondary schools in their home districts who do not qualify for the national merit scholarship. The objective of this policy is to increase student representation from all districts of Uganda. This mechanism is merit based, but students compete withing their home region rather than at the national leve. Similar to the NM scholarships, DM scholarships provide full tuition support and living allowances.

[3]Schools are locally referred to as a faculty or sometimes an institute; we adopt the term school throughout.

[4]Some programs are offered in separate day and evening cohorts that are administratively treated as distinct majors with limited cross-cohort interaction. We exclude evening, extension, and off-campus programs from the main analysis because these students typically live off campus and are not subject to standard dorm assignment; though including them in the analysis does not materially affect the results.

Important for our study, students admitted through the NM and DM scholarship mechanisms are administratively classified the same upon admission. As a result, both scholarship recipient types receive the same publicly observable signal of academic ability to peers as a merit scholar. That is, scholarship status is administratively recorded and publicly observable on campus.[5] This visibility makes scholarship status, and hence a credible proxy for relative academic ability, salient to peers from the start of university life.

2.3.3 Dorm Assignment and Defining Peer Groups

MUK randomly assigns newly admitted students to gender-specific dorms. Of these nine large dorms, three are female and six are male. The assignment is done centrally by Student Affairs using a simple randomization script. However, dorm assignment does not ensure that students receive a room and bed assignment because there are more students assigned than there are beds available. Some majors – those perceived to be especially rigorous (e.g., medicine) – are guaranteed on-campus accommodation in their assigned dorms.[6] Other students must apply for residency in their assigned dorm and will receive a spot if capacity allow. Since subsequent bed allocation is handled at the dorm level rather than conducted centrally, we observe initial dorm assignment but not actual residence or specific room allocation.

Less than half of incoming students ultimately live on campus, while the rest must arrange for off-campus housing. Still, initial dorm assignment may continue to shape campus life for these students as assigned students can and typically do use shared dorm spaces, including dining and entertainment facilities. Moreover, if they choose to participate, many extracurricular activities (e.g., student government and sports) are organized by dorm assignment.

While a peer group generally consists of individuals with shared or similar characteristics who interact in social or other settings, in empirical applications specific definitions of peer groups vary by context. For example, Carrell et al. (2009) define a peer group as a squadron at the US Air Force Academy; Foster (2006) considers a peer group to be students living on the same dorm floor; and pre-collegiate studies, such as Carrell and Hoekstra (2010), use a cohort definition. Here, we define a peer group as students in the same school and assigned to the same dorm as illustrated in Figure 1. The average peer group size in our sample is 26, comparable to the peer group size of approximately 30 used in Carrell et al. (2009). As described above, this peer group definition fits the MUK setting but is also empirically feasible since, unlike some prior studies on college peer effects, we do not observe specific room allocations and residence status.

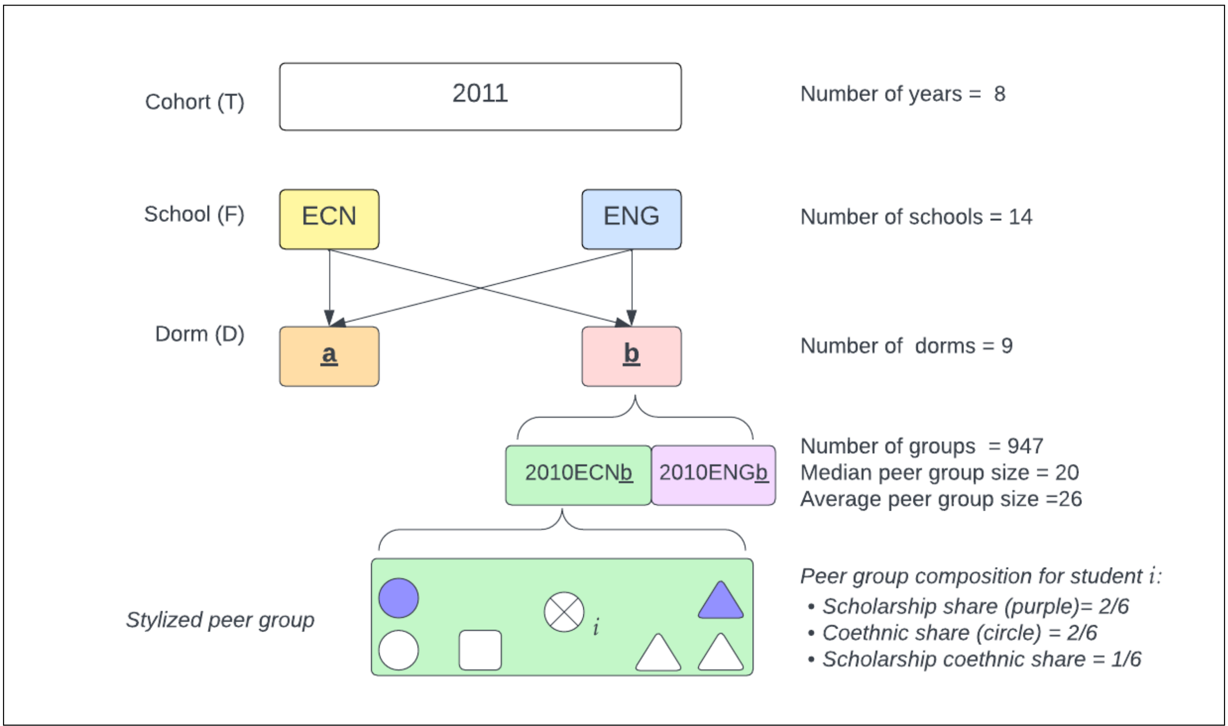
2.3.4 Are Dorm Assignments Truly Random?

Our identification of peer effects hinges on random dorm assignment. Student Affairs at MUK is explicit about its random dorm assignment by gender. We replicate this claimed assignment process to test whether the assignments we observe in administrative data reflect the statistical

[5] For example, the 2017 scholarship students are assigned registration numbers of the form `17/U/XXX`, whereas self-funding students receive registration numbers of the form `17/U/XXX/PS`. These identifiers appear on official registration forms, examination lists, and departmental records that are routinely accessible to students and faculty.

[6] These students can opt to live off campus and receive a housing allowance from the university instead.

Figure 1: Cohort-School-Dorm Peer Group Construction



Notes: This diagram illustrates a peer group definition. Students in these defined peer groups are much more likely to interact regularly with each other, including those of the same or different ethnicity. scholarship students are defined as those on merit scholarships (direct and district), a status that is widely known among all students.

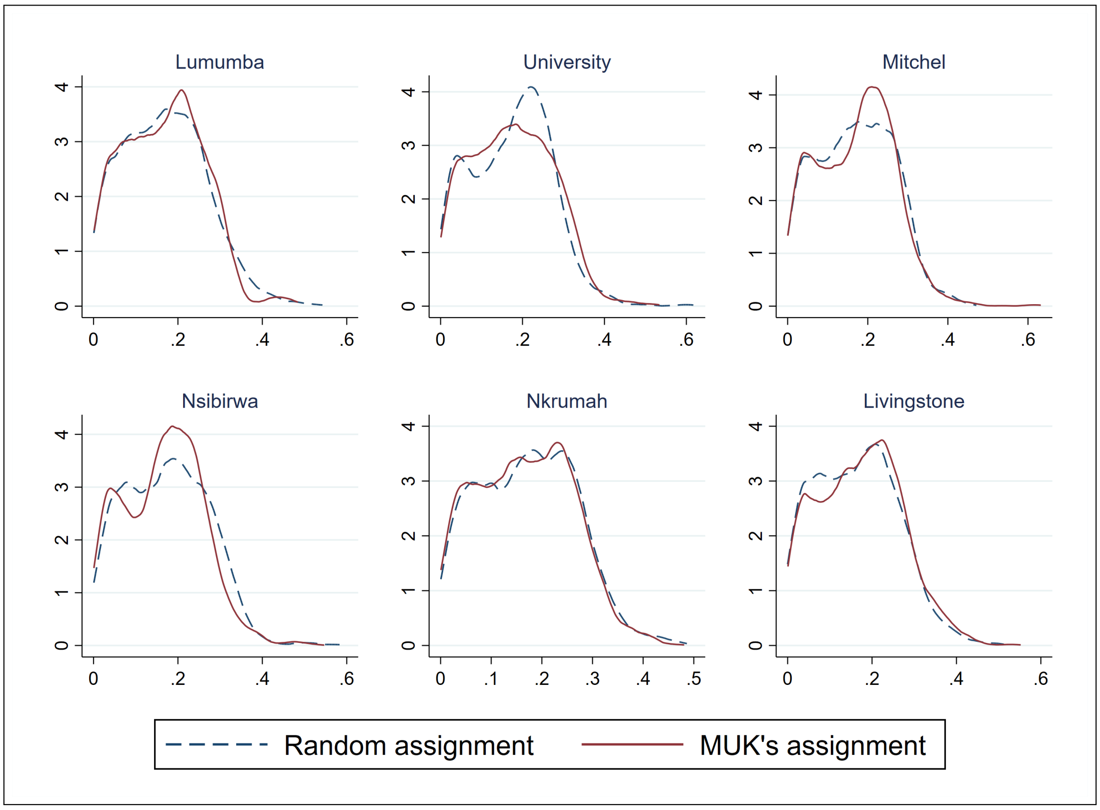
characteristics of a random process. Specifically, we simulate random dorm assignments, stratified by cohort and school, and construct simulated peer groups using the definition outlined above. We then compute our primary peer measures – the share of coethnic and scholarship peers – and visually compare distributions of these simulated measures to those constructed from the MUK administrative data. Discrepancies between the observed and simulated distributions may suggest departures from the randomization protocol in practice (e.g., systematic sorting, non-random influences, etc.)

We present output of this exercise in Figure 2. Panels A and B present kernel density estimates of the coethnic and scholarship shares, respectively, under both the actual and simulated random assignment in male dorms. A similar figure for female dorms is presented in the Appendix Figure A.3. In both panels, the actual and simulated distributions are strikingly similar across all dorms, suggesting that the realized dorm assignment does not exhibit meaningful sorting along these dimensions. The absence of divergence between the two distributions supports the plausibility of treating MUK's dorm assignment as random, at least with respect to these observable characteristics. We confirm this using a balance test in Section 4.1.2.

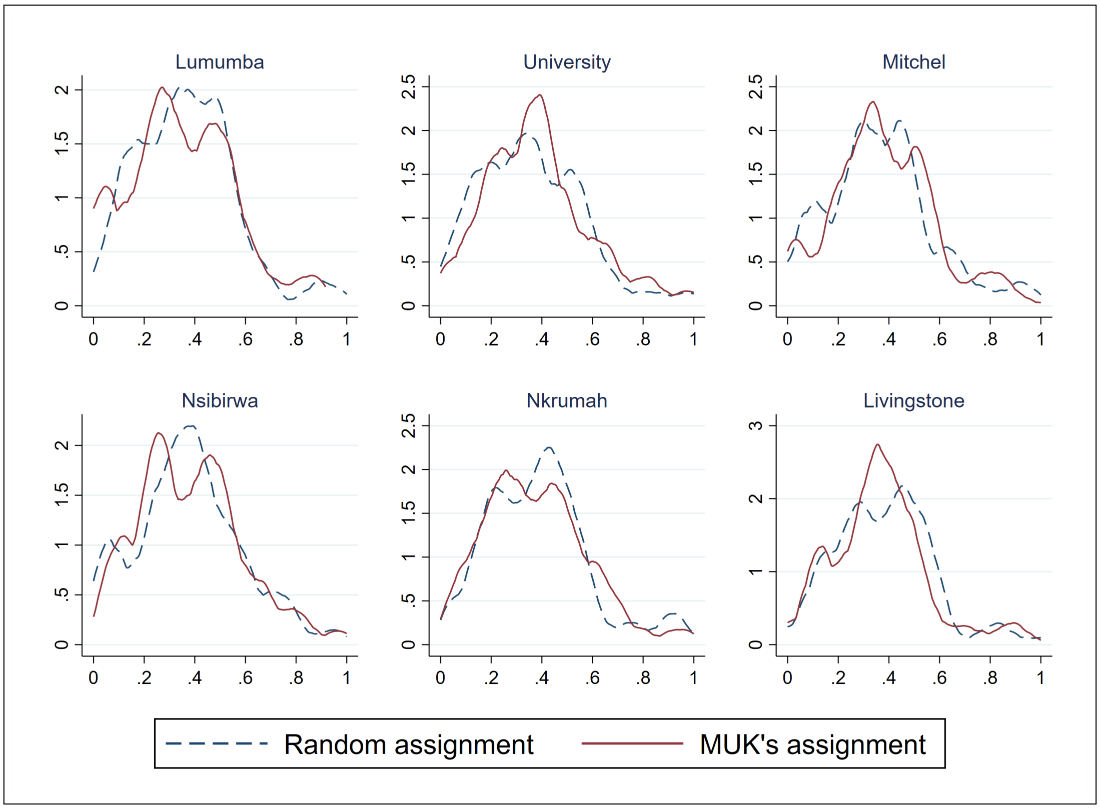
2.3.5 Identifying Peer Effects at MUK

Three classic problems complicate the estimation of peer effects in the literature: self-selection (Hoxby, 2002), endogeneity (Manski, 1993), and correlated common shocks (Bramoullé et al., 2009). Self-selection arises when people choose to join a group based on some pre-treatment characteristics: "*if everyone in a group is high achieving, many observers assume that achieve-*

Figure 2: Comparing random to MUK's assignment: Male dorms



Panel A: Coethnic share



Panel B: Scholarship share

Notes: The distribution of coethnic and scholarship under MUK actual dormitory assignment versus a simulated random assignment for male dorms. Coethnic share is defined as the proportion of peers sharing the student's ethnic group, and scholarship share is the proportion of peers on merit scholarship.

ment is an effect of belonging to the group instead of a reason for belonging to it" (italics added; Hoxby, 2002, p. 57). In higher education, self-selection exists because students can often choose instructors (classrooms), majors, as well as where and with whom they live. To avoid this self-selection problem, the peer effects literature has exploited either (i) specific contexts that use random assignment to roommates (Sacerdote, 2001; Zimmerman, 2003; Foster, 2006) or squadrons (Carrell et al., 2009), or (ii) the natural variation in cohort composition that reflects the local source population (e.g., share of girls or minorities in the classroom) (Hoxby, 2000). As described above, our MUK application provides useful random variation in dorm assignment and the fact that the major and course list are predetermined, thereby alleviating self-selection concerns.

Endogeneity arises from the classic reflection problem, in which a student's own behavior both affects and responds to his/her peers. This feedback loop implies that the student's and his/her peers' outcomes are simultaneously determined. One approach in the literature is to use preexisting characteristics that are exogenous to the dependent variable, such as race, gender, or prior academic scores (e.g., SAT). Following this approach, we exploit two pre-collegiate characteristics–ethnicity and scholarship status–which are both determined before university enrollment and therefore cannot respond to contemporaneous academic performance. In Uganda, ethnic background is largely fixed at birth, and university admissions do not involve ethnic quotas or affirmative action based on ethnicity, eliminating incentives for strategic misreporting. Moreover, rather than relying on self-identification, we predict ethnicity using linguistic characteristics (Section 3.2), further reducing concerns that ethnic classification is endogenous to university outcomes.

Finally, correlated common shocks can generate the illusion of peer effects when correlated outcomes reflect shared exogenous shocks rather than true peer interaction. In our setting, although all students are enrolled at MUK, academic performance may be influenced by shocks operating at the cohort, major, dormitory, or classroom level—for example, instructor quality, grading standards, or dorm-level environments. To address this concern, our main regressions (Section 5.1) include dorm fixed effects, major fixed effects, and classroom (course-by-year) fixed effects, which absorb shocks common to students sharing the same institutional environment. Conditional on these controls, identification relies on within-classroom variation in student's peer (school-by-dorm) composition.

3 Data

The analysis in this paper uses several data sources. We use university applications to characterize student preparation and demographics prior to students arriving on the MUK campus. We also use student transcripts to measure the academic performance of those who accept admission and enroll at MUK. Finally, we leverage student surnames to predict ethnicity using a machine learning model trained on voter registration records.

3.1 Academic and Demographic Characteristics

3.1.1 Application and Admission Data

We observe student application and admissions records for eight entering cohorts (2009 and 2011–2017).[7] These administrative data include student name, application type, admission, and offered majors, as well as age, national examination scores, subject combinations, and religious affiliation. We also observe students' admission track that falls three primary funding schemes: national scholarship, district scholarship, and the self-funded (private) scheme. For each student, we observe both scholarship status and scholarship type.

3.1.2 University Academic Performance

For our outcomes measures we collected student-level transcripts from the 14 schools within MUK. Each student's transcript lists all courses taken by semester, including credit hours, and performance in standardized percentages throughout at students academic career at MUK.

Grading of student performance in classes is standardized at MUK: letter grade cutoffs are common across all majors. Instructors submit each student's performance on a 0-100% scale, which is mapped to final letter grades using the common breakdown. Most majors at MUK are designed to take three years to complete and thus require students to carry a heavy load of courses per semester (6-10).[8]

3.2 Ethnicity

University applications and admissions do not record students' ethnic backgrounds. We address this limitation by exploiting linguistic differences reflected in surnames. Ugandan surnames are closely tied to native languages and can therefore provide information about ethnic background. This naming pattern is not random or unique to Uganda. Historically African parents chose names intentionally.[9]

The meaning of most Ugandan surnames can be traced to the father's tribal clan, religious affiliation, or the prevailing conditions at the time of birth, among others. These are the linguistic characteristics we use to predict each student's ethnicity. Our linguistic approach is intended to capture underlying differences in culture, background, and social norms that are often associated with ethnic affiliation. Data Appendix A.1 describes how we trace ethnic boundaries from current administrative units to historical kingdoms.

Using surnames to trace one's ethnicity is not new in economics or other fields. Surnames have been used in mobility studies to trace wealth across generations within a family in the West (Barone and Mocetti, 2016; Clark and Cummins, 2015), caste membership (Bhusal et al., 2020), ancestral (Monasterio, 2017), and most relevant to predict or proxy ethnic background across several countries (Eifert et al., 2010).

[7] We exclude the 2010 cohort because dormitory assignment data are unavailable for that year.

[8] About 86% of students listed in our transcript data exit the dataset at end of their third year confirming the fact that most degrees graduate at end of third year.

[9] Trevor Noah discusses the same pattern in South Africa in his memoir Born a Crime (Noah, 2016). Also, see https://www.bbc.com/news/world-africa-37912748 (accessed on 07/15/2023), for another example.

3.2.1 Predicting Ethnicity

There are two common approaches to predict ethnicity following the literature. First, using frequencies to predict probabilities as in Bhusal et al. (2020).[10] Alternatively, some studies train a machine learning algorithm on an appropriate dataset that includes both names and known (or a reliable proxy of) ethnic background.

The second method, which is our preferred approach, leverages text categorization procedures summarized in a seminal paper by Cavnar and Trenkle (1994) and recently applied in Michuda (2021) and Monasterio (2017) begins by breaking a surname into N-grams. Taking "AHIMBISIBWE" as an example, this method breaks this surname into 1-grams ("A", "H", " M", "B", "I", etc); 2-grams ("AH", "HM", "MB", "BI", etc); 3-grams ("AHI", "HIM", "IMB", etc..) and so forth. The algorithm then counts the number of frequencies each N-gram appears in a surname and in each region and weights using the term frequency. The most common weighting approach used in linguistics is the term frequency-inverse document frequency (tf-idf) that combines approaches developed by Luhn (1957) and Jones (2004). We then apply gradient boosting on N-grams and tf-idf features on an external dataset described in Section 3.2.2, producing a classification model that we apply to students' surnames.

This algorithm predicts N probabilities if we have N ethnicities. Given lexical similarity between some languages, the predicted probabilities for a specific surname can be between zero and 1. We, therefore, interpret the predicted probabilities as a measure of 'confidence' that a student belongs to a particular ethnicity.

We prefer this classification method when predicting ethnicity for several reasons. First, by following the tf-idf weighting procedure, the algorithm picks each surname's most unique linguistic characteristics. Second, as opposed to using simple frequencies, it does not require an exact match in the name training database. Third, because the model produces a full set of predicted probabilities across all N ethnic categories, it allows us to observe the degree of uncertainty in classification and exploit variation in cases where a name may plausibly belong to multiple groups. We show how we use feature to construct coethnic share in the following section.

3.2.2 Training Data

We use confidential nationwide voter registration to train the machine learning model (gradient boosted). These data contain names, voter ID numbers, age, sex, polling station, and area of residence. The area of residence is given for all units of administration parish, sub-county, county, and district. We link these voter data to spatial administrative and public data containing ethnic boundaries traced from historic kingdoms described in Appendix A.1.

[10] This approach computes simple probability using the frequency of each surname. Suppose $\{E_1, E_2, ..., E_n\}$ is a set of all ethnicities in a population. Also, suppose $N_{s\in E_i}$ is the number of times a surname, s, belongs to an ethnicity, E_i. Then the probability of belonging to a particular ethnicity is computed as:

$$\frac{N_{s\in E_i}}{\sum_{\forall n} N_{s\in E_i}} \tag{1}$$

To illustrate, consider the surname "AHIMBISIBWE": it appears 17,559 times in the name training data, of which 13,904 occurrences in the Ankole/Kiga region/ethnicity. Therefore, there is a 79.2% probability that a student with the surname "AHIMBISIBWE" is of Ankole ethnicity.

People register to vote from a polling station within their parish of residence. Moreover, in many cases, people who live in cities outside their areas of origin often register to vote in their ancestral homes. Because voter registration is manual, it only takes place once in five years. A few Ugandans own cars to travel, so most walk, as long-distance public transportation is costly. Thus, the cost of registering to vote in a village different from their residence is high.

To assess classifier performance, we evaluate the model on a held-out 20 percent test set of voter records not used during training. The classifier achieves 74.0% accuracy, with weighted precision of 73.5% and F1-score of 73.5% across all 17 groups. As a reference, uniform random guessing across 17 groups would yield approximately $1/17 \approx 5.9\%$ accuracy, so the classifier performs roughly 12 times better than chance. Among MUK students, the median predicted probability for the top-assigned cultural group is 0.80, suggesting the classifier assigns most students to their predicted group with high confidence. Rather than hard-classifying students into a single ethnic category, we use the full predicted probability vector Π_{ei} for all students, propagating classification uncertainty directly into the coethnic share measure as described in Section 3.2.3.

3.2.3 Constructing Coethnic Share

We then compute the share of coethnic peers using the predicted probabilities in two ways. First, we use a single ethnic background method by assigning each individual a single ethnicity category corresponding to the group the algorithm is most confident about (i.e., highest probability per surname). This is common in studies employing machine learning algorithms to predict ethnicity, religion, or area of origin in the literature. This method assumes that individuals' top predicted ethnicity corresponds to the 'true' ethnicity and treats ethnicity as a categorical variable without considering potential misclassification errors.

Second, we use a joint probability estimation where we consider all the probabilities that a given surname belongs to different ethnicities. This method acknowledges potential misclassification error associated with using categorical variables for ethnicity and estimates the probable fraction of coethnic peers in a peer group by considering the joint probabilities of two individuals belonging to the same group.

Thus, student i's share of coethnic peers in a peer group G, S^E_{iG} is computed as:

$$\text{Using category assignment}: \quad S^E_{iG} = \frac{\sum_{\forall k \neq i} \text{Number of coethnics}}{N_G - 1} \tag{2}$$

$$\text{Using joint probability estimation:} \quad S^E_{iG} = \frac{\sum_{e=1}^{17} \sum_{\forall k \neq i}^{N_G - 1} \Pi_{e_i} \Pi_{e_k}}{N_G - 1}, \tag{3}$$

where N_G is the peer group size, Π_{ei} is the predicted probability that an individual i belongs to ethnicity group e. We collapse ethnicities to 17 ethnic/language groups as described in Appendix A.1.

For our main results, we adopt a probabilistic approach (Eq: 3) to measuring coethnic peers as it leverages the full distribution of predicted ethnic probabilities rather than imposing a hard classification. Our approach retains the probabilistic nature of machine learning predictions, allowing for partial ethnic affiliation across categories, such as surnames that may cut across multiple ethnicities and about which our prediction algorithm is less confident, thus limiting potential misclassification inherent in hard assignments. The probabilistic approach preserves more of the true variation in coethnic composition across peer groups and yields a regressor that better reflects exposure to coethnic peers. Nevertheless, we present estimates using hard classification based on the highest predicted probability and find that coethnic social interaction still exists, though smaller in magnitude due to the narrower variation in the discrete measure (Appendix A.6, Table A.6). Appendix A.6.3 provides a further check: progressively restricting peer contributions to higher-confidence predictions leaves the coethnic peer effect unchanged, confirming that the result is not driven by low-confidence ethnicity classifications (Figure A.6).

Throughout the paper, we use the term "coethnic share" to denote the estimated share of peers belonging to the same broad ethnolinguistic community. Specifically, our classification aggregates Uganda's more than 60 ethnic communities into 17 groups sharing common languages, cultural traditions, and historical ties. When referring to a student's "ethnicity," we mean the student's most probable ethnolinguistic group, defined as the group with the highest predicted probability based on his or her surname.

3.3 Descriptive Statistics

Table 1 provides summary statistics by student, (constructed) peer group, and course. About half of the student population is female. About 24% are enrolled through national scholarship, and 7% are enrolled through the district scholarship, while the rest are enrolled through the non-scholarship track. Most of the students have a declared religion with a majority Catholic or Anglican. The average age of incoming students is 20. Lastly, about 40% of the students in the sample graduated from a high school located in their home district . Course grades average around 67%, which corresponds to a C+ ("Fairly Good") grade.

The average constructed peer group size is 26, which happens to align with peer group sizes in non-roommate peer effects studies (e.g., the average peer group size are about 30 in Carrell et al. (2009) and 30 in Foster (2006)). About 90% of the peer groups have 50 or fewer members, as shown in Appendix Figure A.7.

The average coethnic share is .17, but this masks substantial heterogeneity across ethnic groups. Figure 3 plots CDFs of the coethnic share of peer groups for the largest 10 ethnic groups. While many peer groups of students from the two largest ethnic groups (Banganda and Banyankore) have a coethnic share greater than 0.2, students from other ethnic groups are likely to have far fewer coethnics in their peer groups. Given the average peer group size of 26, students from the largest ethnic groups often have 5-8 coethnics in their peer groups; students from other ethnic groups may have 1-3 coethnic peers.

Table 1: Descriptive Statistics

	N	Mean	SD
Panel A: Student characteristics			
Female	25,351	0.48	0.50
National scholarship	25,351	0.24	0.43
District scholarship	25,351	0.07	0.26
Home district HS	25,351	0.40	0.49
Age	25,312	20.16	1.42
Religious Identity			
Anglican	25,351	0.37	0.48
Catholic	25,351	0.32	0.47
Muslim	25,351	0.09	0.29
Panel B: Peer group variables			
Coethnic share	947	0.17	0.11
Scholarship share	947	0.34	0.22
Scholarship coethnic share	947	0.06	0.06
Non-scholarship coethnic share	947	0.12	0.09
Scholarship non-coethnic share	947	0.28	0.19
Panel C: Course level			
All course grades	986,001	67.58	9.60
Year-one course grades	319,787	66.92	9.66
Year-two course grades	339,556	67.40	9.80
Year-three course grades	326,658	68.43	9.26

Notes: Data are from MUK and are restricted to students admitted to non-extension majors 14 schools for 2009-2017, excluding 2010. A peer group comprises students admitted to majors within a school in the same year and assigned to the same dorm.

4 Empirical Strategy

4.1 Mean Effects

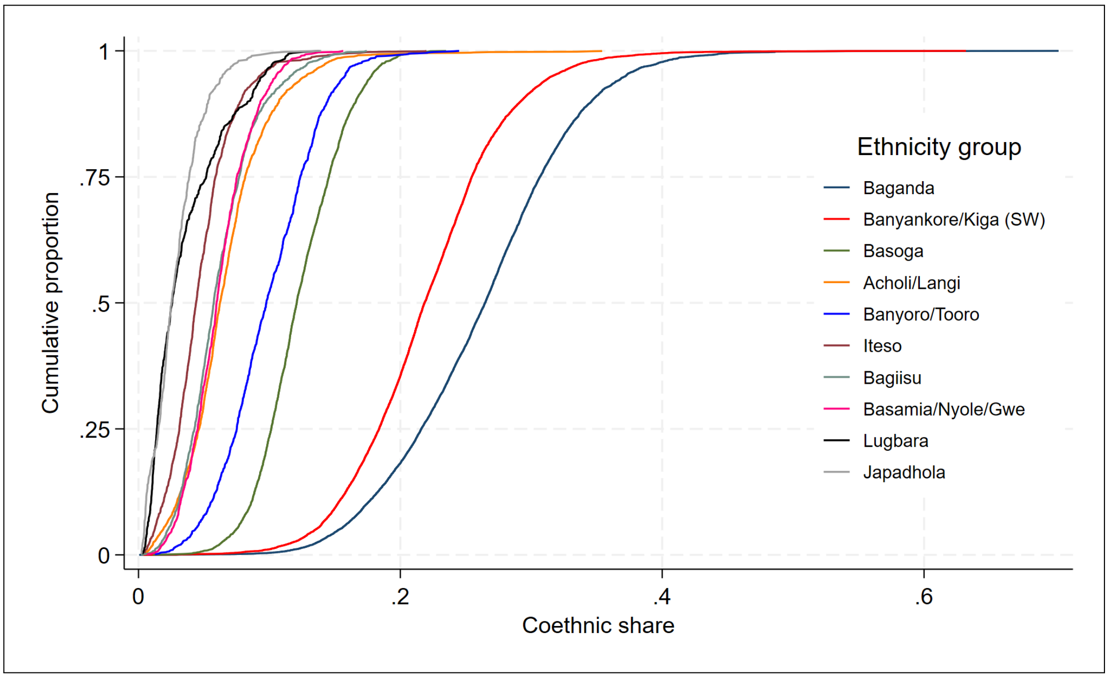
4.1.1 Direct Effects of Coethnic and Scholarship Peers

We refer to peer groups as those students admitted to majors in the same school f, randomly assigned to the same dorm d, and starting at MUK in the same cohort year t. To simplify notation throughout, we use G as shorthand for fdt when indexing peer groups. To estimate the direct effects of coethnic or scholarship peers on academic outcomes, we use a model that exploits exogenous variation in coethnic and scholarship composition of peer groups and controls for characteristics of student i and peer group G as well as a host of fixed effects:

$$y_{ijcG} = \beta_0 + \phi_1 S^E_{iG} + \phi_2 S^S_{iG} + \beta_2 \mathbf{X}_{iG} + \beta_3 \bar{\mathbf{X}}_G + \delta_j + \alpha_c + \lambda_d + \theta_m + \gamma_s + \varepsilon_{ijcG}, \qquad (4)$$

where y_{ijcG} is the standardized grade earned by student i of ethnicity j and belonging to peer group G in course c. S^E_{iG} is the coethnic share of in i's peer group G (see equation (3) in Section 3.2); S^S_{iG} is the share of scholarship peers (district and national combined). This specification controls for i's most probable ethnic group δ_j, a vector of student i's background characteristics $\mathbf{X}_{iG}$ including scholarship status, a vector of exogenous characteristics of peer group G, $\bar{\mathbf{X}}_{G,-i} = \frac{\sum_{k \neq i} \mathbf{X}_{kG}}{N_G - 1}$, dorm λ_d, major θ_m, and HS subject combination γ_s. We cluster

Figure 3: Distribution of coethnic share across peer groups



Notes: Data used to produce these distributions are from MUK and are restricted to students admitted to on campus day majors for 2009–2017, excluding 2010. Coethnic share is computed as the leave-me-out proportion of coethnic peers in a group. This figure plots the 10 largest ethnic groups by the total number of MUK students (out of the 17 total cultural/ethnic groups).

standard errors at the peer group to account for the potential error correlation across individuals in a group.

Our coefficients of interest in this specification are ϕ_1 and ϕ_2, which capture the effect on a students' academic performance of the coethnic and scholarship peers in their group G, respectively. Our identification strategy for estimating these peer effects relies on the assumption that, conditional on a rich set of individual and institutional controls, peer composition is as-good-as randomly assigned. Formally, as we show in Appendix Section A.2, because of random dorm assignment, exposure to coethnic peers and scholarship peers is independent of both observed and unobserved determinants of academic outcomes, conditional on ethnicity, ability, classroom, major, gender/dorm, cohort, and HS subject combination. Ignoring subscripts for simplicity, we assume that:

$$(S^E, S^S) \perp (U, \mathbf{X}) \mid \text{Ethnicity, Scholarship, Classroom, Major, Gender/Dorm, Cohort, HS subjects.}$$

Intuitively, after conditioning on these characteristics, remaining differences in peer composition arise from MUK's random dorm assignment rules rather than from student choice or unobserved sorting, allowing us to interpret the estimated peer effects as causal.

Including our set of fixed effects helps ensure the estimated effects are not driven by correlated shocks operating at different institutional levels. First, classroom fixed effects, defined at the course-by-year level, absorb all shocks common to students enrolled in the same course offering, such as instructor practices, grading standards, and course-specific academic environments. This ensures that peer effects are identified from within-classroom variation rather than from differences across courses or cohorts. Second, major-level fixed effects account for persistent differences in academic environments across fields of study, including selectivity, size, and share of students in a given major on scholarship. Third, dorm fixed effects control for shared residential environments that may jointly influence peer interaction and academic performance, such as differences in living conditions or other dynamics among students in the same dorm. Finally, HS subject combination fixed effects capture heterogeneity in pre-university academic preparation and the fact that certain regions and schools are more likely to offer or excel in specific subject combinations than others among students entering the same major, isolating peer effects from differences in readiness for university coursework.

4.1.2 Evidence Against Selection

Beyond the institutional features that safeguard us from non-random sorting described in Section 2.3.5, one residual concern warrants discussion. Merit scholars in some STEM programs receive preferential dorm assignment, which could threaten identification if scholarship receipt were systematically correlated with ethnicity — such that scholarship holders and the peer groups they shape are ethnically non-representative. A plausible reason to worry exists: Uganda's elite secondary schools, which produce many national scholarship recipients, are historically concentrated in regions dominated by the country's largest ethnic groups.[11] Empirically, however, this correlation is negligible: ethnicity fixed effects explain less than 0.36% of the variation in scholarship status.

As a formal empirical check, we provide balance tests in Table 2, where we regress background characteristics on our peer variables while controlling for the same set of fixed effects as specification (4), while replacing course-by-year with major-by-year fixed effects, since baseline characteristics do not vary within student across courses.[12] Additionally, we complement these results by regressing the share of coethnic or scholarship peers on a full set of pre-university characteristics and report the estimates with the corresponding Joint F-stats in Appendix Table A.2.

Reassuringly, Table 2 shows the correlation between student characteristics and the share of coethnic peers (Panel A) and the share of scholarship peers (Panel B) is economically small and statistically insignificant across all specification but two. Specifically, Muslim students have a slightly lower coethnic peer share and students who attended secondary school in their home district have a slightly higher scholarship peer share. Importantly, these correlations do not affect our results — excluding these students in a robustness exercise leaves our main estimates unchanged. Table A.2 similarly yields small F-statistics of 1.00 for coethnic share and 2.03 for scholarship share, implying that pre-university characteristics have very little collective explanatory power for peer composition. Taken together, these results suggest that nonrandom sorting into peer groups is unlikely to be prevalent in our empirical setting.

[11]As Figure A.2 shows, the two largest ethnic groups are somewhat overrepresented at MUK, consistent with historical disparities in access to elite secondary schools.

[12]Results are robust to running the balance tests at the course level with standard errors clustered at the student level. The pattern of coefficients is virtually unchanged.

Table 2: Balance Tests: Peer Composition vs. Pre-University Characteristics

Panel A: Coethnic Share

	(1) Standardized HS Score	(2) Age	(3) Anglican	(4) Catholic	(5) Muslim	(6) Home dist. HS	(7) Scholarship
Coethnic share	0.029	−0.103	0.028	0.045	-0.086^{***}	0.064	0.052
	(0.07)	(0.16)	(0.06)	(0.05)	(0.03)	(0.06)	(0.05)
Observations	25,173	25,134	25,173	25,173	25,173	25,173	25,173
R-squared	0.63	0.15	0.08	0.07	0.29	0.13	0.29

Panel B: Scholarship Share

	(1) Standardized HS Score	(2) Age	(3) Anglican	(4) Catholic	(5) Muslim	(6) Home dist. HS
Scholarship share	0.062	0.039	−0.003	−0.029	−0.009	0.096^{***}
	(0.05)	(0.08)	(0.03)	(0.03)	(0.02)	(0.03)
Observations	25,173	25,134	25,173	25,173	25,173	25,173
R-squared	0.63	0.15	0.09	0.07	0.29	0.16

Notes: Data are from MUK, restricted to students in on-campus day majors, 2009–2017 (excluding 2010). Each column reports a separate regression of the listed pre-university characteristic on the treatment variable indicated in the panel heading. All regressions include major-year, ethnicity, HS subject combination, and dorm fixed effects, and control for peer group size. Scholarship status (column 7 of Panel A) is included as an outcome and not as a control. In Panel B, regressions additionally control for own scholarship status to remove the mechanical correlation between individual scholarship status and peer scholarship share. Standard errors are clustered at the peer-group level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

4.1.3 Dorm Assignment and Actual Peer Exposure

Our empirical analysis defines peer groups based on dorm assignment rather than actual residence. Due to dorm capacity constraints, not all students ultimately reside in their assigned dorms. Students who reside in their assigned dorm are likely exposed to peers through residential interactions in addition to shared classes, extracurricular activities, and common campus spaces. In contrast, students who live off campus may interact less intensively with their assigned peers, although they continue to encounter them regularly in academic, extra curricular, and other university settings.

Importantly, this setting differs from concerns raised in studies where administrative peer assignments serve as noisy proxies for underlying interaction groups. For example, Carrell et al. (2009) examine the consequences of measuring peer groups using dorm sections that only imperfectly overlap with the true interaction groups at the U.S. Air Force Academy. In our setting, dorm assignment identifies a well-defined peer group, but students within that group may differ in the intensity of their realized interactions. Consequently, our estimates should be interpreted as average effects of exposure to assigned peers whose interactions vary in intensity, rather than as effects of sharing a common residential environment.

Without observing actual residence or the frequency of interactions among assigned peers, it is not possible to separately identify the effects associated with more intensive residential interactions from those arising through shared academic experiences. Stronger assumptions would be required to recover these effects. Accordingly, the estimated coefficients ϕ_1 and ϕ_2 are best interpreted as reduced-form assignment-based effects that average across students experiencing different degrees of peer exposure.

4.2 Coethnic and Scholarship Interaction Effects

While the effect of exposure to scholarship peers (ϕ_2) is generally expected to be positive, the effect of coethnic peers (ϕ_1) is theoretically ambiguous because such an effect may operate through opposing channels. On the one hand, coethnic peers may increase students' sense of social familiarity, particularly in environments where minority students face higher social adjustment costs (Bayer et al., 2020). Some studies show that interventions to increase a sense of social familiarity improve academic outcomes for students (e.g., Walton and Cohen, 2011). Coethnic peers may thus improve academic outcomes by facilitating social adjustment and lowering the costs of forming study networks during the transition to university.

On the other hand, students with shared ethnicity may sort into friend and study groups for cultural reasons, including native language. If so, the consequence of this sorting depends on the scholarship composition of coethnic peers. If coethnic peers are less likely to hold scholarships, sorting into coethnic groups could reduce exposure to peers who signal high ability, producing a negative or null average coethnic effect. Distinguishing these two channels requires separating coethnic peers by scholarship status.

To capture the effect of the pre-university academic quality of coethnics, we augment equation (4) as follows.

$$y_{ijcG} = \beta_0 + \omega_1 S_{iG}^{ES} + \omega_2 S_{iG}^{E\cancel{S}} + \omega_3 S_{iG}^{\cancel{E}H} + \beta_2 \mathbf{X}_{iG} + \beta_3 \bar{\mathbf{X}}_G + \delta_j + \alpha_{ct} + \lambda_d + \theta_m + \gamma_s + \varepsilon_{ijcG}, \quad (5)$$

where S_{iG}^{ES}, $S_{iG}^{E\cancel{S}}$, and $S_{iG}^{\cancel{E}S}$ denote the decomposed shares of peers in three subgroups: scholarship and coethnic peers, non-scholarship and coethnic peers, and scholarship and non-coethnic peers, respectively. All other terms are defined as in equation (4). Whereas the baseline specification (4) captures the average effect of coethnic peers regardless of scholarship status (ϕ_1) and the average effect of scholarship peers regardless of ethnicity (ϕ_2), the decomposition in equation (5) allows these effects to differ by both dimensions relative to the excluded group (non-scholarship and non-coethnic peers).

If coethnic peer effects primarily reflect familiarity and lower adjustment costs, we would expect both ω_1 and ω_2 to be positive and of similar magnitude: ethnicity matters regardless of scholarship status. In contrast, if ethnic sorting into peer groups mainly crowds out academically valuable non-coethnic peers, ω_1 should be near zero or negative while ω_3 raises academic performance regardless of ethnicity.

4.3 Ability Composition and Signaling Effects

While scholarship peers may generate academic spillovers through substantive learning or study interactions, other channels for transmitting peer effects may depend on whether ability is publicly observable. In this setting, scholarship status is both salient and observable, whereas peers' underlying academic ability is not directly observed. This asymmetry creates a clean test: if peer effects operate through latent ability, only high-ability peers should matter regardless of label; however, if peer effects operate through observable signals, the scholarship label itself should drive effects even when the holder's academic preparation is not high.

To implement this test, we classify all students into four mutually exclusive groups based on their test-score rank relative to the school-level merit threshold and their scholarship status:

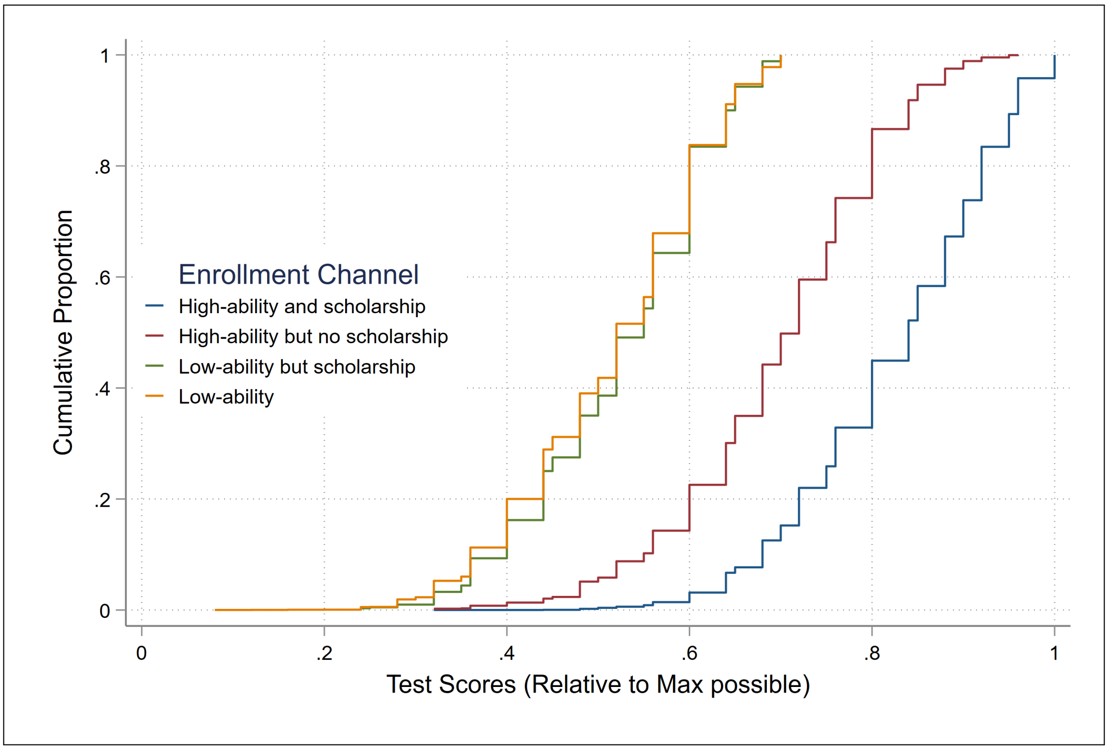
(A) **High-ability students with scholarship**: Students with high standardized test scores who qualify for and receive a merit-based scholarship.

(B) **High-ability students without scholarship**: Students with high test scores who do not receive a scholarship but would have received one had they applied differently or applied in a different year; these students enroll through private admission.

(C) **Low-ability students with scholarship**: Students with relatively low test scores who receive a scholarship through district-based residence and graduation.

(D) **Low-ability students without scholarship (omitted category)**: Students with low test scores who enrolled without a scholarship.

Figure 4: Test score distribution by Ability Type



Notes: This figure plots the cumulative distribution of national A-level test scores by ability type. Test scores are normalized by the maximum possible score to account for a change in the scoring scale in 2013, when the maximum decreased from 25 to 20. Ability types are defined using standardized test scores and scholarship status at entry.

Figure 4 validates this classification by plotting CDFs of the normalized HS national scores for each group. Groups (A) and (B) are clearly separated from (C) and (D), confirming that the ability cutoff meaningfully partitions students. Crucially, the (C) and (D) distributions overlap substantially—suggesting that district scholarships primarily reallocate access rather than select on academic merit—which is what makes the comparison informative: peer effects from group (C) relative to group (D) are unlikely to reflect latent ability differences *on the dimension of measured national scores (academic preparedness)*, and are more plausibly driven by the observable scholarship label.

We estimate the following specification, which parallels equation (5):

$$y_{ijcG} = \beta_0+\phi_1 S^{E}_{iG}+\psi_1 S^{HS}_{iG}+\psi_2 S^{H\not{S}}_{iG}+\psi_3 S^{\not{H}S}_{iG}+\beta_2 \mathbf{X}_{iG}+\beta_3 \bar{\mathbf{X}}_{G}+\delta_j+\alpha_{ct}+\lambda_d+\theta_m+\gamma_s+\varepsilon_{ijcG}, \quad (6)$$

where S^{HS}_{iG}, $S^{H\not{S}}_{iG}$, and $S^{\not{H}S}_{iG}$ denote the shares of high-ability scholarship peers, high-ability non-scholarship peers, and low-ability scholarship peers, respectively. The omitted category is low-ability non-scholarship peers. All other terms are defined as in equation (4). This decomposition yields four sources of variation in peer composition corresponding to ability and scholarship status. While the baseline specification captures the average effect of exposure to scholarship peers, equation (6) allows this effect to differ depending on whether the scholarship label reflects high or low underlying ability.

If peer effects operate through the scholarship signal rather than academic preparation (ability), we expect $\psi_1 > 0$ (group A: strong preparation, signaled), $\psi_2 \approx 0$ (group B: strong preparation, unsignaled), and $\psi_3 > 0$ (group C: weak preparation, signaled). Finding $\psi_2 \approx 0$ alongside $\psi_1, \psi_3 > 0$ would rule out pure academic preparation spillovers and identify the scholarship label as the operative channel.

4.4 Heterogeneous Peer Effects

Average peer effects may mask important heterogeneity in these effects if students differ in how they interact and respond to peers. We therefore examine heterogeneity along dimensions that are both theoretically motivated and salient in this setting. To do so, we estimate equation (4) augmented with interactions between peer composition and a student-level characteristic d_i:

$$y_{ijcG} = \beta_0+\phi_1 S^{E}_{iG}+\phi_2 S^{S}_{iG}+\varphi_1 S^{E}_{iG}\times d_i+\varphi_2 S^{S}_{iG}\times d_i+\beta_2 \mathbf{X}_{iG}+\beta_3 \bar{\mathbf{X}}_{G,-i}+\delta_j+\alpha_c+\lambda_d+\theta_m+\gamma_s+\varepsilon_{ijcG}, \quad (7)$$

where d_i captures a dimension along which peer effects may differ, and φ_1 and φ_2 represent differential effects of coethnic and scholarship peers, respectively. All other terms are defined as in equation (4).

Our main results focus on heterogeneity by a proxy or students' prior exposure to peers from different ethnic backgrounds.[13] The effects of coethnic peers may be particularly pronounced for students who arrive at MUK immediately after completing secondary school in relatively homogeneous home districts. For these students, enrollment at a centrally located national university often represents their first sustained interaction with peers from a broad range of linguistic and cultural backgrounds. Consistent with this interpretation, Okunogbe (2024) finds increased ethnic attachment among Nigerian youth randomly assigned to serve in regions dominated by other ethnic groups. More generally, students transitioning from relatively homogeneous environments may place greater value on interactions with peers from familiar backgrounds during

[13] We provide justification and heterogeneity analysis by gender in Appendix Section A.5.2.

the initial adjustment to university life.

5 Results

5.1 Mean Effects of Coethnic and Scholarship Peers

Table 3: Mean Effects Overall and By Year: Coethnic vs Scholarship Share

Year	(1) All	(2) One	(3) Two	(4) Three
Coethnic share	0.117**	0.131**	0.131**	0.089
	(0.05)	(0.06)	(0.06)	(0.06)
Scholarship share	0.115***	0.087***	0.120***	0.154***
	(0.03)	(0.03)	(0.03)	(0.03)
Scholarship (self)	0.352***	0.380***	0.348***	0.329***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	978,877	318,266	337,158	323,377
R-squared	0.36	0.33	0.38	0.38
Dorm FE	Yes	Yes	Yes	Yes
Major FE	Yes	Yes	Yes	Yes
Course-by-year FE	Yes	Yes	Yes	Yes
HS Subject FE	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Group Controls	Yes	Yes	Yes	Yes

Notes: Data are from MUK and restricted to students admitted to on-campus day majors between 2009 and 2017, excluding 2010. Peer groups are defined as students admitted to majors within the same school and assigned to the same dorm in each year. Each column reports an independent regression, with standardized course grades as the outcome. Individual controls include probable ethnic group, religious affiliation and whether the student graduated from the district of origin. Group-level controls include the leave-me-out averages of individual controls as well as log of peer group size. Standard errors, clustered at the peer-group level, are reported in parentheses.
*p<0.1, **p<0.05, ***p<0.01

We first estimate equation (4) to test how peer group composition – specifically, the share of coethnic peers and scholarship peers – affects students' academic outcomes. Table 3 reports these mean effects. All specifications control for the student's most probable ethnic group, own scholarship status, several fixed effects (dorm, classroom/course-by-year, major and high school subject–combination fixed effects), as well as additional individual- and group-level controls. Each column reports an independent regression. Column (1) pools outcomes across the three years students are on campus, while Columns (2)–(4) report year-specific estimates for the first, second, and third years, respectively. Because we observe assigned dorms but not actual residence status, all estimates should be interpreted as ITT effects that capture the impact of peer composition from random dorm assignments regardless of the dorm residence status of students or their peers.

Table 3 also shows that the effect of own scholarship is substantial: scholarship students perform approximately 0.4 standard deviations better than their non-scholarship peers — a sizable difference that corresponds to a shift in academic performance from the average of a second-class lower division (Fairly Good) to a second-class upper division (Good). As shown in Table 1, the average first-year grade in the sample falls within the second-class lower degree classification in this setting. This effect of own scholarship status on performance provides a benchmark against which peer effects can be compared.

We find strong evidence of peer effects for both measures. In Column (1), the coefficient on coethnic share is 0.117 and significant, implying that moving from a peer group with no coethnic peers to one composed entirely of coethnic peers (at the average group size) increases academic performance by 0.117 standard deviations. Put differently, a one-standard-deviation increase in coethnic share (SD = 0.11) raises performance by approximately 0.013 standard deviations. Because fewer than half of students actually reside in their assigned dorms, this estimate should be interpreted as the average effect of exposure to assigned peers whose interactions vary in intensity.

Similarly, we find that scholarship peers matter for academic performance. A corresponding full-range increase in the share of scholarship peers increases academic performance by 0.115 standard deviations in the pooled specification. In standardized terms, one–standard-deviation increase in scholarship peer share (SD = 0.22) increases performance by roughly 0.026 standard deviations.

Table 3 also examines whether these peer effects persist over time. Because peer groups are fixed at entry and course sequences are pre-determined within cohorts, changes in estimated effects across years can be interpreted as reflecting the duration of exposure to peers. The effect of coethnic share persists into the second year but declines in magnitude and becomes statistically insignificant by the third year, falling to roughly 68 percent of its first-year size. In contrast, the effect of scholarship share not only persists but strengthens over time. As shown, the impact of scholarship peers increases from .09 standard deviations in the first year to 0.15 standard deviations in the third year, an increase of approximately 77 percent higher, suggesting rising academic benefits to prolonged dosage of scholarship peers.

5.2 Decomposing Peer Composition by Scholarship Status and Ethnicity

Next, we test whether the average coethnic peer effect documented in Table 3 is driven by exposure to scholarship peers. To do so, we estimate equation (5), which decomposes peer composition into the shares of scholarship coethnic peers, non-scholarship coethnic peers, and scholarship non-coethnic peers. This decomposition allows us to distinguish scholarship-driven peer effects from coethnic exposure that operates independently of academic quality.

Table 4 reports the corresponding mean effects from this decomposition. In the pooled specification, all three peer types have positive and statistically significant effects on academic performance. The largest coefficient is associated with scholarship coethnic peers: moving from a peer group with no such peers to one composed entirely of scholarship coethnic peers increases grades by 0.20 standard deviations. This compares to 0.14 standard deviations for non-scholarship coethnic peers and 0.13 standard deviations for scholarship non-coethnic peers. When we account differences in the distribution, a one-standard-deviation increase scholarship coethnic share, non-scholarship coethnic share, and scholarship non-coethnic share increases

Table 4: Mean Effects of Scholarship Coethnic and Scholarship Non-coethnic Peers.

Year	All	One	Two	Three
Scholarship coethnic share	0.196**	0.149*	0.218**	0.235***
	(0.08)	(0.09)	(0.09)	(0.09)
Non-scholarship coethnic share	0.140**	0.174***	0.151**	0.094
	(0.06)	(0.06)	(0.07)	(0.06)
Scholarship non-coethnic share	0.128***	0.111***	0.131***	0.157***
	(0.03)	(0.04)	(0.04)	(0.04)
Scholarship (self)	0.352***	0.380***	0.348***	0.329***
	(0.01)	(0.01)	(0.01)	(0.01)
R-squared	0.36	0.33	0.38	0.38
N	978,877	318,266	337,158	323,377
Dorm FE	Yes	Yes	Yes	Yes
Major FE	Yes	Yes	Yes	Yes
Course-by-year FE	Yes	Yes	Yes	Yes
HS Subject FE	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Group Controls	Yes	Yes	Yes	Yes

Notes: Data are from MUK and restricted to students admitted to on-campus day majors between 2009 and 2017, excluding 2010. Peer groups are defined as students admitted to majors within the same school and assigned to the same dorm in each year. Each column reports an independent regression, with standardized course grades as the outcome. Individual controls include probable ethnic group, religious affiliation and whether the student graduated from the district of origin. Group-level controls include the leave-me-out averages of individual controls as well as peer group size. Standard errors, clustered at the peer-group level, are reported in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

academic performance by 0.012, 0.013, and 0.024 SDs, respectively in the pooled regression.

Columns (2)–(4) extend the analysis to subsequent years to examine persistence. The effect of scholarship coethnic peers is present in Year One and increases with time. In contrast, the effect of non-scholarship coethnic peers declines in magnitude and becomes imprecise by the third year. The coefficient of scholarship non-coethnic peers not only persists but strengthens over time, rising from 0.11 standard deviations in the first year to 0.16 standard deviations in the third year. These results suggest that both scholarship and non-scholarship coethnic peers improve academic performance early in students' university careers, but that only scholarship peers generate effects that persist and strengthen over time.

5.3 Peer Composition and Public Signals of Ability

5.3.1 Decomposing Scholarship Peer Effects

Table 5 examines whether peer effects are more closely associated with peers' underlying academic preparation or with the publicly observable scholarship label. Three empirical patterns emerge. First, exposure to peers who are both high-ability and scholarship recipients has a positive and statistically significant effect on academic performance. In the pooled specification,

Table 5: Peer effect transmission decomposition: ability vs signaling

Year	All	One	Two	Three
Coethnic share	0.129**	0.142**	0.141**	0.101*
	(0.05)	(0.06)	(0.06)	(0.06)
High-ability w scholarship share	0.092**	0.074*	0.085**	0.130***
	(0.04)	(0.04)	(0.04)	(0.04)
High-ability w/o scholarship share	-0.023	-0.014	-0.045	-0.013
	(0.03)	(0.04)	(0.04)	(0.04)
Low-ability w scholarship share	0.219**	0.164	0.135	0.384***
	(0.09)	(0.11)	(0.10)	(0.11)
High-ability w scholarship (self)	0.512***	0.558***	0.507***	0.474***
	(0.01)	(0.01)	(0.01)	(0.01)
High-ability w/o scholarship (self)	0.179***	0.196***	0.179***	0.163***
	(0.01)	(0.01)	(0.01)	(0.01)
Low-ability w scholarship (self)	0.168***	0.156***	0.165***	0.182***
	(0.02)	(0.02)	(0.02)	(0.02)
R-squared	0.368	0.335	0.386	0.383
N	978,877	318,266	337,158	323,377
Dorm FE	Yes	Yes	Yes	Yes
Major FE	Yes	Yes	Yes	Yes
Course-by-year FE	Yes	Yes	Yes	Yes
HS Subject FE	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Group Controls	Yes	Yes	Yes	Yes

Notes: Data are from MUK and restricted to students admitted to on-campus day majors between 2009 and 2017, excluding 2010. Peer groups are defined as students admitted to majors within the same school and assigned to the same dorm in each year. Each column reports an independent regression, with standardized course grades as the outcome. Individual controls include probable ethnic group, religious affiliation and whether the student graduated from the district of origin. Group-level controls include the leave-me-out averages of individual controls as well as peer group size. Standard errors, clustered at the peer-group level, are reported in parentheses.
*p<0.1, **p<0.05, ***p<0.01

moving from no exposure to full exposure to such peers increases academic performance by approximately 0.10 standard deviations, with effects that remain stable or increase over time. Second, exposure to high-ability peers who do not hold scholarships has little effect despite these students being academically stronger than the omitted group (as evident by their own academic outcomes and the test score distributions shown in Figure 4). Third, exposure to scholarship recipients drawn from the lower end of the admitted ability distribution also improves academic performance. Taken together, these patterns suggest that students respond more strongly to publicly observable scholarship status than to peers' underlying academic preparation.

Particularly notable is the effect of exposure to scholarship recipients with relatively low underlying test scores. In the pooled sample, moving from no exposure to full exposure to such peers increases academic performance by approximately 0.22 standard deviations, with effects that remain sizable throughout students' university careers. These students have substantially lower test score distributions than high-ability peers, as shown in Figure 4, making it difficult to attribute these estimates solely to peers' underlying academic preparation.

Results in Table 5 also confirm that own academic preparation behaves as expected. Students who are both high-ability and scholarship recipients perform best on average, while high-ability students without scholarships and low-ability scholarship recipients also outperform the omitted category by similar magnitudes. Notably, low-ability scholarship recipients significantly outperform the omitted group despite having comparable test score distributions. These own-ability coefficients are broadly stable across years and further underscore the prominence of scholarship status as a visible marker of academic standing in this environment.

Taken together, this pattern of results ($\hat{\psi}_1 > 0$, $\hat{\psi}_2 \approx 0$, and $\hat{\psi}_3 > 0$ in the notation of equation (6)) is difficult to reconcile with peer influence operating solely through peers' underlying academic preparation. Instead, these estimates suggest that students respond more strongly to the publicly observable scholarship label than to peers' underlying academic preparation.[14]

5.3.2 Additional Evidence: Latent Ability versus Public Signals

Table 5 suggests that students respond more strongly to publicly observable scholarship status than to peers' underlying academic preparation. However, scholarship status may still capture dimensions of academic preparation beyond those incorporated in the signaling decomposition. To examine this possibility, we replace scholarship peer shares with peer averages of standardized national examination scores, which measure underlying academic preparation but are not publicly observable to peers.

Appendix Table A.3 reports these estimates. Across all specifications, students' own examination scores strongly predict academic performance, consistent with the fact that these measures capture meaningful differences in academic preparation. In contrast, the corresponding leave-one-out peer averages are economically small and statistically indistinguishable from zero.

Importantly, the estimated coethnic peer effects remain remarkably stable in both magnitude and significance across specifications. Taken together, these findings reinforce the interpretation that students appear to respond more strongly to publicly observable indicators of academic standing than to peers' underlying academic preparation. They also suggest that the estimated coethnic peer effects are unlikely to be driven by differences in peer groups' underlying academic preparation.

Table 6: Difference Effects by Assumed Ethnic Salience

Year	All	One	Two	Three
Coethnic share	0.072	0.047	0.083	0.086
	(0.06)	(0.06)	(0.06)	(0.06)
Scholarship share	0.108***	0.087***	0.108***	0.143***
	(0.03)	(0.03)	(0.03)	(0.03)
Scholarship (self)	0.357***	0.386***	0.353***	0.333***
	(0.01)	(0.01)	(0.01)	(0.01)
Coethnic share × Home district HS	0.145**	0.250***	0.153**	0.034
	(0.06)	(0.07)	(0.07)	(0.07)
Scholarship share × Home district HS	0.026	0.008	0.039	0.034
	(0.03)	(0.04)	(0.04)	(0.04)
Home district HS (self)	-0.029*	-0.049***	-0.034*	-0.004
	(0.02)	(0.02)	(0.02)	(0.02)
Pval Coethnic share: $(\phi_1 + \varphi_1)$	0.001	0.000	0.001	0.091
Pval Scholarship share : $(\phi_2 + \varphi_2)$	0.000	0.013	0.000	0.000
R-squared	0.362	0.329	0.381	0.378
N	980,418	318,778	337,690	323,870
Dorm FE	Yes	Yes	Yes	Yes
Major FE	Yes	Yes	Yes	Yes
Course-by-year FE	Yes	Yes	Yes	Yes
HS Subject FE	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Group Controls	Yes	Yes	Yes	Yes

Notes: Data are from MUK and restricted to students admitted to on-campus day majors between 2009 and 2017, excluding 2010. Peer groups are defined as students admitted to majors within the same school and assigned to the same dorm in each year. Each column reports an independent regression, with standardized course grades as the outcome. Individual controls include probable ethnic group, religious affiliation and whether the student graduated from the district of origin. Group-level controls include the leave-me-out averages of individual controls as well as peer group size. Standard errors, clustered at the peer-group level, are reported in parentheses.
*p<0.1, **p<0.05, ***p<0.01

5.4 Assumed Ethnic Salience and the Dynamics of Coethnic Peer Effects

5.4.1 Heterogeneity by Pre-University Ethnic Exposure

We now estimate differential impacts by assumed ethnic salience, proxied by home-district schooling, as discussed in Section 4.4, using specification (7) and report the results in Table 6. Column (1) presents pooled estimates across years, while Columns (2)–(4) report year-specific effects. The table also reports p-values for tests of the joint significance of peer effects for

[14]The estimated coefficients from the signaling decomposition are virtually unchanged when controlling for students' own and peers' underlying academic preparation, measured using standardized national examination scores, indicating that the pattern $\hat{\psi}_1 > 0$, $\hat{\psi}_2 \approx 0$, and $\hat{\psi}_3 > 0$ is not driven by differences in peer groups' underlying academic preparation.

high-salience students: coethnic share ($\phi_1 + \varphi_1$) and scholarship share ($\phi_2 + \varphi_2$).

The pooled results indicate that students with high assumed ethnic salience have lower academic performance. The coefficient on the home district HS is negative and statistically significant, implying that, absent coethnic peer effects, these students perform approximately 0.03 standard deviations worse on average across all their undergrad tenure. This gap is largest in the first year (0.05) and gradually shrinks over time, becoming small and statistically insignificant by the third year. Thus, students of assumed high ethnic salience start off academically behind but catch up by graduation.

Conditional on this pattern, coethnic peer effects play an important role for these types of students, although the importance depends on the time of undergrad tenure. In the pooled specification, the interaction between coethnic share and home district HS is positive and statistically significant. In Year One, moving from a peer group with no coethnic peers to one composed entirely of coethnic peers increases grades by approximately 0.30 standard deviations for students with limited prior exposure to linguistic and cultural heterogeneity, while the same change has no statistically or meaningfully significant effect for students of low ethnic salience. The implied peer effect for high ethnic salience students remains positive and statistically significant in the second year but attenuates substantially by the third year.

By contrast, exposure to scholarship peers benefits all students regardless of assumed ethnic salience. The coefficient on scholarship share is positive, precisely estimated, and increasing over time, while the interaction with ethnic salience is small and statistically insignificant in all specifications.

Together, these results point to a dynamic pattern: students with high ethnic salience experience lower academic performance, benefit disproportionately from coethnic peer exposure early on, but converge toward their peers by later years, at which point coethnic exposure no longer plays a meaningful role. This temporal structure is consistent with coethnic peers facilitating early adjustment to university life rather than generating persistent academic advantages.

5.4.2 Corroborating Evidence from Alternative Salience Proxies

Appendix Table A.4 corroborates our results in this section by replacing the binary home-district proxy with two continuous measures of pre-university ethnic exposure: the share of a student's predicted ethnic group in their HS district (Column (2)) and in their home district (Column (2)), both demeaned at the district level for ease of interpretation. The difference between the HS district and home district is informative as it separates the schooling environment from growing up among coethnics. If the schooling environment is what drives the assumed ethnic salience effects on coethnic peer effects, coethnic peer effects should respond to the ethnic composition of the HS district but not the home district.

The results support this interpretation. Coethnic social interactions increase sharply with ethnic homogeneity in the HS district, indicating a strong dose–response relationship with pre-university schooling exposure. By contrast, the analogous interaction using the home district is small and statistically insignificant. Merit-based social interactions remain large and stable across all three specifications and do not interact with either salience measure, further supporting the interpretation that the peer effect heterogeneity likely operates through language and cultural familiarity-based channels.

5.5 Robustness checks

5.5.1 Robustness to Alternative Fixed Effects and Controls

Table A.7 shows that the mean peer effects are not sensitive to alternative fixed effects and control sets. Starting from a parsimonious specification, we sequentially add dorm fixed effects, individual controls, and group-level controls. The estimated coefficients on coethnic share and scholarship share remain very similar across columns, with trivial changes as controls are added. This stability is consistent with the assumption of exogenous peer group assignment and suggests that the main results are unlikely to be driven by dorm-level confounds, sorting on observables, or correlated group-level shocks.

5.5.2 Coethnic Share Based on Discrete Ethnicity Classification

We re-estimate the results using a binary indicator for ethnicity, defined by the top predicted ethnic group, instead of the full vector of predicted probabilities. These results are reported in Appendix Table A.6. This alternative construction of coethnic share may introduce misclassification, particularly for names that plausibly span multiple ethnic groups, an issue discussed in Section 3.2.3. As shown in the table, the estimated coethnic peer effects remain positive and statistically significant. While the point estimates are smaller in magnitude, this attenuation is expected given the narrower variation in the discrete coethnic share measure — the effects are comparable in standardized terms. In contrast, the estimated high-ability peer effects, which are not subject to misclassification, remain stable and closely aligned with the estimates reported in the main analysis

5.5.3 Randomization Inference to Validate Peer Effects

To assess whether the estimated coethnic and scholarship peer effects are due to mechanical correlations, we conduct a randomization inference exercise.[15] Specifically, within each cohort, school, and gender cell, we randomly reassign students to dormitories, thereby preserving the original assignment structure while eliminating the actual peer groups. For each simulated assignment, we re-estimate the baseline specification and record the estimated coefficients on coethnic and scholarship peer shares. Repeating this procedure 2,000 times yields the empirical distribution of peer effects under the null hypothesis that peer composition has no effect on academic outcomes.

The results of this exercise are illustrated in Appendix Figure A.5. The left panel displays the kernel distribution of simulated coethnic peer effects, while the right panel shows the corresponding distribution for scholarship peer effects. The figure also plots the mean effects reported in Section 5.1. As shown, both observed effects lie in the far right tail of the simulated distributions. As shown, the coethnic effect lies in the far right tail of its simulated distribution (p=0.011), unlikely to arise from random assignment alone. The scholarship effect is also above

[15]We do not use Stata's built-in ritest ado package because our test involves reshuffling group assignments and reconstructing peer quality variables)

its simulated median (p=0.063) — suggestive, though short of 5% significance, and best read as corroborating rather than standalone evidence for the causal interpretation in Section 5.1.

5.6 Discussion: Contextualizing Results

The coethnic effects we estimate are modest but broadly comparable to prior estimates in the peer effects literature. A one-standard-deviation increase in coethnic share raises academic performance by 0.013 standard deviations in our pooled estimate (Table 3). For comparison, Zimmerman (2003) finds that a one-standard-deviation increase in roommate verbal SAT scores at Williams College raises GPA by roughly 0.022 standard deviations; Carrell et al. (2009) report an effect of approximately 0.083 standard deviations[16] per one-standard-deviation change in verbal SATs among USAFA squadrons; and Garlick (2018) finds a 0.04 standard deviation effect per one-standard-deviation increase in mean dormitory high-school GPA at the University of Cape Town, although with a relatively wide confidence interval. These comparisons likely understate the effects for students with limited prior exposure to linguistic and cultural heterogeneity, who largely drive the estimated coethnic effects. For first-year students from relatively homogeneous schooling environments, a one-standard-deviation increase in coethnic share raises GPA by 0.032 standard deviations.

Scholarship peer effects are similar in magnitude to prior estimates. A one-standard-deviation increase in scholarship share raises GPA by 0.026 standard deviations in the pooled sample, approximately 65 percent of the point estimate reported in Garlick (2018) and roughly one-third of the effects reported in Carrell et al. (2009). Effects grow with continued exposure. By Year 3, a one-standard-deviation increase in scholarship share raises GPA by 0.035 standard deviations. Because dorm assignment rather than actual residence defines peer groups in our setting, these estimates should be interpreted as assignment-based effects that average across students experiencing different intensities of interaction with their assigned peers.

Beyond average effects, our estimates exhibit clear temporal patterns. Coethnic effects are strongest during students' first year at university and attenuate thereafter. This decline is driven primarily by the diminishing influence of shared linguistic and cultural backgrounds in the absence of an accompanying signal of academic standing. In contrast, scholarship peer effects increase with continued exposure. Both scholarship coethnic and scholarship non-coethnic peer effects grow over time. These patterns suggest that familiarity with peers from similar linguistic and cultural backgrounds is most important during the transition to university, while observable indicators of academic standing become increasingly important as students progress through their degree programs. Our findings differ somewhat from Garlick (2018), who finds that social proximity largely mediates ability peer effects. In our setting, observable indicators of academic standing increasingly shape peer influence irrespective of cultural group.

A central finding of the paper is that coethnic effects vary systematically with students' prior exposure to ethnically and linguistically diverse environments. Students from relatively homogeneous schooling environments perform worse on average during their first year but ex-

[16] The Carrell et al. (2009) estimates are among the largest in the literature, likely because USAFA is an unusual setting: students cannot opt out of their assigned squadrons, interact with them constantly through mandatory shared activities, and have few outside social alternatives. Most universities, including MUK, differ substantially from this environment.

perience significantly larger gains from greater exposure to coethnic peers. This interaction is strongest early in students' academic careers and declines over time, mirroring the attenuation of the average coethnic effect.

These patterns suggest that coethnic peers may ease the transition from relatively homogeneous schooling environments into a nationally representative university setting. This interpretation is reinforced when we replace the home-district indicator with a continuous measure of the share of the student's own ethnic group in the high-school district, regardless of whether the school is located in the student's home district. The persistence of this interaction suggests that the effect is driven primarily by prior exposure to relatively homogeneous schooling environments rather than by geographic familiarity or simply living home.

Our analysis of scholarship peers suggests that students respond more strongly to publicly observable indicators of academic standing than to peers' underlying academic preparation. Both students' own scholarship status and exposure to scholarship peers predict academic performance, whereas peer averages of standardized national examination scores do not. This distinction is informative because scholarship status is publicly observable while examination scores are not.

Scholarship recipients continue to generate positive peer effects even when their measured examination scores are relatively low, whereas high-ability students without scholarships do not exert detectable peer effects. Taken together, these findings suggest that scholarship status serves as a salient institutional marker of academic standing that shapes academic outcomes throughout university. They differ from prior evidence emphasizing peer influences operating through study behavior and effort (e.g., Stinebrickner and Stinebrickner, 2006; Mehta et al., 2019; Frijters et al., 2019). In our setting, we find little evidence that peer examination scores alone influence academic outcomes, while publicly observable scholarship status appears to play a more important role.

These findings point to several limitations worth acknowledging. Because we observe dorm assignment rather than actual residence, our estimates capture assignment-based exposure to peers whose interactions vary in intensity. In addition, the mechanisms underlying the estimated effects are inferred from observed patterns rather than directly measured. Finally, our outcomes are limited to academic performance, and whether these effects persist beyond university remains an open question.

6 Conclusion

Universities bring together students from diverse backgrounds and create many opportunities for young adults to develop social and academic connections with each other. Consequently, they also present a unique opportunity to study how peers influence each other. Estimating peer effects and disentangling the channels through which peers influence academic outcomes is difficult. This paper exploits random dorm assignment at a large elite and ethnically diverse university in Africa to construct relevant and exogenous peer groups with which to separately identify the causal effects of coethnic peers and scholarship peers on academic performance. We further leverage features of this institutional setting to distinguish peer effects operating through visible ability signals from unobserved academic ability.

Our analysis reveals two main effects. First, coethnic peers (irrespective of ability and ability signals) improve academic performance early in university, but the effect fades by the third year. The effect is concentrated among students who arrive with limited prior exposure to other ethnic groups. Coethnic peers appear to ease the transition of these students into a diverse and demanding academic environment. As students progress during their undergraduate degree, it is likely they form cross-ethnic ties become more important, the ethnic basis of peer interaction weakens.

Second, and more strikingly, the effects of peers with scholarships operate through publicly observable signals rather than unobservable academic ability. The effect of scholarship share within a student's peer group increases performance, and this effect grows over time. However, when we separate scholarship peers by underlying ability, only the visible scholarship label matters. High-ability peers without scholarship status generate no detectable peer effects, while lower-ability students who carry the scholarship label generate positive peer effects. Taken together, these estimates suggest that students respond more strongly to scholarship status than to peers' underlying academic preparation. While underlying academic preparation predicts students' own performance, it does not appear to generate peer effects in the absence of a visible institutional signal.

These findings are difficult to reconcile with peer influence operating solely through peers' underlying academic preparation. Instead, they suggest that publicly observable indicators of academic standing may play an important role in shaping academic outcomes. More broadly, our results indicate that peer influences may depend not only on peers' underlying characteristics, but also on which characteristics are publicly observable within an institution.

More broadly, our findings suggest that universities shape peer influences not only through how students are grouped together, but also through which academic distinctions are made publicly visible. Understanding how information and familiarity interact to influence student outcomes remains an important direction for future research.

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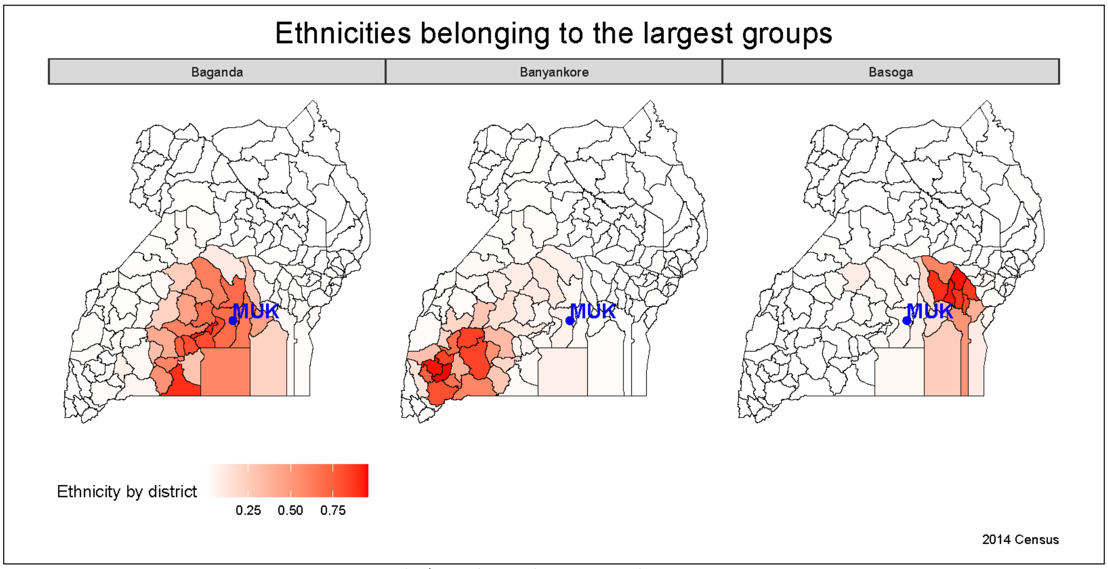
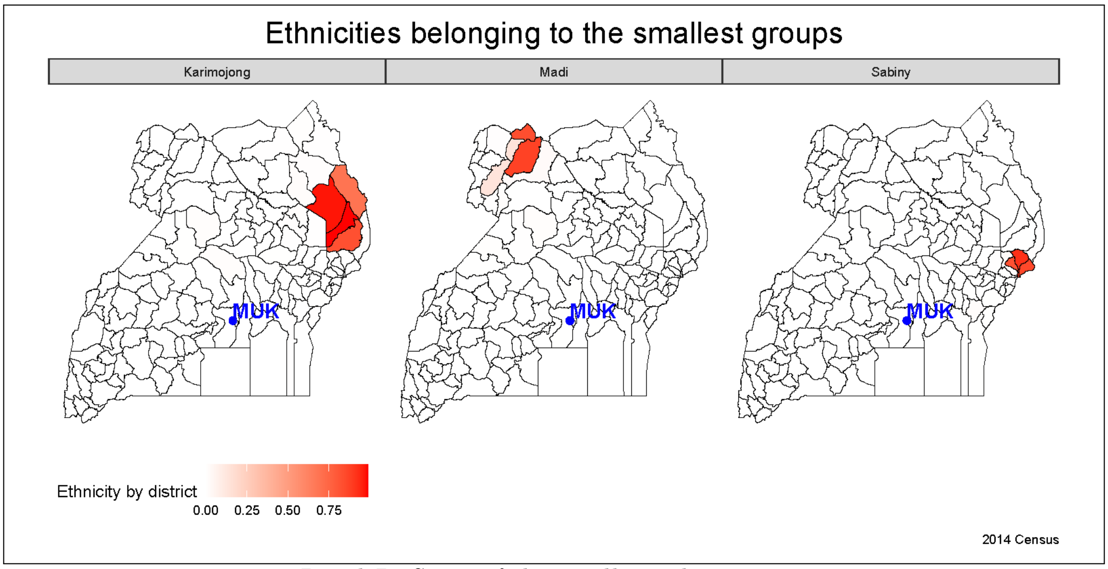
Figure A.1: Geographic Segregation



Panel A: Three largest ethnic groups



Panel B: Some of the smallest ethnic groups

Notes: Ethnicity by district is the proportion of each ethnicity within a district. Data source: 2014 census. District shapefiles can be downloaded from the UNHCR Data Portal.

A Appendix

A.1 Mapping Contemporary Administrative Units to Ethnic Regions

To construct geographic ethnic borders for training the ethnicity classification algorithm, we map Uganda's administrative districts to historical ethnic regions. Because our confidential training data are organized at the level of contemporary administrative units, we complement pre-colonial and colonial-era boundaries with population dominance observed in the 2014 census data.

At independence, Uganda consisted of 15 administrative units—federal states, districts, and territories—engraved at the Parliament gate and codified in the 1964 Constitution. These included the kingdoms of Ankore, Buganda, Bunyoro, and Toro; the territory of Busoga; and districts such as Acholi, Bugisu, Bukedi, Karamoja, Kigezi, Lango, Madi, Sebei, Teso, and West Nile. These units broadly followed pre-colonial ethnic settlement patterns, though British colonial administration sometimes bundled together distinct ethnic groups in eastern Uganda and West Nile that lacked highly centralized political structures prior to colonial rule.

In 1966, Uganda's first president abolished kingdoms and converted federal states into districts, splitting Buganda into four districts for political reasons (Morris, 1966). Since then, the number of districts has expanded to 135, largely through subdivision of existing districts. New districts are almost always carved out of a single parent district rather than formed by combining areas from multiple districts. This pattern of district proliferation implies strong path dependence in ethnic composition. Prior work shows that newly created districts tend to be more ethnically homogeneous, often reflecting efforts by smaller ethnic groups to gain administrative autonomy or bring public resources closer to their communities, although district creation has also been used strategically for political support (Green, 2008; Ssentongo, 2016).

Because colonial-era district boundaries sometimes grouped unrelated ethnicities, we do not mechanically assign current districts to ethnic regions based solely on historical administrative maps. Instead, when necessary, ethnic population dominance supersedes colonial geography in defining ethnic borders.

A.1.1 Census-Based Assignment Rule

We combine publicly available administrative boundary data from the Ministry of Local Government (MoLG) with 2014 national census data from the Uganda Bureau of Statistics (UBOS). We combine publicly available administrative boundary data from the Ministry of Local Government (MoLG) with 2014 national census data from the Uganda Bureau of Statistics (UBOS). We use 2010 administrative district boundaries to ensure consistency across the study period (2009–2017), as district boundaries were stable during this time. Post-2010 district subdivisions are mapped back to their 2010 parent districts. The census reports population counts for 65 ethnic groups in each district, yielding a district–ethnicity panel ($\approx$ 112 districts $\times$ 65 ethnicities). For each district j, we compute the population share of each ethnicity e,

$$\pi_{je} = \frac{\text{Population of ethnicity } e \text{ in district } j}{\text{Total population of district } j},$$

and rank these shares from highest to lowest. The ethnicity with the largest population share determines the ethnic region to which the district is assigned. This rule aligns closely with historical settlement patterns while allowing for post-independence administrative changes.

Although UBOS reports more than 60 ethnic groups nationally, ethnic population shares are highly skewed. Using the 2014 census, 45 of the 66 recorded ethnic groups each account for less than 1% of Uganda's population individually. Among these, the 22 smallest groups together account for less than 1% combined, a negligible share of the national population. These smallest groups consist primarily of non-Ugandan immigrant populations or very small indigenous communities. Immigrant groups tend to be spatially dispersed or concentrated in refugee settlement areas,[17] while indigenous groups, though sometimes geographically concentrated, constitute only a small share of district populations.

Following Alesina et al. (2003), we therefore do not allow very small or residual ethnic groups to define geographic ethnic regions. Accordingly, this approach abstracts from small minority groups within districts. For example, Abim district has a Karimojong population share of 87% and is classified as part of the Karimojong ethnic region despite the presence of small minority groups such as the Gimara (0.03%).

The high ethnic homogeneity of Ugandan districts also reflects the political history of district creation. For example, Nebbi district is 96.2% Alur, having been carved out of the former West Nile district in 1974 — which was historically dominated by the Lugbara — to give the Alur greater administrative autonomy. This pattern, whereby some ethnic groups lobby for separate districts, has contributed to high levels of ethnic homogeneity across contemporary administrative units.

To confirm that the "plurality" rule produces unambiguous classifications in this setting, we compute the ethnic fractionalization index (EFI) for each district according to Hudson and Taylor (1972): $\text{EFI}j = \sum e\pi_{je}(1 - \pi_{je})$. The average share of the dominant ethnicity across districts is 0.727 (median = 0.802), and the median EFI is 0.345, indicating that it is rare to observe districts with evenly balanced ethnic compositions. In contrast, computing the EFI at the national level yields 0.933, consistent with Alesina et al. (2003): while Uganda is highly diverse nationally, its subnational administrative units are not. This pattern is visually evident in Figure A.1.

A.1.2 Ethnicity Grouping and Implications for Training Data

Students do not report ethnicity or ancestral origin during university applications but do report home districts. While districts help anchor geographic ethnic regions, they are insufficient to identify individual ethnicity in the presence of internal migration, particularly rural–urban migration. We therefore predict individual ethnicity using students' surnames, exploiting the strong correspondence between Ugandan surnames and native languages (Section 3.2).

To ensure consistency between geographic and individual measures of ethnicity, we aggregate closely related ethnicities into language groups based on linguistic similarity and geographic proximity. Using language groups as proxies for ethnicity is common in studies of identity in Africa, given the strong overlap between language and ethnic affiliation (Eifert et al., 2010; Depetris-Chauvin and Durante, 2017). Our ethnicity clusters are thus based on two criteria: (i) high mutual intelligibility, i.e., languages that are dialects of the same root language and are spoken in the same geographic area and (ii) shared historical lineage. For example, residents of the historical Ankole and Kigezi regions speak mutually intelligible dialects and are grouped as SW (Banyankore/Kiga), while the Batoro and Banyoro are grouped together due to shared historical origins prior to the nineteenth century (Turyahikayo, 1976) and also shared language. Applying these criteria reduces the number of ethnic groups used in the main analysis to 17, as reported in Table A.1. This aggregation also improves the performance of the ethnicity

[17] Uganda is one of the world's largest refugee-hosting countries; see UNHCR statistics at https://www.unhcr.org/refugee-statistics/.

Table A.1: Ethnicity/Language Group Composition

Ethnicity/language group	Composition	Number
Alur–Jonam	Alur, Jonam	2
SW (Ankole/Kiga)	Banyankore, Bakiga, Bafumbira, Bahehe, Bahororo, Banyaruguru, Banyarwanda, Batagwenda, Barundi	9
Ganda	Baganda	1
Gisu	Bagisu, Babukusu, Shana	3
Iteso	Iteso	1
Jopadhola	Jopadhola	1
Kakwa	Kakwa	1
Kalenjin	Pokot, Sabiny	2
Karimojong	Karimojong, Jie, Dodoth, Napore, Nyangia, Kebuokebu	6
Madi	Madi	1
Northern Luo	Acholi, Langi, Kumam, Ethur	4
Nyoro	Banyoro, Batoro, Batuku, Bagungu	4
Rwenzori	Bakonzo, Baamba, Babwisi, Basongora	4
Samia–Nyole–Gwe	Basamia, Banyole, Bagwe	3
Soga	Basoga, Bagwere, Bakenyi	3
West Nile	Lugbara, Aringa	2
Nyara–Ruli	Banyara, Baruli	2
Extremely small	Vonoma, SoTepeth, Reli, Chope, Nubi, Ngikutio, Mvuba, Mening, Lendu, Kuku, Gimara, IkTeuso, Batwa, Banyabutumbi, Banyabindi, Aliba	16
All		65

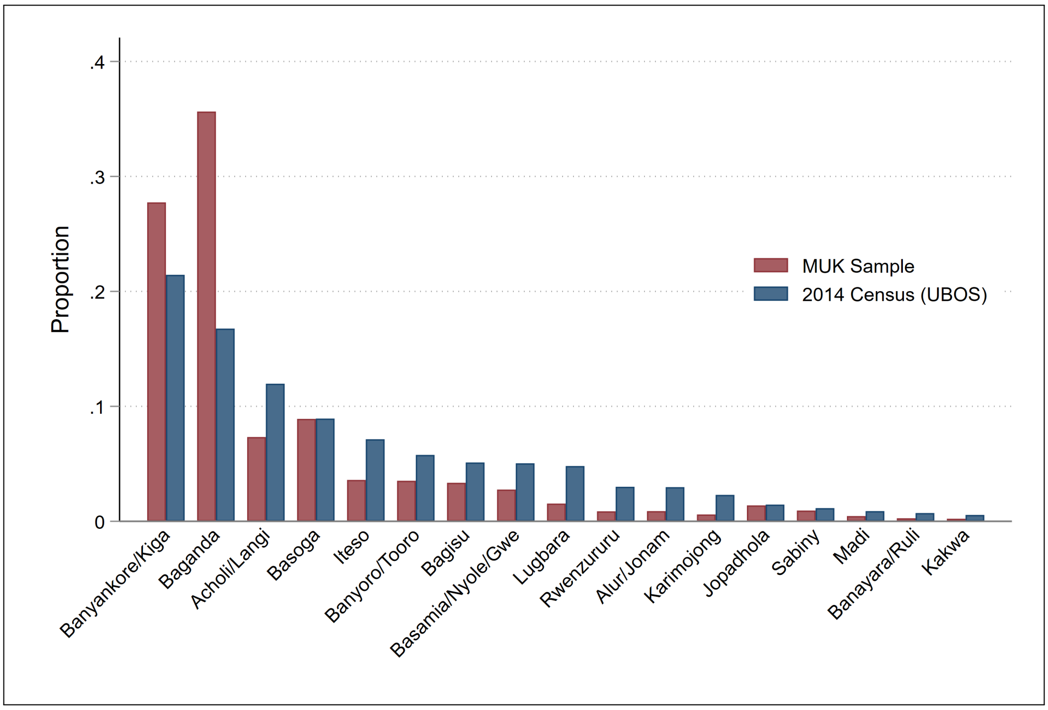
Notes: Source is the Uganda Population and Housing Census (2014). Ethnicity/language groups were constructed by aggregating closely related ethnicities based on linguistic and regional proximity. Extremely small groups jointly account for approximately 2.93% of the population. Reported percentages for this category are calculated excluding foreign nationalities (non-Ugandans), which are shown separately.

classification algorithm by reducing noise arising from fine linguistic distinctions and overlap in surname features.

To further mitigate the influence of internal migration on training labels, we exclude voter registration records from Kampala and Wakiso districts, as they are part of the Greater Metropolitan Kampala Area and attract substantial immigration. We similarly exclude records from districts hosting large refugee settlements, where ethnic composition is strongly shaped by recent forced immigration. In both cases, the registered district of a voter is an unreliable indicator of ethnic origin, weakening the surname-to-ethnicity correspondence used to construct training labels.

As a validation check, Figure A.2 compares national population shares by ethnic group from the 2014 census with the distribution of predicted ethnicity among MUK students. Census proportions are constructed using the same ethnicity groupings in Table A.1.

Figure A.2: Distribution of coethnicity: MUK Sample vs the Census.



Notes:The population proportion is computed from 2014 census data from UBOS. These data provide a breakdown by ethnicity in each district as of 2014. We follow the same ethnicity grouping in Section A.1 to compute the proportion of each ethnic group in both our sample and Census.

The MUK student population broadly reflects national ethnic patterns, though the largest ethnic groups (Baganda and Banyankore/SW) are moderately overrepresented, consistent with historical differences in access to elite secondary schools and higher education. Smaller ethnic groups are correspondingly underrepresented. These differences motivate controlling for a student's most probable ethnic group throughout the analysis

A.2 Derivation of Identification and Additional Supporting Evidence

A.2.1 Derivation of the Identifying Assumption

Using the same notation as Figure 1, let D denote dorm assignment, F denote school/faculty, C denote entry cohort, W denote gender, and Y denote the main outcome of interest (exam scores). We aim to estimate the causal effects of peer characteristics on Y, which we model as

$$Y = f(S^E, S^S, \mathbf{X}, A, U), \tag{A.1}$$

where S^E is the coethnic share in the peer group, S^S is the scholarship share in the peer group, $\mathbf{X}$ denotes observed individual covariates *excluding* the individual ability signal A (merit scholarship status), and U captures unobserved individual characteristics.

Dorm Assignment Assumption (Conditional Randomness). Conditional on school F, entry cohort C, and gender W, dorm assignment is independent of individual traits:

$$D \perp (U, \mathbf{X}, E) \mid F, C, W. \tag{A.2}$$

Peer Group Definition. The peer group G is determined by dorm assignment within each school–cohort–gender cell:

$$G = g(F, D, C, W). \tag{A.3}$$

Combining the dorm assignment assumption with the peer group definition yields

$$G \perp (U, \mathbf{X}, E) \mid F, C, W. \tag{A.4}$$

Peer Measures. Peer characteristics are leave-one-out group means of ethnicity and scholarship status:

$$S^E = h^E(G,\, E_{-i}), \qquad S^S = h^S(G,\, A_{-i}). \tag{A.5}$$

Implication for Identification. It follows that, conditional on school F, cohort C, gender W, own ethnicity E, and own ability signal A,

$$(S^E, S^S) \perp (U, \mathbf{X}) \mid F, C, W, E, A. \tag{A.6}$$

That is, within school–cohort–gender cells and controlling for individual observable characteristics, variation in peer coethnicity and peer scholarship share arises solely from quasi-random dorm assignment and is independent of individual confounders. This supports a causal interpretation of the estimated effects of S^E and S^S on Y.

A.2.2 From Stylized Assignment to Empirical Specification

The derivation above formalizes identification in a simplified setting in which peer groups are formed by school/faculty F and dorm assignment D. In the empirical analysis, we translate this logic into a richer set of controls that better reflects how students are actually exposed to peers and how academic outcomes are determined.

First, although the derivation conditions on school/faculty, we do not include school fixed effects in the estimating equation. This is because no academic major is offered in more than one school. As a result, controlling for major fixed effects absorbs all between-school differences in selectivity, grading standards, cohort composition, and academic resources, making separate school fixed effects redundant.

Second, we further condition on course-by-year fixed effects. This absorbs course-specific shocks, instructor effects, grading practices, and classroom-level diversity that could otherwise generate correlated outcomes across students facing similar peer environments.

Finally, we include high-school subject-combination fixed effects to capture differences in pre-university academic preparation. Students entering the same major may have taken different subject bundles in upper secondary school, which are strongly predictive of first-year performance and may also be correlated with peer composition.

With these additional controls, the identifying assumption implied by the derivation can be written more concretely as

$$(S^E, S^S) \perp (U, \mathbf{X}) \;\Big|\; \text{Ethnicity, Scholarship, Classroom, Major, Gender/Dorm, Cohort, HS subjects.}$$

Under this conditioning set, remaining variation in peer coethnic and peer ability shares is driven by quasi-random differences in group composition induced by administrative assignment rather than by student choice or unobserved heterogeneity. This is the identifying assumption implemented by the empirical specification in Section 4.

A.2.3 Additional Evidence against Selection

Table A.2 reports balance test regressions that are similar to the main empirical specification, with peer-group characteristics as dependent variables instead of academic outcomes. Column (1) uses the coethnic peer share and Column (2) uses the scholarship share as outcomes. All regressions control for the full set of controls and fixed effects, except that we include program-by-year fixed effects instead of course-by-year fixed effects since these regressions are estimated at the student level. Under non-random assignment, predetermined student characteristics would be correlated with peer composition.

Across both specifications, coefficients on pre-university characteristics are small in magnitude and largely statistically insignificant. For coethnic share, the joint F-statistic is 1.00, and the within R^2 is 0.0006. For scholarship share, the joint F-statistic is 2.03, driven primarily by home-district schooling; however, the within R^2 remains below 0.005, confirming that the explained residual variation is economically negligible. The overall R^2 of 0.72 and 0.74 reflects the explanatory power of the fixed effects rather than the covariates. Taken together, these results provide additional support for the identifying assumption that, conditional on the included controls, variation in peer coethnic and peer scholarship shares is driven by quasi-random assignment.

Table A.2: More Evidence against Selection

	(1) Coethnic share	(2) Scholarship share
Scholarship	0.001	0.005*
	(0.00)	(0.00)
Standardized test scores	-0.000	0.000
	(0.00)	(0.00)
Age	-0.000	0.000
	(0.00)	(0.00)
Anglican	0.001	0.005
	(0.00)	(0.01)
Catholic	0.001	0.004
	(0.00)	(0.01)
Muslim	-0.004	0.004
	(0.00)	(0.01)
Pentecostal	-0.000	0.008
	(0.00)	(0.01)
Unspecified religiosity	0.001	0.007
	(0.00)	(0.01)
Other religions	-0.001	0.018*
	(0.00)	(0.01)
Home district HS	0.001	0.005***
	(0.00)	(0.00)
Log peer group size	0.001	0.016*
	(0.00)	(0.01)
Observations	25,134	25,134
R-squared	0.72	0.74
F-stat	1.00	2.03

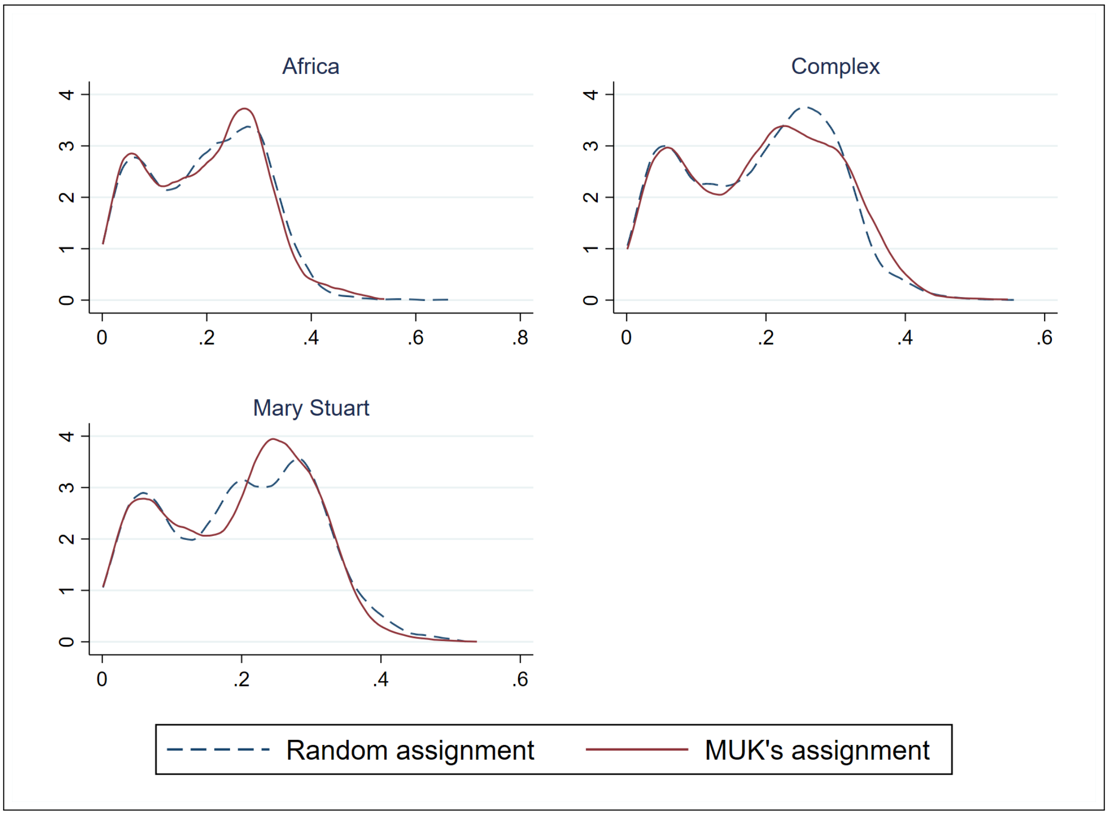
Notes: Data are from MUK and are restricted to students admitted to on-campus day majors for 2009–2017, excluding 2010. Each column reports an independent regression of either the coethnic peer share or the high-ability peer share on pre-university characteristics. All regressions include school-by-year, ethnicity, and hall fixed effects. Standard errors are clustered at the peer group level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.2.4 Graphical balance checks

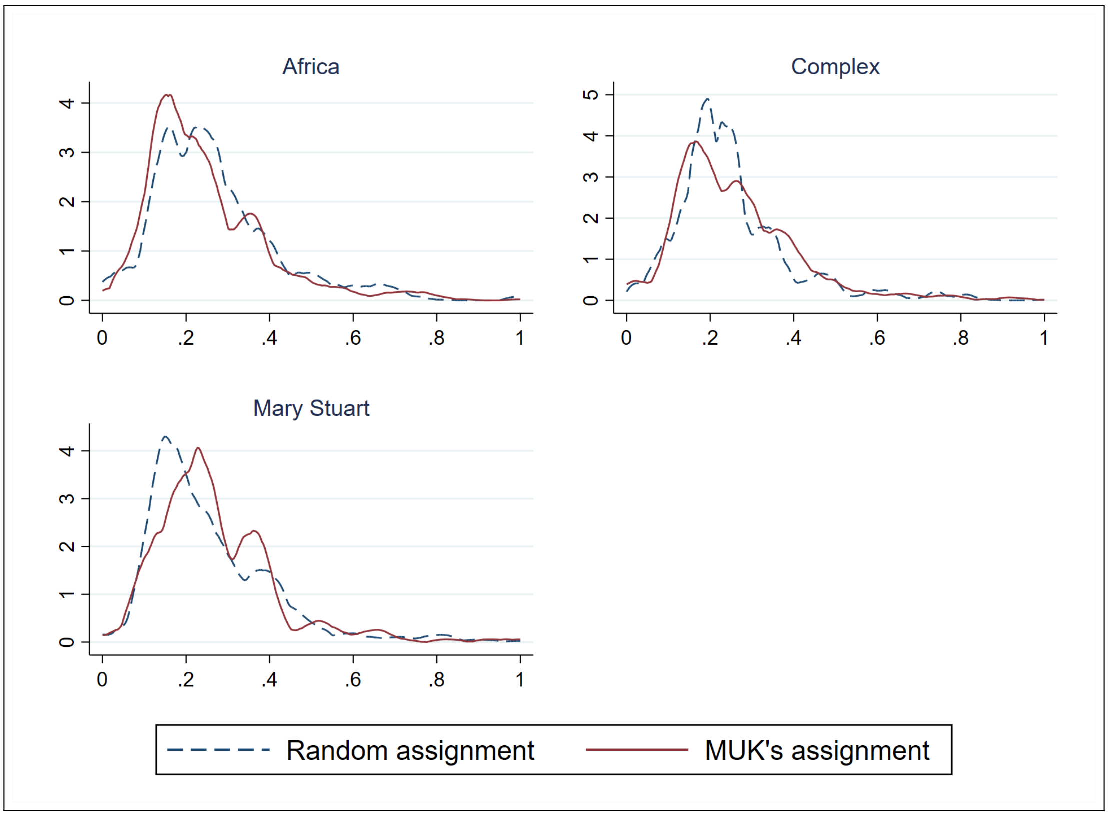
Figure A.3 compares the distribution of peer characteristics under the observed Makerere University assignment to a simulated random assignment for female dorms (male dorms are shown in the main text, Section 2.3.4). Panel A plots coethnic share, while Panel B examines scholarship peer share. Figure A.4 plots test score distributions for male dorms in Panel A and female dorms in Panel B.

Across dorms, the observed assignment closely mirrors the random benchmark, with only minor deviations. These overlapping distributions indicate that peer composition within dorms is largely driven by the random assignment process rather than systematic sorting. Together with the balance tests reported in the main text, these figures support the validity of the identification strategy and the interpretation of estimated peer effects as causal.

Figure A.3: Comparing Random to MUK's Assignment: Female Halls

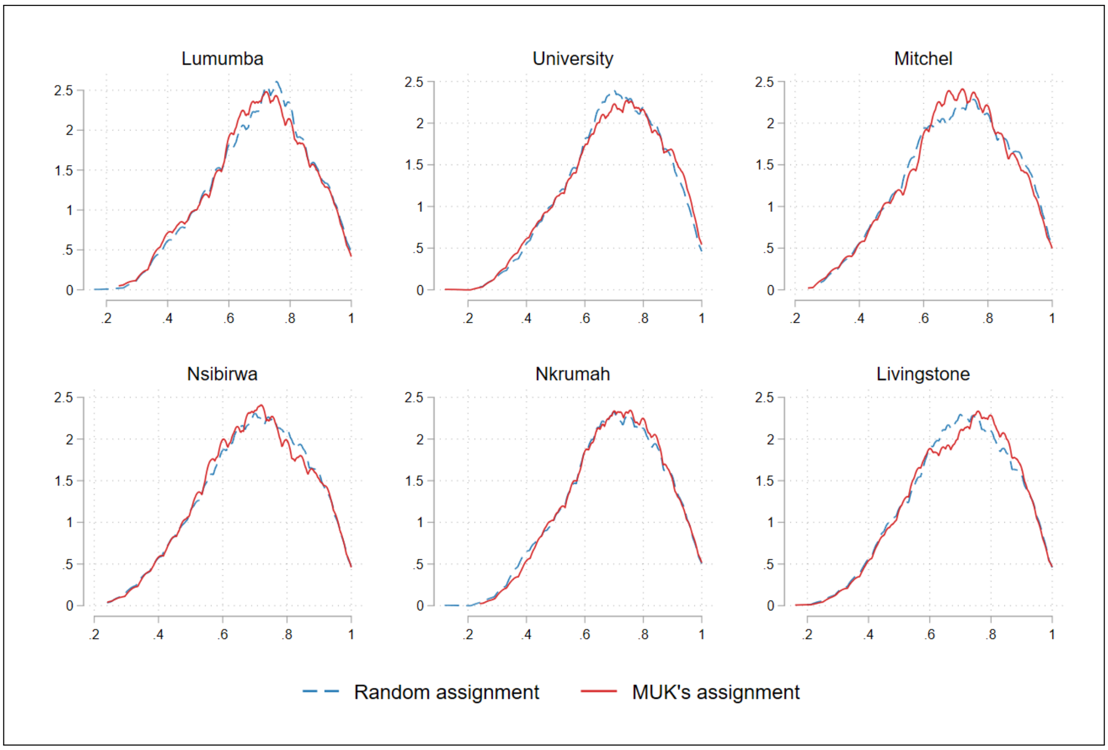


Panel A: Coethnic Share.

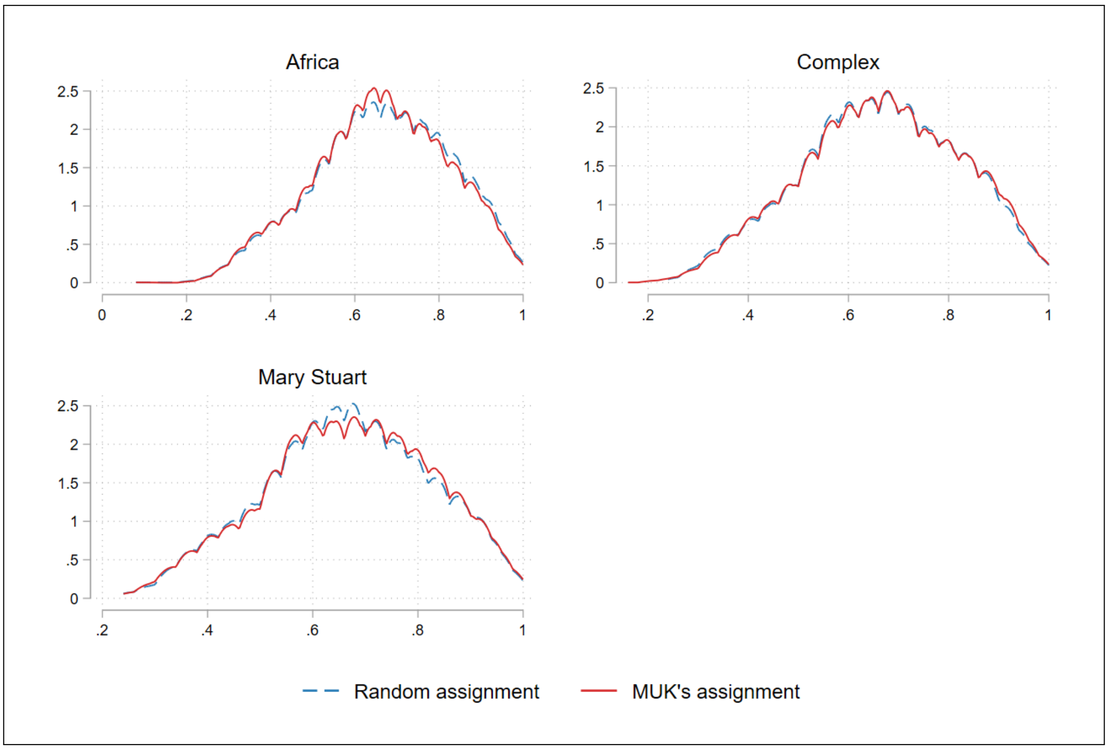


Panel B: Scholarship share

Figure A.4: Comparing Random to MUK's Assignment: Test Scores



Panel A: Male Dorms.

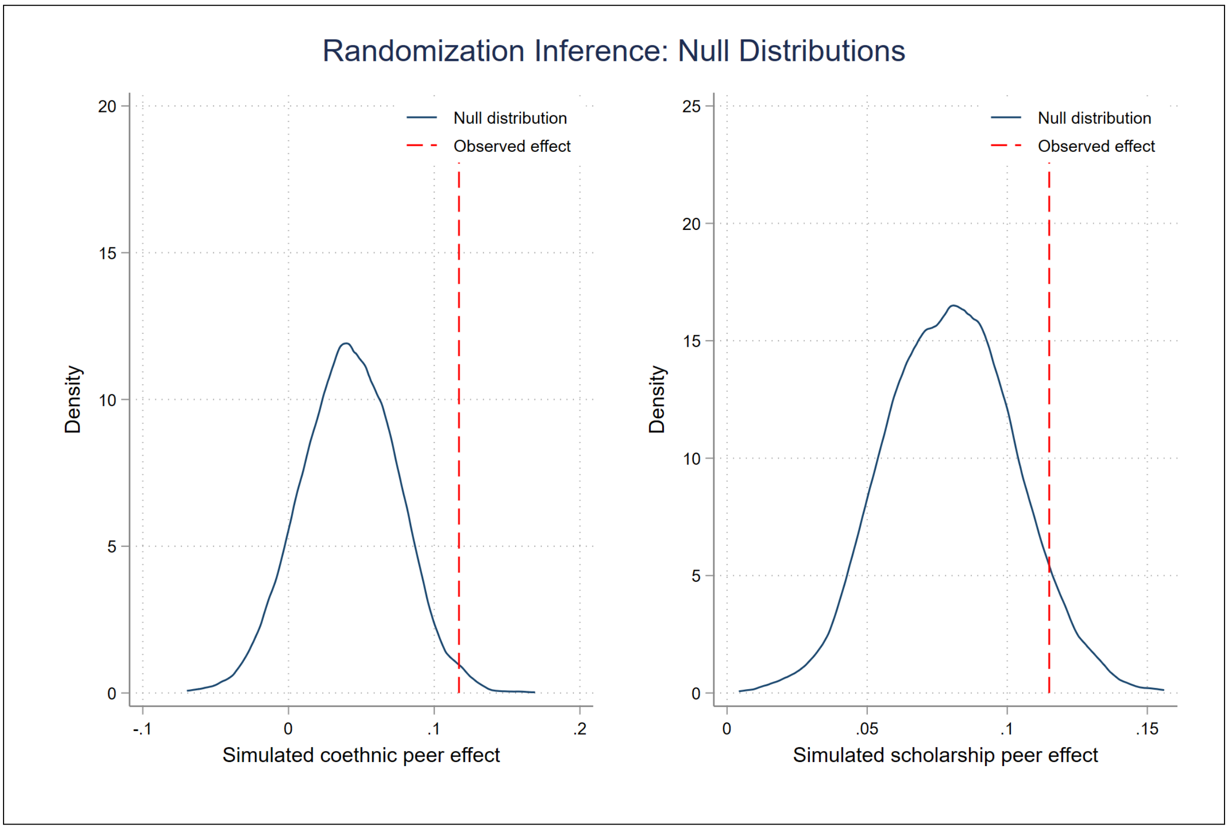


Panel B: Female Dorms

A.3 Randomization Inference

To complement the main results, Figure A.5 presents the randomization inference null distributions. Both observed effects lie in the far right tail, confirming the results are unlikely to arise by chance.

Figure A.5: Randomization Inference: Null Distributions



Notes: Each panel plots the kernel density of 2,000 simulated peer effect coefficients obtained by randomly re-assigning students to dorms within each cohort, school, and gender cell. The red dashed line marks the observed coefficient from the main regression. The randomization p-values are 0.011 for coethnic share and 0.063 for scholarship share.

A.4 Additional Evidence on the Signaling Channel from National Test Scores

Table A.3 reports additional evidence on the signaling channel by replacing scholarship share with UNEB test score measures of latent ability — a characteristic that, unlike scholarship status, is not publicly observable to peers. The first column duplicates the pooled specification from Table 3 and is included for ease of comparison. The remaining columns re-estimate the same specification replacing scholarship share with proxies of individual ability based on standardized national exam scores (UNEB), with peer ability constructed as leave-me-out means of these scores. Unlike scholarship status, these measures capture latent academic ability that is not publicly observable to peers.

Specifically, Column (2) replaces scholarship-based ability with standardized UNEB test scores. Column (3) uses residualized UNEB test scores that net out subject-combination and cohort effects, producing a cleaner within-group measure of relative ability and addressing

Table A.3: Mean Effects: Peer Latent Ability versus Observable Signals

	Scholarship signal	Z test scores (UNEB)	Residualized test scores (UNEB)	Top quartile (UNEB)
Coethnic share	0.114**	0.111**	0.114**	0.117**
	(0.05)	(0.05)	(0.05)	(0.05)
Peer ability measure (leave-me-out)	0.111***	-0.006	-0.077	-0.039
	(0.03)	(0.02)	(0.10)	(0.03)
Own ability measure	0.355***	0.275***	1.729***	0.331***
	(0.01)	(0.00)	(0.03)	(0.01)
R-squared	0.364	0.373	0.372	0.356
N	978,877	978,877	972,065	978,877
Dorm FE	Yes	Yes	Yes	Yes
Major FE	Yes	Yes	Yes	Yes
Course-by-year FE	Yes	Yes	Yes	Yes
HS Subject FE	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Group Controls	Yes	Yes	Yes	Yes

Notes: This table reports estimates from specifications that replace the scholarship-share proxy for peer ability with alternative measures based on UNEB test scores. Data are from MUK and restricted to students admitted to on-campus day majors between 2009 and 2017, excluding 2010. Peer groups are defined as students admitted to majors within the same school and assigned to the same dorm in each year. Each column reports an independent regression, with standardized course grades as the outcome. Individual controls include probable ethnic group, religious affiliation and whether the student graduated from the district of origin. Group-level controls include the leave-me-out averages of individual controls as well as peer group size. Standard errors, clustered at the peer-group level, are reported in parentheses.
*p<0.1, **p<0.05, ***p<0.01

concerns that raw score comparisons across subject combinations may be noisy. Column (4) defines ability using a binary indicator for top-quartile UNEB performance. All specifications maintain the same peer group definition, fixed effects structure, and control variables as in the main analysis, ensuring that differences across columns reflect only the alternative measurement of ability rather than changes in identification or sample composition.

While own test score measures of ability remain strong predictors of academic performance across all specifications, the corresponding peer effects are consistently small and statistically insignificant. These patterns indicate that peer effects based on latent ability, as captured by national standardized test scores, do not emerge, whereas peer effects associated with publicly observable scholarship certification are both economically and statistically significant. The stability of coethnic peer effects across all specifications further suggests that these effects are not driven by ability-based peer composition.

A.5 Additional Heterogeneity and mechanisms

A.5.1 Alternative Ethnic Salience level Proxies

Table A.4: Heterogeneous Coethnic Peer Effects: Alternative Salience Proxies

	(1) Home District HS	(2) A-level District (Continuous)	(3) Home District (Continuous)
Coethnic share	0.072	0.078	0.114**
	(0.06)	(0.05)	(0.05)
Scholarship share	0.108***	0.115***	0.113***
	(0.03)	(0.03)	(0.03)
Scholarship	0.357***	0.358***	0.356***
	(0.01)	(0.01)	(0.01)
Coethnic share × Pre-university ethnic exposure			
	0.145**	0.534***	0.134
	(0.06)	(0.12)	(0.10)
Scholarship share × Pre-university ethnic exposure			
	0.026	0.045	0.037
	(0.03)	(0.07)	(0.06)
Pre-university ethnic exposure	−0.029*	−0.105***	−0.047*
	(0.02)	(0.03)	(0.03)
R-squared	0.362	0.362	0.362
N	980,418	980,418	980,418

Notes: Data are from MUK and restricted to students admitted to on-campus day majors between 2009 and 2017, excluding 2010. All specifications include the full set of controls and fixed effects from the main analysis. Standard errors clustered at the peer-group level in parentheses. Column (1) uses a binary proxy for pre-university ethnic exposure equal to one if a student graduated secondary school from their home district (2010 boundaries). Columns (2) and (3) replace the binary proxy with a continuous measure equal to the share of the student's predicted ethnic group in their A-level district and home district respectively, demeaned at the district level. The contrast between columns (2) and (3) isolates the schooling environment as the operative channel: coethnic peer effects are driven by the ethnic composition of the A-level district, not the home district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This appendix presents additional analyses that assess the robustness of the heterogeneous coethnic peer effects results to alternative measures of the assumed level ethnic salience and provide additional evidence that it is the pre-university schooling environment, rather than the home environment, that drives these effects. Table A.4 reports estimates from specifications that similar to the main analysis but vary the way ethnic salience is proxied.

Column (1) reproduces the pooled specification from Table 6 and is included for ease of comparison. As discussed above, assumed high ethnic salience is captured using a binary indicator equal to one if a student graduated secondary school from the same district as their reported home district. The interaction between this indicator and coethnic share captures whether coethnic peer effects differ between students who experienced a pre-university environment similar to their home district and those who did not.

Columns (2) and (3) replace the binary proxy with a continuous measure of pre-university ethnic exposure. Both are constructed as the share of a student's predicted ethnic group in the relevant district, calculated using census data from UBOS and the classifier training data, and demeaned at the district level. Column (2) uses the district in which the student graduated HS; Column (3) uses the student's reported home district. These measures capture the intensity of ethnic exposure in the pre-university environment and allow for a "dose–response" interpretation. Also, the contrast between columns (2) and (3) is informative: if the schooling environment is the operative channel, coethnic peer effects should respond to the coethnic composition of the A-level district but not to that of the home district. The opposite would be true if home environment, such as family ties or simply growing up among coethnics were the important channel instead.

Across specifications, a consistent pattern emerges. Coethnic peer effects are substantially driven by students of higher assumed ethnic salience. Using the binary proxy in Column (1), these effects are concentrated among students who graduated HS from their home district. The continuous specifications sharpen this this interpretation: in Column (2), coethnic peer effects increase sharply with the ethnic dominance of the HS district, while in Column (3) the analogous interaction using the home district is small and statistically insignificant. This pattern isolates the schooling environment as the operative channel.

Importantly, students with limited pre-university exposure to other ethniciities have a negative baseline performance, indicating that students who remained in the same district or who attended more ethnically homogeneous schools tend to perform worse on average at university. The positive interaction between assumed high ethnic salience proxy and coethnic share therefore suggests a compensatory mechanism: students who arrive at university at an academic disadvantage due to differences in their pre-university environment benefit disproportionately from coethnic peers once exposed to a diverse university setting.

In contrast, merit-based peer effects are large and stable across all specifications and do not systematically interact with assumed ethnic salience. This asymmetry supports an interpretation in which heterogeneity in coethnic peer effects operates through identity- or familiarity-based channels rather than through general learning spillovers.

A.5.2 Differential Effects by Gender

We begin by discussing the rationale for heterogeneity along the gender dimension. Several studies document that peer effects on academic and non-academic outcomes differ by gender. For example, Carrell and Hoekstra (2010) study peer exposure to domestic violence in classrooms and find stronger peer effects for boys than girls. Using high-school GPA as a measure of peer quality, Stinebrickner and Stinebrickner (2006) find significant peer effects on study habits and academic performance for women at Berea College. More recently, Zárate (2023) document limited peer effects on academic outcomes but stronger effects on social and psychological skills in a field experiment in Peru, with effects varying by gender.

In addition, evidence from similar contexts suggests that friendship networks often form along ethnic lines (Salmon-Letelier, 2022), and such homophily may interact with gender if friendship networks overlap with study partnerships. Anthropological and experimental evidence further suggests that women may exhibit stronger homophilous tendencies than men (de la Cadena, 1995; Gallen and Wasserman, 2023). Tracking university students' friendships and study partnerships, Jackson et al. (2022) find that assortative homophily by gender and ethnicity persists over time. Together, this literature motivates examining gender differences in both coethnic and ability-based peer effects.

Table A.5: Differential Peer Effects by Gender

Year	**All**	**One**	**Two**	**Three**
Coethnic share	0.109*	0.115*	0.154**	0.085
	(0.06)	(0.07)	(0.07)	(0.07)
Scholarship share	0.108***	0.081**	0.118***	0.151***
	(0.03)	(0.03)	(0.03)	(0.04)
Scholarship (self)	0.355***	0.391***	0.357***	0.336***
	(0.01)	(0.01)	(0.01)	(0.01)
Female (self)	0.041**	-0.052**	0.061***	0.130***
	(0.02)	(0.02)	(0.02)	(0.02)
Coethnic share × Female	0.029	0.040	-0.020	0.038
	(0.06)	(0.06)	(0.06)	(0.06)
Scholarhip share × Female	0.018	0.024	0.012	0.018
	(0.04)	(0.05)	(0.04)	(0.05)
R-squared	0.362	0.325	0.377	0.373
N	980,418	319,376	338,878	325,576
Major FE	Yes	Yes	Yes	Yes
Course-by-year FE	Yes	Yes	Yes	Yes
HS Subject FE	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Group Controls	Yes	Yes	Yes	Yes

Notes: Data are from MUK and are restricted to students admitted to on-campus day majors for 2009-2017, excluding 2010. A peer group comprises students admitted to majors within the same school assigned to the same dorm in each year. Each column is an independent regression, but the outcome is the standardized course grades. The differences between each specification are indicated at the bottom and come from the controls. All regressions control for own ethnicity, gender, own ability, major FE, and HS subject combination FE. Dorm fixed effects are not included as dorms are single-sex, making them collinear with gender. Individual controls include religious indicators and graduating from the district of origin. Group controls include the leave-me-out averages of individual controls in addition to peer group size. Standard errors are in parentheses and are clustered at the peer group level.
*p<0.1, **p<0.05, ***p<0.01

Table A.5 reports estimates from specifications that interact both coethnic share and scholarship share with a female indicator. Across all specifications, we find no evidence that peer effects differ systematically by gender. The main effects of coethnic and scholarship peer exposure are broadly consistent with the baseline results. The coethnic share effect is positive and significant in the pooled and early-year specifications but attenuates by Year Three, while scholarship share exhibits positive and statistically significant effects throughout. The interaction terms between peer shares and the female indicator are small in magnitude and statistically insignificant across all specifications, for both coethnic and scholarship peer share. This suggests that the academic spillovers generated by these peer characteristics operate similarly for male and female students.

The coefficient on the female indicator itself varies across years of study, reflecting gender differences in average academic performance over time, but these differences do not mediate how students respond to peer composition. Overall, these findings suggest that the peer effects documented in the paper are not driven by gender-specific mechanisms, reinforcing the interpretation that salient peer characteristics shape academic outcomes broadly in this context.

A.6 Additional Results and Robustness Checks

A.6.1 Robustness to Alternative Construction of Coethnic Share

Table A.6: Mean Effects: Coethnic vs. High-Ability Peer Share

	Probabilistic (main)	Binary ethnicity assignment			
	All years	All years	Year 1	Year 2	Year 3
Coethnic share	0.117**	0.062*	0.064*	0.080**	0.038
	(0.05)	(0.03)	(0.04)	(0.04)	(0.04)
Scholarship share	0.115***	0.114***	0.086***	0.119***	0.153***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Scholarship (self)	0.352***	0.352***	0.380***	0.348***	0.329***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	978,877	978,877	318,266	337,158	323,377
R-squared	0.36	0.36	0.33	0.38	0.38

Notes: Data are from MUK and restricted to students admitted to on-campus day majors between 2009 and 2017, excluding 2010. Column (1) reports the main specification, in which coethnic share is constructed using predicted probability distributions of ethnicity. Columns (2)–(5) use a binary (0/1) ethnicity assignment to construct coethnic share and are included for comparison. All columns control for own scholarship status, individual characteristics, peer group means, peer group size, and include program-year, ethnicity, HS subject combination, hall, and course-year fixed effects. Standard errors in parentheses, clustered at the peer group level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

In the main analysis, we construct coethnic share using the joint-probability estimator in Equation 3, which uses the full distribution of predicted ethnicity probabilities for each student. Specifically, for each student i, coethnic share is defined as the leave-one-out mean of the joint probabilities that i and each peer belong to the same ethnic group. As a robustness check, we use construct coethnic share constructed using the categorical 0/1 assignment in Equation (2), in which each student is assigned to a single ethnic group based on their highest predicted probability and coethnic share is defined as the fraction of peers sharing that group.

Table A.6 reports estimates under both constructions. Column (1) reproduces the pooled specification from the main analysis and is included for comparison. Columns (2)–(5) replace the probabilistic measure with the binary assignment, with Column (2) pooling all years and

Columns (3)–(5) splitting by year of study. Coethnic peer effects remain positive and statistically significant under this alternative construction. While the estimated coefficient in Column (2) is smaller in magnitude than in Column (1), the two measures have different scales and standard deviations, making a direct comparison uninformative. Scaling by the respective standard deviations yields comparable effect sizes. Importantly, the pattern and relative magnitudes of the estimates across academic years are preserved. These results indicate that the main findings do not depend on the probabilistic construction of coethnic share used in the baseline specification.

A.6.2 Robustness to Alternative Fixed Effects and Control Sets

Table A.7: Mean Effects: Coethnic vs High-ability Share

	(1)	(2)	(3)	(4)
Coethnic share	0.136***	0.137***	0.128**	0.117**
	(0.05)	(0.05)	(0.05)	(0.05)
Scholarship share	0.117***	0.120***	0.120***	0.115***
	(0.03)	(0.03)	(0.03)	(0.03)
Scholarship (self)	0.357***	0.357***	0.354***	0.352***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	980,418	980,418	978,877	978,877
R-squared	0.36	0.36	0.36	0.36
Major FE	Yes	Yes	Yes	Yes
Course-by-year FE	Yes	Yes	Yes	Yes
HS Subject FE	Yes	Yes	Yes	Yes
Dorm FE	No	Yes	Yes	Yes
Individual Controls	No	No	Yes	Yes
Group Controls	No	No	No	Yes

Notes: Data are from MUK and are restricted to students admitted to on-campus day majors for 2009-2017, excluding 2010. A peer group comprises students admitted to majors within the same school assigned to the same dorm in each year. Each column is an independent regression, but the outcome is the standardized course grades. The differences between each specification are indicated at the bottom and come from the controls. All regressions control for own ethnicity, gender, own ability, major FE, and HS subject combination FE, but gender drops out (2)-(4) since dorms are single-sex. Individual controls include, religious indicators, and graduating from the district of origin. Group controls include the leave-me-out averages of individual controls in addition to peer group size. SEs are parentheses and are clustered at the peer group level.
*p<0.1, **p<0.05, ***p<0.01

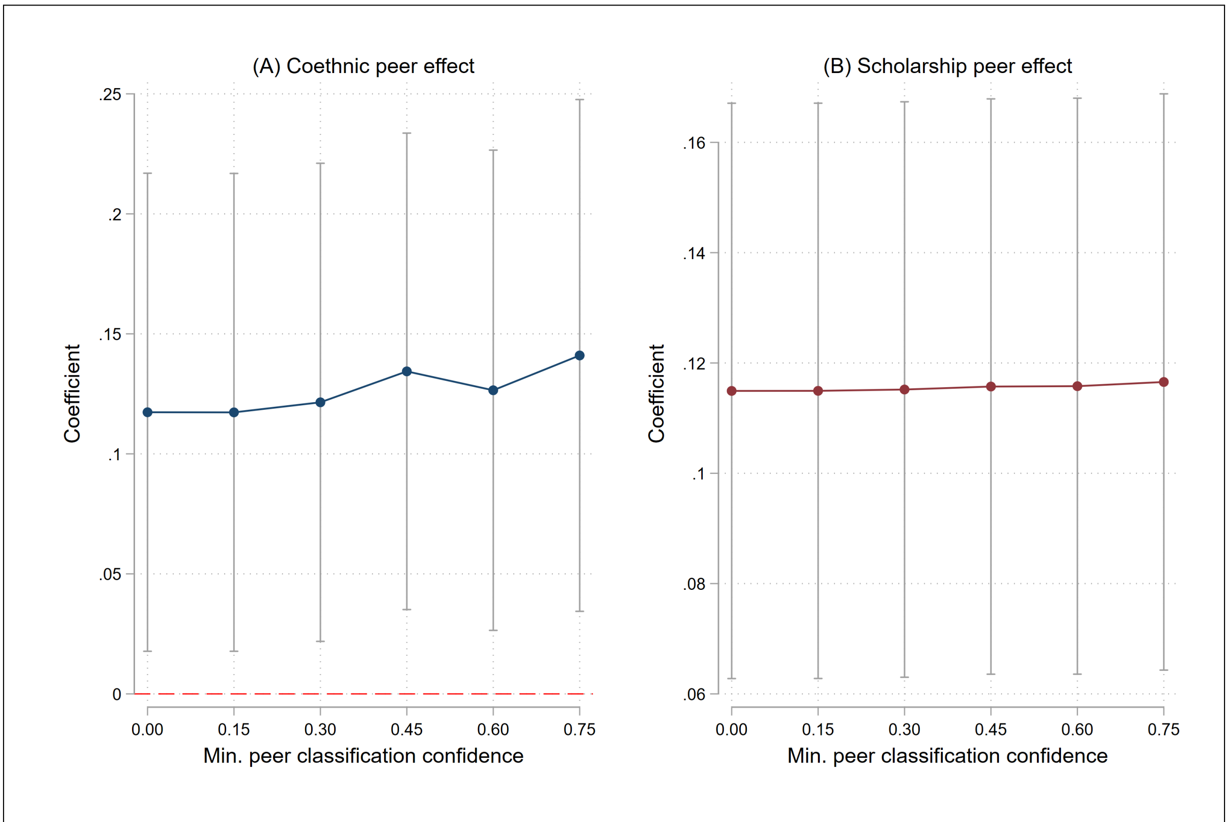
Table A.7 tests the robustness of the estimated peer effects to progressively richer sets of fixed effects and controls. Each column reports estimates from an independent regression using the same outcome and main peer variable measures, but differs in the fixed effects, individual controls, and group-level controls. By progressively adding different controls, we evaluate whether the estimated coethnic and scholarship peer effects are sensitive to alternative specifications or driven by omitted variables.

Column (1) presents a parsimonious specification without dorm fixed effects or additional controls. Column (2) adds dorm fixed effects, which absorb any time-invariant dorm-level characteristics and potential concerns related to dorm-specific environments that might affect our outcomes of interest. Column (3) further includes individual-level controls, including religious affiliation and whether a student graduated from their district of origin to account for observable background characteristics. Finally, Column (4) adds group-level controls, which are the leave-me-out averages of individual characteristics and the log of peer group size, and corresponds to the most restrictive specification used in the main analysis.

Across all specifications, the peer effects of both coethnic share and scholarship share remain stable in magnitude and statistical significance. The limited sensitivity of coefficients as controls are progressively added suggests that the main results are not driven by dorm-level confounds, individual sorting on observables, or correlated peer characteristics within groups.

A.6.3 Robustness to Classifier Confidence

Figure A.6: Sensitivity of Coethnic Peer Effect to Ethnicity Classifier Confidence



Data are from MUK and are restricted to students admitted to non-extension day majors for 2009-2017, excluding 2010. A peer group includes students admitted to majors within the same school and assigned to the same dorm. At each threshold, peers whose maximum predicted ethnicity probability falls below the threshold contribute zero to the coethnic share; the full sample is retained throughout. Panel B uses scholarship peer share — based on directly observed scholarship status — as a placebo. Threshold = 0 reproduces the baseline estimate. All regressions use the full main specification.

Our coethnic share measure relies on predicted cultural group probabilities from the surname classifier. To assess whether measurement error in these predictions attenuates the estimated coethnic peer effect, we construct a series of alternative coethnic shares that progressively restrict the peer predictions used in the calculation. Specifically, for each threshold $t \in \{0, 0.15, 0.30, 0.45, 0.60, 0.75\}$, we zero out the ethnicity contribution of any peer whose maximum predicted probability falls below t, while retaining the full sample and assigned peer groups throughout. Formally, peer j's predicted probabilities Π_j are set to zero in the leave-one-out sum when $\max_e \Pi_{ej} < t$. At $t = 0$ this reproduces the baseline measure exactly. As t rises, the coethnic share is computed from an increasingly confident subset of peer predictions, providing a direct test of whether low-confidence peer classifications are biasing the estimated effect.

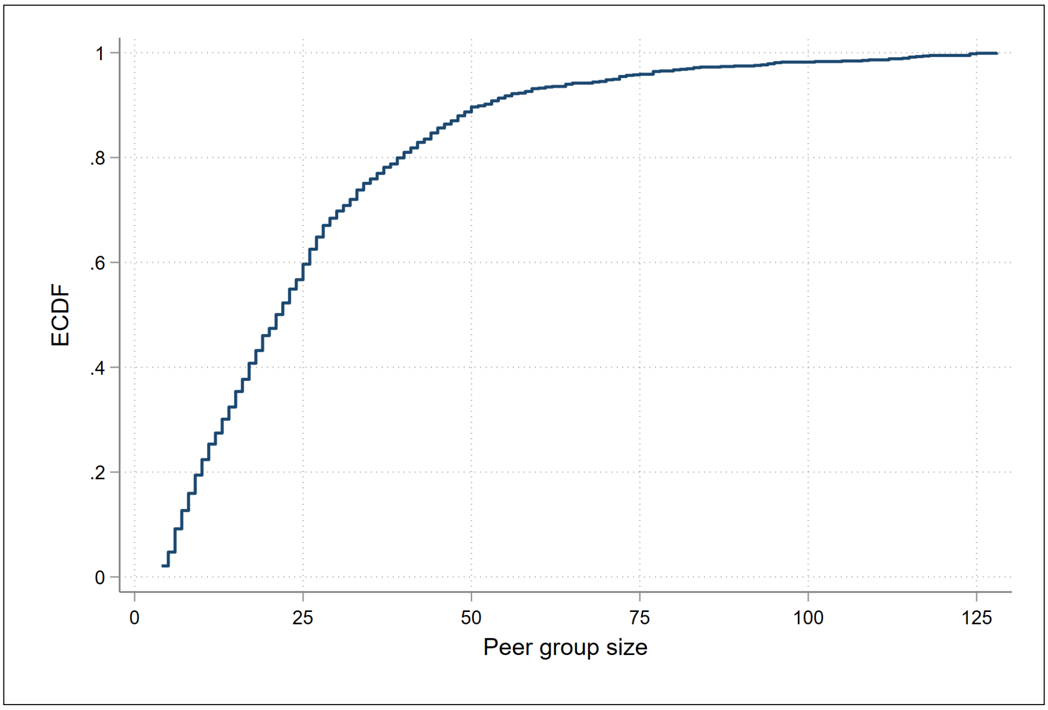
Figure A.6 presents the results. Panel A plots the coethnic peer effect coefficient and 95% confidence interval at each threshold. The coefficient is stable throughout, ranging from 0.117 and rising slightly to 0.140 at $t = 0.75$, and remains statistically significant across all thresholds. Panel B plots the corresponding coefficient on scholarship share — a variable based on directly observed scholarship status rather than predictions and thus acts as a within-regression placebo. This scholarship share coefficient is flat at approximately 0.115 (as the main effect) across all thresholds, confirming that the sample is unchanged and that variation in Panel A reflects changes in the coethnic measure specifically rather than sample composition. The stability of the coethnic coefficient across thresholds indicates that the result is not affected by measurement error in ethnicity predictions.

A.7 Additional Figures

This appendix subsection gives supplementary figures that provide descriptive context and more validation for the empirical analysis. These figures document key features of the data, the construction of peer groups, and the institutional assignment process at Makerere University.

A.7.1 Peer Group Size Distribution

Figure A.7: Peer Group Sizes.



Notes: Data are from MUK and are restricted to students admitted to on-campus day majors for 2009-2017, excluding 2010. A peer group includes students admitted to majors within the same school and assigned to the same dorm.

Figure A.7 plots the empirical cumulative distribution function of peer group sizes we use in the analysis. As aforementioned, we define peer groups at the dorm–school–cohort level, reflecting both academic and residential environments in which students interact. The figure shows substantial variation in peer group sizes, with most groups containing between approximately 10 and 40 students. This variation motivates controlling peer group size in our restrictive specifications although our peer effects do not change even when we do not control peer group size. Because peer composition measures are defined at the peer-group level, observations within the same group share common exposure and may have correlated error terms. We therefore cluster standard errors at the peer-group level.