Language, Identity, and Ethnic Cohesion in Schools

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Abstract

Distinct group identities can create barriers for trade, investment in public goods, and social learning. This paper studies how integration impacts productivity and cohesion in settings with broad linguistic diversity, where language differences may deepen the divides created by group identities. We randomly assign high school students to same- or different-ethnicity partners to play a cooperative video game in Macedonia, where students are segregated by ethnicity. We measure productivity using video game scores and assess social cohesion with two measures: the willingness to pay for a same-ethnicity partner (via an adaptive choice experiment) and ingroup bias (via a dictator game). We find that inter-ethnic contact improves social cohesion without affecting productivity. To examine the role of language, we exploit random variation in the language skills overlap within pairs. We find that shared language skills are associated with greater inter-ethnic cohesion gains and lower non-pecuniary costs. These results reflect both a match effect (i.e., sharing a language is beneficial) and a correlation between language skills and positive potential outcomes. Additionally, cross-ethnic pairs match on one of the partner's home languages or a third neutral language. Cohesion gains are largest for participants who match on their own home language; if anything, matching on a neutral language is worse than matching on the partner's home language.

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1 Introduction

Distinct group identities can create barriers to trade, investment in public goods, and social learning (Alesina and La Ferrara, 2005). These barriers are particularly pronounced when groups speak different languages, as linguistic differences can generate costly communication frictions and reinforce the salience of perceived identity divisions. Thus, early interventions that enhance social skills and shared linguistic human capital (Boucher et al., 2024; Alan et al., 2021; Chyn, 2018) could potentially have large positive effects on prosperity through adult social and economic interactions. However, allocating resources towards integrated education can come at the cost of educational efficiency. Indeed, learning in one's mother tongue can be more efficient, especially early in life, which typically requires segregation.[1] To balance this trade-off, policymakers commonly propose cooperative contact interventions—organized activities intended to foster greater cohesion between groups.

This paper asks how integration impacts productivity and cohesion in the presence of linguistic diversity. We do so in Macedonia, a country with a history of ethnic conflict, within high schools that segregate ethnic Macedonian, Albanian, and Turkish students by language of instruction. We randomize students into pairs to play a cooperative video game that requires teamwork and communication, with variation along two dimensions: whether partners share a common ethnicity and the overlap between partners' baseline language skills, which we call "communication potential". We measure cohesion with a choice experiment that elicits participants' willingness to pay for a same-ethnicity partner and measure ingroup bias in the amount of money shared during dictator games. Productivity measures come from video game scores.

We find that inter-ethnic contact improves social cohesion without affecting productivity, as scores in the game are not statistically distinguishable between same-ethnicity (control) and different-ethnicity (treated) pairs. On average, treated pairs have a lower willingness to pay for a same-ethnicity partner by 14 percent and ingroup bias is lower by 34 percent, leading to cohesion gains of 0.19σ.[2] At the same time, control pairs communicate at a much lower rate, per facilitator observations. Thus, students in cross-ethnic pairs overcome verbal communication challenges to perform as well as their peers in same-ethnicity pairs in the game, while improving their attitudes toward peers from different ethnicities. Nevertheless, students in cross-ethnic pairs

[1] The World Bank and related international institutions have promoted mother tongue (L1) instruction as a good education policy, (See also Piper et al. (2016) and Laitin and Ramachandran (2016)).

[2] σ is the control group standard deviations (see below).

report lower overall satisfaction with the experience. There are non-pecuniary costs to integration which can reflect prejudicial preferences or cognitive costs.

Language skills vary sufficiently so that randomly assigned *different-ethnicity* partners may or may not have overlapping language skills, or *communication potential*.[3] Higher communication potential is associated with more observed communication, a better self-reported experience (a proxy for utility), and greater cohesion gains with no statistically significant impact on productivity. These raw correlations do not account for a subtle form of bias. Students with stronger language skills and students in strata with cross-ethnic students who know their home language are likelier to have high *communication potential* matches. This creates a "selection" bias, which we address by borrowing an idea from the econometrics of spillovers. We extend the potential outcome framework to define several language-specific treatments such as high (above median) versus low (below median) communication potential (Aronow and Samii, 2017). As in the spillover setting, predetermined characteristics influence the likelihood an individual is assigned to a treatment.[4] The key insight is that the propensity score is *known* for specific match types that correspond to pre-determined observables. We perform re-weighting in an augmented inverse propensity weighted (AIPW) estimator. Results that rely on the communication potential are 'intent-to-treat' effects since it is an *a priori* measure that predicts but does not require communication between partners in a particular language.

Accounting for baseline skills, the correlation between communication potential and social cohesion can be decomposed into the benefit of being assigned a partner with high communication potential (match effects) and underlying heterogeneity correlated with language skills (selection). For example, language skills can reflect personality traits such as openness to experience that could also improve an individual's response to inter-ethnic contact. Alternatively, learning multiple languages can improve perspective-taking (Kinzler, 2020; Fan et al., 2015), which may be a fundamental building block for contact (Alan et al., 2021; Pettigrew and Tropp, 2006). The distinction is relevant for policy because they imply different solutions.[5] If match effects drive the results, then a policymaker could increase cross-ethnic cohesion by funding activities similar to our intervention while carefully pairing students with overlapping language skills. More broadly, teaching

[3] There is only linguistic variation in the different ethnicity pairs.

[4] "Exposure" in the language of Aronow and Samii (2017). In this analysis, the predetermined covariates are language skills.

[5] Formally, we define potential outcomes indexed by high (above median) or low (below median) communication potential and estimate the average effect of going from a low to a high match. We use propensity re-weighting (detailed below) to account for selection bias, nevertheless, the match is a bundled treatment since pairs, in theory, could systematically match on communication potential and some other unobserved match characteristics that demonstrate complementarities.

mutual languages more effectively could complement future integration efforts. The decomposition suggests room for matching as a policy tool for building cohesion. Counterfactual analysis shows that optimal (feasible) matching could have improved the observed cohesion gains by roughly 10 percent–and the gains would be 30 percent higher if everyone could be assigned a high communication potential match.

Finally, coordinating on a language can complicate integration. Different ethnicity pairs either match on one of the partner's home languages or a neutral language (*lingua franca*) like English. Psychology research shows positive home language effects.[6] Using one player's home language implies the other partner is speaking a language where they are likely less fluent and may feel disadvantaged. The history of conflict and policy debates about the role of Macedonian language in schools (see Section 2) raise concerns about backlash from non-Macedonian students in cross-ethnic pairs that match on Macedonian. Thus, home language effects could lead to positive overall cohesion effects that are one-sided. Further, the contact hypothesis says contact is effective when participants have equal footing (Allport et al., 1954). So, it is ambiguous whether and how the language of the match might matter.

We find a positive home language effect that is not offset by backlash. When a cross-ethnic pair has the highest communication potential in one of the students' home languages, the home language speaker shows large cohesion gains. The dictator game bias drops to nearly 0. At the same time, there is no evidence of backlash for the second partner who continues to have positive cohesion effects. Finally, while noisy, there does not appear to be a "neutral language" effect–if anything, using Macedonian increases cohesion outcomes relative to English for the non-Macedonian students.

Macedonia provides a close-to-ideal laboratory to study the role of language in inter-group contact due to its recent history of conflict and the fact that schools are increasingly segregated (Fontana, 2017; Bloodworth, 2020). In 2001, an armed conflict between Albanian militia and Macedonian authorities was resolved with the Ohrid Framework Agreement, which expanded the language rights of (large) minorities.While Albanians and Turks had access to home language instruction before 2001, the Ohrid Framework policy changes reduced access to and need for the Macedonian language (Fontana, 2017). For example, a 2007 policy change made English the first *second language* taught in schools. Non-Macedonians study Macedonian language starting in fourth grade for a couple of hours per week. Many communities face tradeoffs between cohesion building

[6] e.g. In a lab experiment, Jewish Israelis showed higher support for a peace plan when it was presented in Hebrew rather than English. The Hebrew effect held whether the proposal came from other Jewish Israelis or Palestinians (Grant et al., 2023).

and efficient learning in home languages.[7] In the United States, for example, one in five households use a language other than English, and state-level policies have oscillated between banning and providing well-resourced bilingual education.[8][9] Macedonia provides an example where language policy privileges home language instruction above nation-building throughout students' educational experiences.

We contribute to several strands of literature. First, we are the first paper to isolate the role of language in cooperative contact – and more broadly we show that the effectiveness of contact depends on participant skills, preferences, and the match between participants. We build on recent work with youth that has found mixed evidence of the effect of contact on cohesion: social preferences, attitudes, and behaviors toward outgroups. Some show strong positive results in attitudes and behaviors (Rao, 2019), others indicate changes in behavior but not attitudes (Scacco and Warren, 2018; Elwert et al., 2023), and still others report generally positive results, which degrade as the proportion of out-group team members increases (Ghosh et al., 2024). In work complementary to ours, Boucher et al. (2024) show that integration improves language acquisition of Syrian refugee pre-schoolers in Turkiye.[10] These and other studies of the effect of inter-group contact tend to treat individuals as exchangeable based the study-defining group membership; such as racial category (Corno et al., 2022), religious background (Scacco and Warren, 2018; Mousa, 2020), caste (Lowe, 2021), economic class (Rao, 2019), or cross-cutting group identity (Bursztyn et al., 2024; Finseraas et al., 2019). By incorporating a measure of baseline language skills, we show that inter-ethnic contact is less effective in improving cohesion if parties have communication barriers and also find that students who have higher language skills *ex ante* have better cohesion outcomes ("selection on gains"). Since skills can be learned (e.g. Alan et al., 2021) and assignments to contact experiences can be targeted, it is important to understand this heterogeneity.

Second, we contribute to the literature on the role of social identity on human behavior (Tajfel et al., 1971;

[7] The tradeoffs also consider controlling costs and developing language skills to access regional or international markets.

[8] Lawmakers most recently passed bans in California (1998), Arizona (2000), and Massachusetts (2002), though only Arizona's law still stands as of 2024. See Matsudaira, 2004 for a review. A tragic example comes from U.S. Indian policy where as recently as 1980 Native American students were sent to boarding schools where school officials beat them for speaking their languages (Churchill, 2004).

[9] India, with over 100 local languages, has struggled to implement a language policy that carves out more resources for home language instruction (Jain, 2017; Kumar et al., 2022). African governments, from South Africa to Uganda to Morocco, face similar challenges (Heugh, 2013; Miguel, 2004; Angrist and Lavy, 1997; Laitin and Ramachandran, 2022). The list goes on to include Pakistan, Indonesia, Kazakhstan, Ukraine, ethnically diverse regions such as Quebec in Canada, Catalonia in Spain, Bolzano in northern Italy or Luxembourg that support multi-lingual education systems (Ginsburgh and Weber, 2020). Areas in Latin America where indigenous language speakers have fought for the rights to mother tongue instruction (Hynsjö and Damon, 2016) and so forth.

[10] In fact, language skill development is often a result of extended inter-group contact, a point we return to in the conclusion.

Akerlof and Kranton, 2000). One strand of this literature seeks to understand why cleavages between groups become salient and why individuals identify with specific groups at specific times (Shayo, 2020; Posner, 2005). Previous work has highlighted the role of group status and group salience on identification with a group.[11] More recently, Atkin et al. (2021) show that costs can contribute to identification with a group. Whereas they refer to the market prices of group-specific foodstuffs and totems, we show that the language barriers impose non-pecuniary costs. Inter-ethnic pairs with higher communication potential demonstrate lower in-group favoritism and report lower non-pecuniary costs associated with contact. Thus, language differences increase individuals' identification with their ethnic group by increasing the costs of adopting a more inclusive identity. The fact that the cohesion gains are associated with a "home language effect"– suggests that this identification is not a function of being more "cosmopolitan" *a priori*.

Relatedly, we add to the literature on linguistic diversity and productivity. A common theoretical perspective is that linguistic diversity decreases productivity due to communication barriers (Lang, 1986), which is a finding supported by recent empirical work (Dale-Olsen and Finseraas, 2020; Guillouët et al., 2024). Others find that ethnic diversity in the workplace can lead to sabotage (Hjort, 2014). In our setting, communication barriers do not impact productivity and students have monetary incentives to provide effort for a joint payout. Yet, there may be non-pecuniary costs of inter-group contact and students are willing to pay to avoid these. The costs may reflect cognitive burden, in-group preference (homophily), or outgroup animus. We find that such costs decrease as communication potential increases, which highlights a positive externality to an individual's language learning, as in Lazear (1999). The result suggests that school systems that provide mother-tongue-based instruction create opportunities to build communication skills across groups through integrated classrooms for some portion of the day and extracurricular classes. More broadly, the work adds to our understanding of how language and communication influence social interactions (Durlauf and Ioannides, 2010).

Finally, we provide insights for experimentalists interested in working with paired experiments or measuring productivity and cohesion. The use of pairs in experimental settings raises several complications and opportunities. We introduce novel techniques for implementing and analyzing such experiments. While several studies focus on partners such as roommates (Boisjoly et al., 2006; Corno et al., 2022) or deskmates

[11] Some examples of endogenous identification based on status and salience include Nix and Qian (2015), Fouka (2020), and Blouin and Mukand (2019).

(Elwert et al., 2023), these studies tend to use simple randomization schemes and often focus on a "focal" partner. We randomize in a manner that ensures specific proportions of partner types while minimizing the recruitment costs for a given sample size. Further, we borrow from the literature on spillovers to estimate match quality effects based on predetermined characteristics. Moreover, we innovate on the measurement of social preferences and cohesion by integrating novel data sources and methods from outside of this literature. First, by using video games, a culturally appropriate medium for youth contact, we obtain a novel measure of productivity and effort in a team setting. Finally, we show how a measure from the labor economics literature on valuing workplace amenities can be adapted to provide estimates of the willingness to pay for segregation at an individual level (Drake et al., 2022).

Outline The paper proceeds by describing the institutional context (section 2). Next, we discuss the experiment (section 3) and data and data generation (section 4). The analysis takes part in two steps: First, we introduce the empirical strategy for studying the effects of an inter-ethnic partner (section 5) and present results (section 6). Then, we introduce the empirical strategy for studying the effects of language match (section 7) and show the role of match effects and the importance of which language students match on (section 8). Finally, we conclude (section 10).

2 Setting

Macedonia provides a useful setting for studying the role of language and integration. This section outlines the recent history of conflict, language policy's role in the conflict, and the recent negotiated resolution. Then, we show the implications of current language policy for the lived experiences of students in the three high schools of the study.

For background, Macedonia is a middle-income Balkan country of 2 million citizens with diverse ethnicities, languages, and religions. Nationally, ethnic Macedonians are the majority, with Albanians the largest minority, followed by Turks, Roma, and other less populous ethnicities.[12] Our study site is an ethnically diverse

[12] In the 2021 census at least 58 percent identified as Macedonian, 24 percent Albanian, and 4 percent Turkish with contingents of Roma, Serbs, Vlachs, among others. 7 percent of the population are "persons whose data are taken from administrative sources" and do not have an ethnic affiliation in the census. Censuses in Macedonia are politically fraught. https://makstat.stat.gov.mk/PXWeb/pxweb/mk/MakStat/MakStat__Popisi__Popis2021__NaselenieVkupno__Naselenie__EtnoKulturniKarakteristiki/T1008P21.px/

town, in which the Albanians are the majority of the population, followed by Macedonians and Turks as large minorities.[13]

In 2001, Macedonia faced a crisis that led to the death of 200 people, and the displacement of nearly 7 percent of the population (Merdzanovic, 2015). Following Kosovo and other Yugoslav Wars, Albanian militants aimed to gain broader rights in the young republic[14] where Macedonian political leaders had centralized governing powers, and restricted access to bureaucratic jobs as well as expression of culture.[15] To avoid broader conflict, Albanian and Macedonian political leaders negotiated a power-sharing framework, the Ohrid Framework Agreement of 2001. The Framework guided future constitutional amendments and policy changes that increased the rights of Albanians and other minorities. Notably, it made Albanian an official national language, provided for Albanian-language tertiary schools, and guaranteed bureaucratic jobs assigned by ethnic quotas controlled by the "Balancer" mechanism.[16] These policy changes likely increased ethnic segregation in schools.

Questions about language policy underscore long-standing political tensions about the role of *nationalities* and cohesion in the multi-ethnic state. Since at least 1950, governments for what now makes up Macedonia have funded Albanian-language primary and secondary classrooms, though at times government support was low.[17] For example, in the mid-1990s when Albanians opened an "illegal" Albanian-language university in the city of Tetovo, the government bulldozed several buildings and arrested its leaders on "charges of sedition" (Fontana, 2017, pg. 101). After the Ohrid Framework Agreement, local governments gained more control over schools, secondary schools in Albanian and Turkish expanded, and the Albanian-language university gained full recognition. These policy changes increased the sense that the country had "a set of ... parallel

[13]Gostivar census figures stand at 55 percent Albanian, 21 percent Macedonian, 12 percent Turkish with 6 percent not participating. Students from other municipalities attend high school in Gostivar lowering the Albanian share.

[14]Formerly a constituent republic of Yugoslavia, Macedonians voted for independence in a 1991 referendum, which Albanian citizens largely boycotted.

[15]The flying of national flags provides flash point where these tensions become visible. On July 8 1997, the government passed a law restricting the use of "foreign" flags, aimed primarily at the Albanian's double-headed eagle and the Turkish flag. In defiance of the law, the Gostivar mayor Rufi Osmani flew both flags over the municipal building. On July 9, 1997, Macedonian police forces removed the flags and raided the municipality killing 3 and injuring more(*Police Violence in Macedonia* 1998). Today, in front of the Gostivar municipal government building fly the flags of the municipality, Macedonia, Albania, Turkey, the Roma, and the United States.

[16]For a recent NY Times discussion see https://www.nytimes.com/2024/05/15/world/europe/north-macedonia-ethnic-balancer.html

[17]For example, nationalistic policymakers reduced access to non-Macedonian language instruction through the Law of Secondary Education 1985 (Official Gazette of SRM No 16/85). The law required 30 students from a language group in a cohort to have a secondary school class in their preferred language. As a result, the number of high schools offering Albanian language classes fell from 39 in 1980/81 to 5 in 1990/91 (Ortakovski (1998), see p. 362). The numbers rebounded post-independence.

education systems, depending on the language" according to 2010 policy document (cited in Fontana, 2017, pg. 257). The current approach to cohesion can be summarized as "if they don't interact, they won't fight."[18]

There is uncertainty about the role of Macedonian versus English as the language of common use in the country, which motivates our final set of research questions. English is considered a neutral language that could serve as a *lingua franca* if more students were to prioritize and master it. As of 2007, everyone begins English as a second language in first grade. Recent calls for integration focus on using full-time English-based classrooms in high schools, which are being piloted at several schools around the country (Bloodworth, 2020). In 2010, the government attempted to make Macedonian the *first* second language; the policy faced severe backlash and was overturned by the courts (Fontana, 2017). Currently, Albanian- and Turkish-tracked students commence Macedonian language instruction in fourth grade.[19] A common complaint by non-ethnic Macedonians is that ethnic Macedonians are not required to and, typically, make no effort to learn Albanian (or Turkish). Theory predicts such communication free-riding by the majority (Lazear, 1999). We show, below, that there is a mixture of language learning strategies across groups–though Macedonians rarely learn local out-group languages.

In the study site, the schools offer parallel tracks in Albanian, Macedonian, and Turkish.[20] In practice, students from distinct ethnic backgrounds attend the same physical schools but are segregated by the language of instruction from kindergarten through high school, limiting opportunities for cross-ethnic social interaction. As an illustrative example, consider two fictitious students: Amra, an Albanian girl, and Ana, a Macedonian girl, both born in the same year. Their parents enroll them in Goce Delcev Primary School for kindergarten through 8^{th} grade. Amra and Ana are rarely on campus simultaneously because the school day is divided into shifts organized by language.[21] Next, Amra and Ana matriculate into the Gymnasium, a college preparatory high school, which organizes school shifts by grade level. Although they begin the day with all first-year students regardless of ethnicity, their classes are conducted in their home languages, and their peers are predominantly of the same ethnicity. Additionally, Macedonian classrooms are in different hallways from

[18] Private conversation with a Macedonian education policymaker in October 2023. See also Fontana (2017, p. 257) citing the 2010 policy document *Strategy for Integrated Education*: "ethnic separation is seen as a measure of conflict prevention."

[19] Learning Macedonian in schools is mandated by the constitution: "In schools where education is carried out in another language, the Macedonian language is also studied" Article 48 of the Constitution of Macedonia (1991)

[20] As a note, Turkish identity reflects a remnant from the Ottoman colonial past, rather than recent immigration. (Ottomans held the territory of Macedonia for nearly 500 years until the First Balkan War of 1912.)

[21] The schools alternate which ethnicity takes the morning shift each week; during Ramadan, Albanian students take the afternoon shift for the entire month.

Albanian and Turkish classrooms. Thus, despite increased opportunities for cross-ethnic encounters, Amra and Ana's K-12 experience remains largely segregated. They would have similar experiences at the other two high schools in our setting. (The Medical High School also organizes shifts by cohort. The Technical School organizes shifts by ethnicity.) Outside of a school setting, students might have opportunities for cross-ethnic contact if they grow up in relatively integrated neighborhoods or participate in activities outside of school.[22]

Thus, high schools in Macedonia offer a unique setting to study language and integration. While students remain socially segregated, they attend the same schools, which lowers the direct costs of integration activities. Although students share some language skills, the level of overlap varies between pairs, revealing different communication barriers. Moreover, there is some political support for addressing these questions; policymakers wonder whether the Ohrid Agreement reforms have led to too much segregation (Bloodworth, 2020).

3 Study Design

In this section, we describe the recruitment and data collection timeline, how we randomized students into co- and cross-ethnic pairs, and the procedures for the lab-in-the-field experiment.

3.1 Recruitment and Timeline

All students at the three focal high schools were invited to participate in the study. From December 2023 to May 2024, enumerators visited classrooms, explained what participation would entail, and helped students complete the initial assent form. (Participation required parental consent as well.) Enumerators were always fluent speakers of the students' language of instruction. To avoid experimenter demand effects and reduce self-selection into the study, we framed the study around friendship and social learning. The consent language stated: *"We are conducting a study to understand how students make friends and learn about educational*

[22]Mixed families are rare–in our data, only 6 students report multiple ethnicities. We do not have data on the spatial extent of segregation in Gostivar–from discussions with locals, there are both mixed and highly segregated neighborhoods and villages.

opportunities. Participation includes answering questions and participating in games. As part of the study, you may have the opportunity to participate in free tutoring/school enrichment in English."

Data comes from two sources: (1) a baseline survey and (2) video game sessions. Upon receiving parental consent, enumerators conducted the baseline survey that featured modules on language skills, social networks and social closeness, parents, video games, educational outlook, and a real stakes dictator games.[23] Once both partners in a pair completed the baseline survey, we sent the session information to the facilitation team who organized the video game sessions described below.[24] Many of the same participants were invited to participate in our companion study, where students, in groups of 6, played a networked social learning game, where participants pass information to peers via a website we built for the study (Anisfeld and Pandiloski, 2024). The second study involves limited cross-group interaction as students are instructed to remain quiet. Nevertheless, we strive to have students complete the first three steps before participating in Study 2.

Data collection is ongoing. As of September 1, we have completed nearly all baseline surveys and 262 video game sessions (with 524 students). In this draft, we present estimates from these sessions focusing on results from the video game sessions. Our goal is 400 video game sessions.[25]

3.2 Randomization

We randomize students into pairs stratified by school, grade level, and gender; keeping gender and age constant within pairs to help isolate the role of language and ethnic identity. For efficient logistics and cost minimization, we randomized within strata on a rolling basis to provide steady work for the facilitation teams.[26] Randomization into pairs based on individual characteristics requires randomization that is more complicated than coin flips. Treatment is defined as having a partner of the same or different ethnicity, so we must ensure that individuals assigned to a specific treatment has an adequate match. Our procedure (detailed in Appendix B.1) adjusts *individual's* probability of being assigned a co-ethnic partner downward within

[23] The baseline survey is available upon request.

[24] A handful of students participated in the video game session before the baseline survey.

[25] We currently have an endline survey in the field. At least two weeks after the video game session, enumerators conducted the endline survey, which re-asked social network and behavioral measure questions from the baseline survey and invited students to sign up for an inter-ethnic course focused on communication skills. As of September 1st, we had 65 percent of the endline surveys for the current population.

[26] 11 strata have one wave of randomization, 10 strata have two waves, and 2 strata have three waves

strata to hold the *strata-wide* co-ethnic assignment proportion at 25 percent. The method maximizes the sample size given the recruitment effort. The procedure is fully automated, which allows us to determine *de facto* assignment probabilities and the probability of language skill match for an individual within a strata. We did not have access to self-reported ethnicity when conducting randomization and relied on language of instruction as a proxy. In practice, 96 percent of students' ethnicity matches the language of instruction.

3.3 Main intervention: video game sessions

In the video game session, pairs of students learn to play "Overcooked! 2", a cooperative video game where players work as chefs to prepare as many orders as possible under time pressure. During the sessions, facilitators led students to a room with two computers prepped for survey taking and video game play.[27] Before starting the survey, students introduced themselves and answered two icebreaker questions: "What languages do you speak?" and "What is your favorite class?" Then, using Qualtrics on separate computers, students verified their name and birth month, proceeded to learn how to play the game from YouTube videos[28], and then played practice rounds in "solo mode" where they control both chefs (described below).[29] The "solo mode" scores measure individual skill–the controls are identical to cooperative play.[30] Then, students play together in "cooperative mode" on a single computer, each student controls one chef and they work together to provide sushi (level 1.3) or pasta (level 1.5) to hungry, impatient customers. Pairs play each level twice, a practice and an "official round", wherein students get payouts based on the team score. In addition to learning and playing "Overcooked! 2", students answered questions about their experience in the session and completed an adaptive choice experiment, which provides additional behavioral measures. Based on their scores in "official rounds" and dictator game responses, we calculate payouts that the field team leaders deliver at a later date. The entire session lasts an hour on average, with pairs interactively playing the game for roughly 20 to 25 minutes.

"Overcooked! 2" has many advantages for cooperative contact and studying social interactions.[31] First,

[27] We hired teachers from the school to organize and run sessions. Each session had two teacher facilitators so that each student had at least one teacher who taught in their language of instruction, or was fluent in their language of instruction. We directed facilitators to interfere with gameplay as little as possible, and not at all during the "official play".

[28] We produced these in each of the local languages.

[29] Students play level 1.1, watch a second video, and then play level 1.2 twice. The second time is an "official round", which counts toward their payout.

[30] These measures may be affected by treatment assignment, so our main specification does not use them.

[31] For researchers considering using a cooperative game for a research project, we considered a few off-the-shelf alternatives.

students work cooperatively towards a common goal on equal footing, matching the criteria set out by the contact hypothesis (Allport et al., 1954). Second, productivity in the game mirrors a real-world kitchen (minus the knife skills) and in some ways is more challenging than the real-world setting, because players must self-organize their production process. To provide some flavor of how the game works, level 1.5 has chefs prepare spaghetti with red sauce. To prepare one dish requires boiling pasta, chopping tomatoes, sauteing tomatoes, plating the pasta and the sauce before either one becomes "overcooked"[32], and finally delivering the plated dish to the window. As orders go out, plates get dirty and must be washed. On top of this, the kitchen is in hot air balloons that shift over time as wind gusts blow them and the chefs across the screen. While each chef could work independently, like Adam Smith's pin factory, there is profit in the division of labor. We hypothesized that shared language skills would have a meaningful impact on students' ability to divide labor and score points. Since many, if not most, real-life scenarios for integration require communication, this feature of the game helps mimic realistic conditions. Finally, video games are a culturally appropriate medium for youth contact. In our sample, two-thirds of students could recommend a video game by name, 55 percent said they played some days or more frequently and 72 percent said the expression "I love video games" describes them somewhat or more.[33] Boys tend to enjoy video games more; the figure is 87 percent for boys and 60 percent for girls. The boys score more points on average, but we do not find heterogeneous treatment effects by gender.

4 Data and Measures

4.1 Sample characteristics, baseline integration, and balance

In Table 1, we present summaries of baseline characteristics and balance tests for the sample of students who have participated in the experiment through June 2024.[34]

"Keep Talking and Nobody Explodes" gives players asymmetric information as partners race to diffuse a bomb. The game instructions and manual are too English-centric for our use case. Some puzzle games require cooperation, such as "Pico Park", but productivity is harder to measure in these games. We also considered "Moving out". Earlier research in communication studies used "Overcooked! 2" in a lab setting (Velez et al., 2021).Benatov et al. (2021) devise a bespoke curriculum for "contact through gaming" using Minecraft and similar work could be conducted in Roblox. Kovalčík et al. (2023) discuss additional games used to study teamwork and cohesion.

[32] In which case, the kitchen catches fire!

[33] At least 5 on a scale from 0 to 10.

[34] Data collection paused during the summer holidays and resumed in October 2024. The fieldwork will be completed by December 2024. Table A1 and A2 contain data for the full sample who have been randomized partners. The full sample appears more

The ethnic composition of the sample reflects that of the town. The plurality of students are in Albanian language of instruction (LOI) tracks, followed by Macedonian LOI and then Turkish LOI. Ninety-six percent of participants report an ethnicity that matches the language of instruction.[35] In practice, 7 students whose LOI does not map to their ethnicity are assigned to control unit "co-ethnic" pairs.[36] The hard-to-classify individuals attenuate treatment effect estimates as some control units may act like treated units. The main demographic variables are balanced across treatment and control except Albanian LOI. In the current sample, the treatment group skews Turkish.

In this segregated environment, baseline measures show low social integration and cohesion (See Table 1). First, social networks are quite segregated. We elicit social networks using several methods including explicitly asking students to name up to six cross-ethnic peers. Only 45 percent of students name at least one peer who speaks a different mother tongue; compared to 96 percent who name at least one friend. In total, 88 percent of all distinct named links are co-ethnic.[37] Second, we find that students feel very close to students from their language-of-instruction (LOI) group and quite far from students from different LOIs. We measure this using the *inclusion of self-in-others* measure where a respondent selects one of 7 pairs of circles that range from barely touching to nearly completely overlapping (Aron et al., 1992). One is labeled "you" and the other has the name of a group (i.e. "Turks", "students from the Medical School"). We ask "what picture best describes your relationship with [group identifier]?". On average, students give their in-group LOI a 6.4 out of 7 while out-group LOIs average 2 out of 7 (barely overlapping circles). Finally, students share less with out-group members in dictator games. On average, there is a 12 percent gap in baseline sharing (See section 5).

The treatment group in the analysis sample has hints of imbalance. They name .14 more cross-ethnic links than the control ($p < .05$), yet they give less money to outgroup peers in dictator games (-15 denar, $p = .1$).

"balanced" as the p-values appear closer to a uniform distribution.

[35]There are two prevalent exceptions. First, There are 15 Roma students in our study. Roma are the fourth largest ethnic group in the Municipality (<4 percent, Per census data. https://makstat.stat.gov.mk/PXWeb/pxweb/mk/MakStat/MakStat__Popisi__Popis2021__NaselenieVkupno__Naselenie__EtnoKulturniKarakteristiki/T1008P21.px/). Students from Roma backgrounds tend to complete schooling in Macedonian and report the strongest sense of belonging to Macedonian students. Using the Inclusion of Others in Self measure, Roma students feel close to Macedonians at a level slightly below what Macedonian students report (5.6 versus 6.4 out of 7). Second, roughly 10 percent of students who identify as Turkish in this study complete coursework in Albanian or Macedonian. Several tracks within the technical high school (e.g. "architecture") are not offered in Turkish. Additionally, some ethnically Turkish students speak a different local language as a home language.

[36]There are 16 students in this group altogether in the current sample but the 'treated' units fortunately have a truly "cross-ethnic" partner. The classification issues are slightly larger in the full sample affecting 36 of 822 students.

[37]This represents a lower bound as 148 students use all 6 slots for their friend clique.

We use generalized random forests (described below) with these and other baseline controls which adjust the estimates to account for chance imbalance.

4.2 Communication potential

Communication potential is a key component of the analysis. Partners may or may not share proficiency in English, Macedonian, Albanian, or Turkish. *A priori*, we would like to know to what extent partners have mutual communication skills, which will help understand match effects.

Figure 1 shows that many but not all students report high skills in a second language.[38] The figure shows we can expect variation in whether two random students have complementary language skills. As expected, Macedonian students tend to have low second-language skills in local languages and specialize in English. Meanwhile, Albanians and Turks have more varied language skills.[39]

Operationalizing *communication potential* requires measures of individual language skills and a method to combine them to characterize the complementarity of skills. To measure the language skills, we employ a commonly used survey instrument the Language Experience and Proficiency (LEAP) Questionnaire (Marian et al., 2007). Students self-rate their English, Macedonian, Albanian, and Turkish skills using a scale of 0 to 10 in four domains: speaking, listening, reading, and writing. We average to provide one score per language.[40] Then, for pair i and j, we determine a joint score in a given language by multiplying i and j's scores.[41] We define *"communication potential"* as the maximum joint score over the four languages. Formally, each student i has a vector of linguistic skills $f_i \in [0, 10]^4$. Then, for pair (i, j)

$$\underset{\text{communication potential}}{f_{ij}^{\max}} = \max(f_i \odot f_j)$$

where $\odot$ denotes the element-wise (Hadamard) product and the $\max$ operator selects the largest element in

[38] That is they report an average of 7.5 on a scale from 0 to 10 for skills in that language. See below for details on the measure.

[39] Linguists say language similarity predicts ease of learning; here, the languages all come from distinct branches of the language tree; Albanian, Macedonian and English are Indo-European languages that are Albanian, Slavic, and Germanic respectively; Turkish is a Turkic language and has the least similar grammar.

[40] Averaging helps reduce noise in the self-reported skills.

[41] This corresponds to the geometric mean (squared).

the vector. The "communication potential" reflects the two-way nature of communication and the inherent complementarity of communication skills. The "max" captures first-order communication skills. For co-ethnic pairs, $f_{ij}^{\max}$ is typically $10 \times 10 = 100$. For cross-ethnic pairs, we see substantial variation in $f_{ij}^{\max}$. It has a median of 75, an interquartile range of 62 to 88. Students are likelier to use languages with high joint scores; less than 10 percent of pairs have a second-place language match above 75 suggesting that the language associated with the top score is the most likely to be used for communication. Facilitators report on pairs language usage which provides supporting evidence; Roughly 3 out of 4 pairs that talk more than "a moderate amount" use the language with the highest communication potential (See Appendix D). By design, almost every control pair has a communication potential of 100. So analyses that rely on the communication potential measure focus on treatment pairs only.

4.3 Outcomes

We build 4 indices to measure cross-ethnic cohesion, productivity, student experiences (non-pecuniary benefits), and communication. Together these capture the main trade-offs in integration. Building cohesion or reducing prejudice is typically the purported goal of integration, but this can come at a cost to productivity or have non-pecuniary costs. Communication is a mechanism hypothesized to explain these costs (Dale-Olsen and Finseraas, 2020; Guillouët et al., 2024; Lang, 1986) and serves as a proof of concept for the "communication potential" measure. For each outcome type, we construct indices based on 2 to 3 related variables. To allow for comparisons, we normalize each measure $\tilde{y}_i$ by subtracting the control group mean μ_0 and standard variation σ_0:

$$y_i = \frac{\tilde{y}_i - \mu_0}{\sigma_0}$$

Then, the index is the simple average of all composite variables.

4.4 Outcomes: Cross-Ethnic Cohesion Measures

The purpose of contact interventions is to build cross-group cohesion or reduce cross-group prejudices. Social ties across groups, prosocial preferences, and an absence of violence characterize a cohesive school environment. We measure these concepts using survey instruments:

1. Willingness to pay for same-ethnicity partners
2. Out-group bias measured with dictator games

Adaptive Choice Experiment: Willingness to pay for same-ethnicity partners

The amount a student is willing to pay for a co-ethnic partner provides a snapshot of how much the student values segregation. Intuitively, students who chose co-ethnic partners over a cross-ethnic partner with a larger bonus are willing to forgo money to maintain segregation (Hedegaard and Tyran, 2018). An individual's valuation may reflect ingroup preference (homophily), out-group bias (animus), or expected non-pecuniary benefits, such as lower cognitive burden or more fun. The expectation of non-pecuniary benefits is a form of statistical discrimination. Let us define prejudice as the sum of animus and the extent to which the students' expectations of non-pecuniary benefits are incorrect. Then, treatment effects net out the homophily channel and "correct" statistical discrimination, so treatment effects can be interpreted as a decline in prejudice.[42] In any case, the decrease in willingness to pay for a same-ethnicity partner implies an increase in willingness to cooperate across ethnic lines.

Students face 12 binary choice problems where they reveal their preferences regarding partner ethnicity, partner language skills, and a sure cash payment.[43] Bayesian updating determines the next trade-off in a process analogous to a binary search (Drake et al., 2024). (In Appendix C, we detail the theoretical basis and estimation) The benefit of the machinery is that it allows us to estimate multiple parameters (see below) at the *individual* level where previous work has relied on take-it-or-leave-it offers which provide a single data

[42] This is under the assumption that homophily is unchanged. In general, changes in homophily and prejudice are not easily distinguished.

[43] The choice problem is framed as an opportunity to provide input for an end-of-year event. Students read: "At the end of the study, Gostivar Municipality hopes to host a competition where you can play 'Overcooked! 2' again. We will let you choose the characteristics of your partner and give a cash bonus for playing with them." We stress that there is no payment for gameplay scores and the partners should be considered equal in every other way.

point to understand the demand for segregation/integration (Hedegaard and Tyran, 2018; Rao, 2019; Lowe, 2021).

We focus on the parameters of the following linear utility function:

$$U_{ij} = p_j + \theta_i \mathbb{1}_{\{ethnicity_i = ethnicity_j\}} + \gamma_i^e \mathbb{1}_{ESL_j} + \gamma_i^m \mathbb{1}_{MSL_j} + (\gamma_i^{eo} \mathbb{1}_{ESL_j} + \gamma_i^{mo} \mathbb{1}_{MSL_j}) \times \mathbb{1}_{\{ethnicity_i = ethnicity_j\}} + \epsilon_{ij}$$

Where p_j is a sure price paid for choosing partner j with a utility parameter normalized to 1. Terms $\mathbb{1}_X$ equal one if X is true. θ_i is the WTP for a co-ethnic partner. γ_i^e and γ_i^o are the willingness to pay for the partner's second language skills in English (ESL) or Macedonian (MSL). (Recall, English and Macedonian are taught in school and we are interested in the role of neutral *lingua franca* compared to the language of the dominant group.) γ_i^{eo} and γ_i^{mo} capture the additional valuation of a co-ethnic partner's second language skills.[44]

We estimate these parameters using population Monte Carlo in two steps. First, we estimate the vector of $\{\theta_i\}_{i \in N}$ using a nuisance prior, and then we determine an empirical prior using the mean and standard deviation of the leave-one-out set of parameter estimates (See Appendix C).[45]

The utility parameter θ_i captures the amount i is willing to pay for a co-ethnic partner holding language skills constant. As stated above, the parameter cannot be interpreted as a direct measure of discrimination. However, average differences in θ (i.e. a treatment effect) can be interpreted as a change in the "price of prejudice".

The individual willingness-to-pay parameters induce a demand curve that allows us to visualize heterogeneity in the population. We build the demand curve by counting what fraction of the population is willing to pay for a characteristic at a given price level. (The curve is one minus the CDF of the distribution of WTP measures). Note, as with any quantile treatment effects, the effects are only identified across distributions,

[44] The term γ_i^{mo} is meaningless for Macedonian respondents as their co-ethnics speak Macedonian as a first language.

[45] In practice, the nuisance prior has a limited effect on the estimates; the correlations between a nuisance prior with standard deviation 100 versus one with standard deviation 200 is roughly .98.

not for individuals at a given quantile. To get at the latter, we need an additional strong assumption that individuals maintain their rank across treatment assignments.

To provide support for the validity of this measure, we examine correlations between language skills and demand for students who speak English or Macedonian as a second language. Students with an additional point of fluency (on a 10-point scale) in a given language are willing to pay 3.5 to 5 denar for a partner who speaks that language.[46]

Bias in dictator game sharing

Dictator games are widely used to measure prosocial preferences of altruism or fairness norms and bias. Regardless of the underlying preference parameter, dictator games performed on networks show that people share more with people who they feel closer to (Goeree et al., 2010). For this reason, we focus on the difference between the amount shared with co-ethnic versus cross-ethnic pairs.

In a dictator game, each participant is asked to split 600 Denar (roughly $12 USD) with a peer.[47] Each question reads: "Imagine you are splitting 600 denars between yourself and the randomly chosen **student from your high school with the same mother tongue as you**. They will not know who split the money. Decide how much of the 600 denars to send to the **student from your high school with the same mother tongue as you**. You will keep the rest of the money." Students make four splitting decisions varying the anonymous recipient's mother tongue ("the same" or "a different") and school membership ("from your high school" or "from another high school"). Some choices are realized, adding an incentive for truthful reporting.[48] We measure out-group bias by subtracting the average amount shared with a co-ethnic partner from the amount shared with a cross-ethnic partner. In the baseline survey, students share 29 Denar (13.5 percent) more with their co-ethnic partners on average.

We conduct dictator games during a baseline survey, directly after the contact intervention, and again during the endline survey, which allows us to measure persistence or fade-out.

[46] These come from simple linear regressions of willingness-to-pay price on the fluency measure with or without language of instruction fixed effects. In reality, the demand is non-linear in the fluency measure.

[47] We say 50 denar equals $1 when in reality the exchange rate is closer to 55 denar for $1. The national minimum wage in 2023 was 20,175MKD per month, which amounts to roughly 120 MKD per hour. Purchasing power is higher in Macedonia. An espresso cost between 40 and 60 MKD in Gostivar in mid-2024.

[48] These games have been used successfully with high school students and younger children (e.g. Ghosh et al., 2024; List et al., 2017).

4.5 Outcomes: Productivity

The productivity index comprises of:

1. Cooperative game scores
2. Quiz scores on how to play the game

Our primary measure of productivity is the total points scored in the "official rounds" of cooperative play. Players get points for successfully delivering dishes; they get tips for delivering dishes quickly and even more tips for delivering the dishes in the same order in which they came in; conversely, players lose points for failing to deliver an order in time.[49] The total score encapsulates total productivity.[50] Because students play in solo mode first, we have access to a baseline video game ability for each student. However, this measure may be influenced by treatment assignment. We observe sample imbalance which could be by chance or reflect differential effort in the presence of different types of partners. Our main specifications forgo using these scores as a control, but we also present robustness checks where we include the scores and find similar results either way (see below).

Students complete a quiz on how to play "Overcooked! 2". The quiz includes questions about the mechanics of the game controls and details about recipes and scoring. Learning is an important productivity outcome in a school setting. We explicitly teach some of the quiz content in students' home languages. This mimics learning in a bilingual classroom where bilingual course material is available.

4.6 Outcomes: Communication

The communication index comprises of:

1. Amount of communication, rated by facilitators

[49] Each order has a timer which is visualized as a bar that turns from green to red as the bar shrinks.

[50] In future work, we will use image detection algorithms to extract additional information from the 120 hours of screen recordings. While the total score provides a holistic measure of productivity, the number of failed orders might provide a distinct measure of cooperative effort. We can also fairly easily extract the time series of points scored, which could provide a richer picture of playing styles. More challenging computationally, we could train a model to identify which actions go into making a dish and determine how much joint effort goes into the point totals.

2. Ease of communication, rated by students

Facilitators rate the amount of communication and which languages they heard used. Facilitators rate "How much did the students communicate during the joint game?" on a seven-point scale. Inter-rater correlation is high (.78). The second measure uses students' ratings on ease of communication which correlates with facilitator scores (.47).[51]

4.7 Outcomes: Student Experience (non-pecuniary costs/benefits)

Experience captures non-pecuniary costs or benefits of the "job". This captures a potential compensating differential that could affect wages and explain in part the demand for segregation.

The experience index comprises student self-reports on their experience playing with their partner on three dimensions:

1. Enjoyed playing
2. Would play again with the same partner
3. Was partner fun?

The student measures are highly correlated within students, correlation coefficients range between 0.6 and 0.73. Facilitators also report, from their vantage point, how much fun each player seemed to have and how frustrated they seemed to be. These measures correlate less tightly but in the expected direction with student self-measures. Correlation between facilitator-assessed enjoyment and student self-measures ranges from .27 to .33. Facilitator-assessed frustration and student self-measures are negatively correlated and range from -.23 to -.30. The concordance between measures gives us confidence that student self-assessments are reliable.

[51] In future iterations, we hope to analyze audio from the screen recordings. This may allow us to unpack the quality and content of communication. However, we are concerned that sound quality is not uniformly good.

5 Empirical Strategy

5.1 Main results

To estimate average treatment effects, we run the regression:

$$y_i = \alpha + \tau D_i + \epsilon_i$$

and

$$y_i = \tau D_i + \phi_g + \beta X_i + \epsilon_i$$

y_i is an outcome for individual i (normalized as described above) or an average of these variables (index), D_i is an indicator of being assigned a cross-ethnic partner, τ is the treatment effect and object of interest reflecting the causal effect of participating with a cross-ethnic partner rather than a co-ethnic partner. α is the control group mean, which typically equals 0 or is close to zero due to normalization. ϕ_g is a strata fixed effect (school $\times$ grade $\times$ gender) and X_i is a baseline measure of the outcome if it exists. Specifically, we include solo scores[52] for productivity measures and baseline dictator game bias for cohesion measures. We cluster the standard errors at the strata by randomization wave level.

5.1.1 Robustness with causal forests

We use causal forests, a "doubly robust" treatment effects estimator, to test the robustness of the main results against finite-sample bias due to chance imbalances and against biases due to variation in individual-level treatment propensity scores (Athey, Tibshirani, et al., 2018). At a high level, we include predetermined covariates to address chance imbalances and use inverse propensity weights to address variation in treatment scores.[53] We use the `grf` package in R to implement the causal forest, following Algorithm 1 from Athey

[52] As mentioned earlier, these are measured during the session and could be influenced by treatment assignment–e.g. if being in the same room changes effort. On the other hand, they are measured before the main contact experience. Ultimately, the results are similar with or without the solo scores included. We present this in the results.

[53] Covariates include: *demographics*: indicators for grade level, school, gender, the language of instructions, whether the ethnicity matches the language of instruction; *re-randomization based propensity score* probability of being assigned control, probability of a high language match; *language ability*: fluency measure for Albanian, English, Macedonian and Turkish, as well as the max

and Wager (2019) who recommend a data-driven approach for covariate selection which is said to improve the stability of the estimates.

The causal forest provides estimates of $\tau(X_i) = \mathbb{E}[Y_i(1) - Y_i(0)|X_i = x]$ using random forests to estimate an adaptive kernel weights $\alpha_i(x)$ as well as auxiliary regression functions for the mean outcome value $\hat{\mu}(X_i) = \mathbb{E}[Y_i|X_i = x]$ and the propensity score $\hat{p}(X_i) = \mathbb{E}[D_i|X_i = x]$ (Athey, Tibshirani, et al., 2018).[54] The estimator combines these in the causal forest to estimate conditional average treatment effects given pre-determined covariate vector x:

$$\hat{\tau}(x) = \frac{\sum_{i=1}^{n} \alpha_i(x)(Y_i - \hat{\mu}^{(-i)}(X_i))(D_i - \hat{p}^{(-i)}(X_i))}{\sum_{i=1}^{n} \alpha_i(x)(D_i - \hat{p}^{(-i)}(X_i))^2}$$

The superscript $(-i)$ denotes the estimators are calculated using subsets of the data that exclude i (i.e. they are "out-of-bag" predictions). Finally, the individual-level predictions are aggregated to provide average treatment effects using an augmented inverse-propensity weighting (see equations 8 in Athey and Wager (2019)).

The assumptions required for the causal forest to consistently estimate treatment effects are selection on observables (conditional exogeneity) and overlap. For the analysis of inter-ethnic contact, the treatment assignment indicator is exogenous and all but two students have a probability of being assigned treatment or control due to our randomization procedure.[55] With these assumptions met, the algorithm returns unbiased estimates of the average treatment effects with asymptotic normal standard errors, which are clustered following Athey and Wager (2019).

fluency measure for non-mother tongue local language; *socio-economic indicators*: indicators for cellphone ownership, whether a parent attended college, whether the student went to a Gostivar middle school, and if the student intends to get a graduate degree; *baseline measures*: trust score, life satisfaction score, number of friends named, number of cross-ethnic friends named, baseline dictator game bias, baseline dictator game sharing with the outgroup; *IOS measures*: measure for own ethnicity, mean of other ethnicities, own school and mean of other schools; *video game*: Likert score for "enjoy video games", indicator for whether they make a recommendation.

[54] The random forest regressions also use the adaptive kernel weights, so that $\hat{\mu}(x) = \sum_j^n \alpha_i(x) Y_i$

[55] The two students were the only representatives of their language of instruction within a strata, so we technically cannot assign them to the control condition.

5.2 Correlation between "Communication potential" and outcomes

We next turn to the role of language. The following regressions explore the heterogeneity between communication potential and outcomes across treated students (in cross-ethnic pairs). Recall there is almost zero variation in communication potential in the control group. We run:

$$y_i = \alpha + \gamma * D_i + \beta * D_i * \frac{f_{ij}^{\max}}{IQR} + \epsilon_i$$

β, the parameter of interest, estimates how much outcome y changes on average when moving from the 25^{th} to 75^{th} percentile of the "communication potential" distribution among different-ethnicity pairs. The interquartile range (IQR) goes from 61.75 to 90 points or approximately 1.5 standard deviations.

To summarize effects, we also use the following specification, after splitting the treated sample at the median "communication potential".

$$y_i = \beta^L D_i \times \mathbf{1}_{\text{match}_{p(i)}<m} + \beta^H D_i \times \mathbf{1}_{\text{match}_{p(i)}\geq m} + \gamma_g + \beta X_i + \epsilon_i$$

We are interested in β^L and β^H, which represent the contrasts between the control group and the average y_i for individuals in pairs with Low and High language matches. These matches are defined as being below or above the median m, respectively.

6 Contact Results

6.1 Contact improves cohesion without lowering productivity

We first present an overview of the "contact intervention" results. Students randomly assigned to a same-ethnicity partner communicate significantly more and report a better overall experience compared to those with cross-ethnic partners. Yet, treated students display lower out-group bias and lower willingness to pay

for a co-ethnic partner. Moreover, productivity levels are similar across treated and control pairs. In short, contact improves cohesion without lowering productivity, but has non-pecuniary costs.

The following paragraphs flesh out these results and put the standard deviation effects in perspective. Figure 2 shows treatment effects (i.e. $\tau = E[Y_i(1) - Y_i(0)]$) for indices and the items that compose them in standard deviation units, along with the ATE estimates from the causal forests (shown in the lighter shade). In Tables 4 and 3, we report the same results and also include covariates.

Students in cross-ethnic pairs had a harder time communicating than those in co-ethnic pairs (Figure 2, rows 1-3) and had a worse experience (Figure 2, rows 4-7). The communication challenges motivate understanding the role of language in later analysis. To put treatment effects in perspective, facilitators report the amount of communication on a 7-Likert scale with "a moderate amount" of communication as the midpoint. 88 percent of co-ethnic pairs communicated more than "a moderate amount" compared to 36 percent for treated groups.[56] The experience index captures the non-pecuniary costs of working across ethnic lines. To put the experience results in perspective, we asked students to what degree they agreed with the statement "I would like to play again with my partner". 97 percent of control students said *agree* or *completely agree* compared to 85 percent of students in mixed pairs.

While students in cross-ethnic pairs had trouble communicating, we cannot reject a null effect on productivity measures (Figure 2 rows 8-10). Same-ethnicity pairs scored 325 points on average. Mixed-ethnicity pairs score 9 fewer points (s.e. 16) on average. The productivity loss is less than 3 percent and indistinguishable from 0 (CI: -41 to 22.4 points). The effects are nearly identical when accounting for video game skills (as measured with solo scores) and gender by cohort fixed effects. Moreover, students perceive their partner's skills similarly across treatment arms, even after accounting for the partner's solo scores. Note, observed communication is positively correlated with productivity. To put this in perspective, in the full sample, having moderate communication or more is associated with an extra 54 (se = 16.7) points in the official rounds of the game. Compare this to the relationship with solo scores. When the sum of a pairs solo scores is one standard deviation higher, they 52 (se = 9.9)

The final three rows of Figure 2 show that playing with a cross-ethnic partner improves cohesion and decreases bias. The cohesion index comprises of ingroup bias measured through dictator games and willingness-

[56] 55 percent of co-ethnic pairs talked "Constantly" compared to 11 percent of cross-ethnic pairs.

to-pay for co-ethnic partners, multiplied by negative 1. Cohesion increases by .18 standard deviations, reflecting similar magnitude statistically significant declines in sub-measures. In levels, ingroup bias falls from 22.3 to 14.5 Denar. This reflects a 30 percent drop in ingroup bias.[57] (See below for a discussion of willingness-to-pay.)

Treatment effect estimates are quite stable when estimated with causal forests (lighter points on 2). This is reassuring, but not surprising given the data come from a randomized experiment where the propensity scores are very similar across units and the samples are relatively balanced on observables.

6.2 Contact shifts demand for segregation inward

Demand curves show the distribution of valuations which provides insights beyond mean differences. Figure 3a and 3b show demand curves induced by the individual valuations for a co-ethnic partner and for a partner who speaks Macedonian as a second language, respectively. The curves report the empirical probability that an individual's willingness to pay is above some price over the support of prices, censored at -100 Denar.

Starting with the control group's demand (red line), roughly 10 percent of students are willing to pay to have a cross-ethnic partner.[58] Most students are willing to pay for co-ethnic partners and demand is disbursed over the support from 0 to 500 Denar.[59] Roughly 20 percent of students have WTP above 300 Denar, roughly the expected value of participating in the video game session.[60]

The demand curve for students assigned a cross-ethnic partner (blue line) bends inward starting at the 75th percentile.[61] So while WTP declines by 20 percent on average for students assigned to cross-ethnic partners, "cohesion gains" are not uniform. Assuming the ranking of valuation does not shift dramatically with treatment assignment, the results are consistent with a model where students update their preferences and those who initially are close to indifferent due to low prejudice are not impacted by the experience.

[57] Another way to put the estimates in perspective is to compare them to other studies. In a recent study of a two-week inter-ethnic camp in India, participation reduced ingroup bias in dictator games by .28σ **ghosh˙creating˙nodate**, roughly double the effects found here.

[58] We use language of instruction as 'ethnicity' in this analysis. As noted earlier, for a small portion of students, the language of instruction is not their ethnicity. In the analysis not shown, we drop these students and get visually similar demand curves.

[59] Roughly 0 to 10 USD

[60] 25 percent of the expected value of participating in the study in total.

[61] A Kolmogorov-Smirnov test rejects the hypothesis that the demand curves were drawn from the same distribution (p = .05).

The language results provide corroborating evidence. The demand for Macedonian as a second-language has a similar shape to the co-ethnicity demand curve for Macedonian students, but we see no difference between treatment and control students from non-Macedonian tracks. In other words, among Macedonian students willingness to interact increases with other ethnicities when they do not speak Macedonian as a second language. This is consistent with the ethnicity results; suggesting more openness to working across ethnolinguistic lines.[62] Finally, Figure 3c shows no treatment effect on WTP for English as a second language. The mean WTP for English is positive and roughly 30 percent of the average WTP for a co-ethnic partner. Students value English skills on average, and the treatment does not appear to influence that valuation.

6.3 Language match predicts outcomes

Communication potential has significant positive associations with communication, experience, and cohesion. At the same time, we fail to reject the null that there is no relationship between communication potential and the productivity index. We characterize the relationship between communication potential and outcomes in two ways (see 5.2). Figure 4 shows the linear relationship and Tables 5 and 6 provide the non-parametric summary comparing students in pairs with below and above median communication paper.

First, The communication potential score is highly correlated with the communication index. This correlation supports using "communication potential" to measure a pair's ability to communicate, while only relying on each student's self-reported fluency vector. Every 30-point increase in communication potential$_{ij}$ is associated with a .78 (se =.13) standard deviation gain in the communication index. To put these numbers in perspective, from the bottom to top quartile, the average amount of communication increases by 1.6 points on a 7-point Likert scale. These move from 3.3 to 4.95, where 4 is "a moderate amount". (For reference, the control mean is 6.1). Second, students in pairs with higher communication potential report better experiences; while there is a substantial gap in experience reported for the lowest matches, the treated individuals approach the control group average as $f_{ij}^{\max}$ approaches 100. Third, cohesion is increasing in communication potential. Low matches have marginally significant gains in cohesion, while high matches have highly significant positive gains. Moving from the 25th to the 75th percentile of the communication potential dis-

[62] Appendix Figure A6 shows demand for partners with specific language skills disaggregated by ethnicity (aggregating across treatment assignment). The median Macedonian student is willing to pay over 100 Denar for their partner to know Macedonian as a second language, whereas the median non-Macedonian student has a WTP of 0.

tribution is associated with an extra 0.1 standard deviation in the cohesion index. While the high match pairs see statistically significant gains, the evidence is consistent with a story that says having low communication potential results in little to no reduction to ingroup bias. Finally, we cannot reject the null hypothesis that there is no relationship between communication potential and productivity. The noisy association is negative when looking at the cooperative game scores. Interestingly, there is a positive and significant correlation between scores and the communication index; This can happen if some pairs with lower communication potential, communicate more and produce more than expected based on the associations shown in figure 4.

Taken together, we find strong, but suggestive, evidence of communication potential influencing students' experiences and cohesion outcomes.

Figure 5 shows the demand for segregation. The biggest demand shift occurs for students in high-match pairs (above median communication potential match) in the middle of the distribution. The students in the low-match pairs have demand curves very similar to the control until the highest prices. The Kolmogorov-Smirnov test rejects the null that high-match and control demand are drawn from the same distribution ($p < .001$) and fails to reject the null for low-match versus control (p = .69). However, as we have already hinted at, the inward bowing of the "high match" versus "low match" demand curves can reflect heterogeneous treatment effects or selection bias. In other words, students who have better language skills may also have other characteristics that interact with assignments to a cross-ethnic pair. In the next section, we discuss how to move toward causal estimands of mechanisms.

7 Towards Causal Heterogeneous Effects: Exposure Mapping

Randomly assigning partners gives us hope that we can untangle two effects. One is the benefit of having a partner with a high "communication potential": we call this "match effects". The other is underlying heterogeneity. Students with better language skills are more likely to be in a high-language match pair. And at the same time, they may share traits that affect both their language skills and their cohesion outcomes.

We do this by estimating propensity scores with respect to communication potential from baseline skills. Then, we use re-weighting to estimate causal effects. This is the idea behind "exposure mapping", which is a

technique used to measure spillovers in network settings (Aronow and Samii, 2017). In this section, we first provide an illustrative example; then, formally define exposure mapping; and finally, bring the technique to our data and discuss limitations.

7.1 Illustrative example:

A toy example clarifies how selection bias *could* occur when splitting the sample based on a pre-determined characteristic and how probability weights derived from the randomized design can be used to estimate unbiased treatment effects. Consider a strata with two types of students in a trilingual environment. Bilingual students, labeled B, have fluency in their home language and another language, while monolingual students, labeled M, only know their home language. Though we are equally likely to get $(M, M), (B, M), (M, B), (B, B)$ pairs, the probability a pair is a high language match is not equal. Bilingual students are likelier to get a high language match pair than monolingual students. In cross-ethnic pairs, two monolingual students (M, M) will *never* have a high language match, while two bilingual students (B, B) will *always* be a high language match. Depending on the particulars, (B, M) or (M, B) pairs may or may not be language matches. In our toy example, the $B - M$ pairs have a 50 percent chance of being a high match so that the overall propensity scores are $p(B) = \frac{3}{4}$ and $p(M) = \frac{1}{4}$, where $p(X) = \mathbb{E}[\text{High Match}|X]$. Meanwhile, co-ethnic pairs always have a high-language match.

To highlight the bias, Figure 6 shows results from a stylized simulation with constant treatment effects due to cross-ethnic contact. We set $\tau = \mathbb{E}[Y(1) - Y(0)] = 0.2$ for all pairs, but we assume heterogeneity in baseline cohesion measures. This is plausible. Bilingualism may endow B students with better perspective-taking (Kinzler, 2020; Fan et al., 2015), which could, in turn, increase their cohesion measures regardless of treatment (Alan et al., 2021).

Then, the true data-generating process is given by:

$$
\begin{aligned}
Y_i &= \beta_i^L D_i \times (1 - H_i) + \beta_i^H D_i \times H_i + Y_i(0) \\
\beta^L &= E[Y_i(LowMatch) - Y_i(0)] \\
\beta^H &= E[Y_i(HighMatch) - Y_i(0)]
\end{aligned}
$$

Where $Y_i(0)$ captures underlying heterogeneity, D_i is a treatment indicator, and H_i is an indicator of having a high language match. In the toy example, we set constant effects so $\beta_i^L = \beta_i^H = \tau$.

A few observations: First, while bilingual students are likelier to be in a high language match pair, the pooled results return $\tau = \mathbb{E}[Y(1) - Y(0)] = 0.2$ since this is a randomized experience. Next, when we split the sample by language match, we get biased estimates–it appears there are heterogeneous treatment effects because the conditional treatment effects use the unconditional expected outcome of the control group $\beta^{\text{Match Type}} = \mathbb{E}[Y(1,1)|\text{Match Type}] - \mathbb{E}[Y(0)]$. This insight suggests we can recover the conditional ATE (CATE) if we can estimate $\mathbb{E}[Y(1,1)] = \mathbb{E}[\mathbb{E}[Y(1,1)|\text{Match Type}]]$. Inverse propensity weights allow us to address the biased CATEs.[63] Moreover, we know the true propensity score because we know how baseline characteristics map to a "high" or "low" match given the randomization scheme.

7.2 Exposure mapping:

Here is how the toy example maps to the empirics from the previous section. First, recall in Section 5 we proposed a regression summarizing the effect of a high or low match:

$$y_i = \alpha + \beta^L D_i \times \mathbf{1}_{\text{match}_{p(i)}<m} + \beta^H D_i \times \mathbf{1}_{\text{match}_{p(i)}\geq m} + \epsilon_i$$

β^L and β^H may suffer from selection bias as in the toy example. To unpack the estimands, we extend the potential outcome framework and then show how we can estimate potential outcomes of having a high or low language match partner. Then, the difference between these potential outcomes provides an ATE of having a high match versus a low match.

We define $d(z, l)$ where index z is 1 if the student is in a co-ethnic pair and the index l is 1 if the pair has a high-language match. This produces three potential treatment assignments

- $d(0) = d(0, 1)$: co-ethnic pair
- $d(1, 0)$: cross-ethnic pair, low language match

[63] In this simple example, controlling for bilingual ability also works, but that will not translate so easily to the data.

- $d(1,1)$: cross-ethnic pair, high language match

When we run the regression of an outcome on a vector of ones and indicators for match type, the coefficients give the mean contrasts:

$$\hat{\beta}^H = \frac{1}{\sum_{i=1}^{n} \mathbb{1}_{(z_i,l_i)=(1,1)}} \left(\sum_{i=1}^{n} y_i \mathbb{1}_{(z_i,l_i)=(1,1)} \right) - \frac{1}{\sum_{i=1}^{n} \mathbb{1}_{z_i=0}} \left(\sum_{i=1}^{n} y_i \mathbb{1}_{z_i=0} \right)$$
$$\xrightarrow{p} \mathbb{E}[y(1,1)|(z_i,l_i)=(1,1)] - \mathbb{E}[y(0)]$$

with $\hat{\beta}^L$ defined analogously and $\hat{\gamma} \equiv \hat{\beta}^H - \hat{\beta}^L$. We have bias in $\hat{\beta}^H$ unless $\mathbb{E}[y(1,1)|(z_i,l_i)=(1,1)] = \mathbb{E}[y(1,1)]$. Selection bias depends on the correlation between language skills and the distribution of potential outcomes. While we do not observe potential outcomes $Y_i(1,1)$ and $Y_i(1,0)$ for the same student i, correlations between pre-determined covariates and match type suggest $\mathbb{E}[y(1,1)|(z_i,l_i)=(1,1)] \neq \mathbb{E}[y(1,1)]$. Going from the 25^{th} to 75^{th} quartile probability of being assigned to a high-match pair is associated with .52 additional named friends (11 percent increase) and .13 additional named cross-ethnic friends (27 percent increase). If this sociability makes the partner more likely to have a higher $Y_i(1,1)$ then our estimators are biased as we over-sample higher $Y_i(1,1)$ individuals when estimating the average outcome $\bar{Y}(1,1)$.

We can account for selection bias using propensity scores $\pi_i(z,l) = Pr_i(z,l)$. The key insight of Aronow and Samii (2017) is that the propensity score is *known* because we control the treatment assignment mechanism and know the language skill vectors. Simulations of the randomization procedure provide estimates of $\pi_i(z,l)$. We run the randomization code R times and for each randomization collect the assignment $d(z,l)$ for each individual. Then, $\pi_i(z,l) = \frac{\sum^{R} \mathbb{1}_{z_i=z,l_i=l}}{R}$.

Then, under the following assumptions, we can estimate the average causal effect of being randomly assigned a high-language match partner versus a low-language match partner

$$\gamma = \frac{1}{N} \left(\sum \mathbf{1}_{z_i=1,l_i=1} \frac{y_i(d(1,1))}{\pi_i(1,1)} - \sum \mathbf{1}_{z_i=1,l_i=0} \frac{y_i(d(1,0))}{\pi_i(1,0)} \right)$$

Assumption 1 Overlap: $0 < \pi_i(z,l) < 1$. In words, units with no possibility of being in a high or low language match must be dropped as the counterfactual is ill-defined. Roughly 14 percent of the current sample had 0 probability of being matched to a high match (46 students) or a low match (26 students). When we restrict the sample based on overlap, we estimate a LATE local to students who have some possibility of being in any given treatment state.

Assumption 2 Correct specification: The exposure mapping is accurate. The assumption says that $E[Y(1,1)]$ and $E[Y(1,0)]$ are estimands that capture the true causal relationship between communication potential and outcomes. Sävje (2024) argues this assumption is overly strong.[64]

Assumption 3 Selection on Observables: $Y_i(1,1), Y_i(1,0), Y_i(0) \perp D_i|X_i$. This assumption is met by random assignment and because the exposure mapping uses X_i to define the potential outcomes.

To increase estimation efficiency, we use causal forests to estimate γ with an augmented inverse propensity weighted estimator (AIPW). As discussed in Section 5, causal forests combine flexible estimators of the mean and propensity score (estimated via nuisance functions). Causal forests provide similar results to exposure mapping with some slight variation. Theoretically, the results should be similar because both methods adjust for differences in treatment propensity. When using causal forests, we estimate propensity scores ($\hat{\pi}(z,l)$) in first-stage random forests that use the propensity scores estimated from the randomization procedure and a rich set of other baseline covariates. (The procedure uses sample splitting or "honesty" to avoid overfitting.) This smooths the propensity scores away from zero, which may be desirable since idiosyncrasies in recruitment impact the propensity scores (see Appendix B.1). Causal forests produce consistent estimates when potential outcomes are independent of treatment assignment conditional on observables $Y_i(1,1), Y_i(1,0), Y_i(0) \perp D_i|X_i$ and there is overlap, which are subsets of the assumptions required for the exposure mapping argument.

[64] "Exposure mappings are primarily used to collect and describe sets of assignment vectors that share a similar interpretation with respect to the application at hand. It is rare that exposures that serve this purpose are also correctly specified in the sense that they provide a complete description of the causal structure. The results herein show that conventional point estimators perform well even if the exposures are misspecified, provided that the specification errors are only weakly dependent." (Sävje, 2024).

8 The Role of Language

Now we return to the key questions about whether and how language skills influence cohesion building. Section 6 showed language skill match is associated with better experiences and stronger cohesion outcomes. In this section, we consider two mechanisms. First, we focus on the communicative power of language and then consider the role of identity and language.

8.1 Language match and selection

Recall that "communication potential" is associated with better experiences and cohesion outcomes (see Figure 4). To what extent do these patterns reflect language match versus selection effects? The distinction is relevant for policy because they imply different solutions. If match effects drive the results, then a principal could increase cohesion through activities like this one by carefully pairing students with similar language skills. More broadly, an additional focus on teaching mutual languages might complement future integration efforts. On the other hand, selection effects would require further investigation. It could be some factor, such as openness to experience, that influences both individual language skills and cohesion measures, or it could be that learning languages enhances cohesion (e.g., through perspective-taking). In this case, the policy implications are less clear. Even more broadly, this distinction is useful for theorizing when and why integration efforts work to reduce biases and increase willingness to cooperate in the future.

We find evidence for both match and selection effects. Figure 7 shows the estimated average treatment effects of high match or low match compared to the co-ethnic control.[65] Language match has a clear impact on how students react to and experience working with a different ethnicity peer. Students report better experiences in high-match pairs than in low-match pairs (95% CI [-0.01, 0.43]) and communicate more (95% CI [0.237, 0.842]). The evidence for match effects is weaker for cohesion measures (95% CI [-0.0629, 0.289]).

Knowing the propensity score allows us to gain valuable insights about the underlying effects. The probability of being assigned a "high language match" is known for every individual and indexes types; *i.e.* students with

[65] Appendix Figure A3 shows estimation using inverse propensity weights and bootstrapped standard errors. The procedure uses the same exposure mapping weights for each iteration. The results are similar to those obtained with the causal forest procedure, though noisier.

a 40 percent probability of being assigned a high match are likely to be similar in expectation. If we had much more data, we would be able to estimate marginal treatment effects non-parametrically. Given limited power, we estimate regressions of outcome indices against the probability of being assigned to a "high language match" ($\pi(1,1)$). Each plot in Figure 8 has a separate line for $Y(0)$, $Y(1,0)$, and $Y(1,1)$ allowing us to compare how potential outcomes vary over the distribution of types (at least in terms of the line of best fit). Recall that we normalize outcomes based on the control group distribution, so $Y(0)$ (solid red lines) is centered around 0 by construction.

Let us examine the plots in Figure 8 by first comparing levels and then slopes. In theory, the differences in levels integrate to the average treatment effects (shown in Figure 7).[66] For example, the difference between Y(1,1) (low match, blue dashed lines) and Y(0) (control, red solid lines) in the communication index varies between -1.5 and -0.6 which averages to a bit over -1, which is roughly the estimated ATE for high match pairs using the communication index.[67] The gaps between high match and low match lines show potential language match effects are consistently present throughout the distribution.

The slopes of the lines help clarify the selection effects. Steep upward sloping lines reflect positive selection, i.e. potential outcomes are increasing in the propensity score. Flat lines suggest constant effects with respect to the propensity score. Notice that the potential outcomes for co-ethnic pairs (i.e. $Y(0)$ or red lines) tend to be flatter. This pattern suggests that being a higher type is not associated with better outcomes i.e. outcomes for students in same ethnicity pairs bares little relationship to the probability of being in a high communication potential match. The cross-ethnic pairs tend to have positive slopes so that high propensity types have better treatment effects. This pattern suggests "selection on gains". These patterns informed our interpretation of the results in Figure 7 described above.

The results suggest room for "benign self-selection" into inter-ethnic contact in the real world. Students with stronger propensity to a high language match, have higher non-pecuniary benefits and cohesion gains and so may be more likely to join an integration activity. With more high propensity types participating, the probability of a high match increases further and so during opt-in based integration activities, we likely see high matches among high propensity types. This evidence predicts that in smaller studies, where individuals select into participation, the estimated effects of contact will appear particularly high, and that is what meta

[66] The integration is with respect to the distribution of propensity scores, which is relatively flat (see Figure A7).
[67] See the estimated "High match" effect for the Communication Index in Figure 7 (blue circle).

analyses show (Paluck et al., 2019).[68]

To understand underlying heterogeneity, we turn to the personalized estimates of treatment effects β_i^H and β_i^L provided by causal forests. For cohesion measures, we care about sample characteristics such as the range and averages of the personalized treatment effects.[69] The range of treatment effects for a low match is between -0.01 and .26 standard deviations, while for a high match, the estimates range from .099 to .42. This is nice to see because it implies there are very few students for whom treatment "backfires".

Finally, the correlation between propensity scores and baseline covariates can shed light on what underlying heterogeneity matters in this context (see Tables 8 and 9). High propensity types are older, more connected to Gostivar, more likely to say the plan to go to college, and, of course, have higher baseline language skills.[70] At baseline, they also report higher sense of closeness to out groups in the Inclusion of Other in Self measure. Nevertheless, they are no more likely to have out-group friends and other behavioral measures, such as dictator game bias do not have a significant linear relationship with the propensity score (see Table 9).

8.2 Does the language used matter?

Next, we turn to mechanisms, focusing on hypotheses driven by theory and context. Contact theory predicts that when individuals are on equal footing, cohesion outcomes improve more (Allport et al., 1954) (to the best of our knowledge, this has not been formally tested). Further, language skill differences could upset this balance, creating a power dynamic that may cause backlash (Fouka, 2020). This is particularly concerning in this setting, given the fraught history of Macedonian as a second language (see Section 2).

We address these issues with three sets of tests, which take the form of defining match-types based on the

[68] Alternatively, in smaller studies, the effort made by implementers might be particularly high.

[69] The personalized treatment effects are quite noisy individually because the data is small and the signal is not precise. Using sample splitting, we can learn about the quality of our estimates. The idea is that we hold out a portion of the sample when fitting the model and then can use predictions on the hold out sample to test the model quality. The forests do a good job estimating the average treatment effect, even though individual heterogeneity measures are not as good out of sample.

[70] Tables 8 and 9 show the correlations between with the overlap sample which is defined based on the values for $\pi(1,1)$ where we see overlap in the data (between .1 and .9). For this sample, we report characteristics for pairs whose probability of a high language match approaches zero (Intercept), and for pairs whose probability of a high language match approaches 1 (Slope). In the overlap sample, the high types are more likely to be fluent in English, to have attended a middle school in Gostivar (a proxy for being from the town rather than villages or elsewhere), to plan to go to college and to be in the 3 year of high school. (While noisy, the linear relationship suggest high propensity types are more likely to be female, less likely to be Turkish, and better at a local non-mother tongue language.)

individual's relationship to the language that they and their partner have the highest communication potential in. There are three relationships within a pair. For each of relationship, we estimate $\beta^{Type} = E[Y(Type) - Y(0)]$ using the AIPW estimator from the causal forest.[71] First, a participant can be randomly assigned a partner with whom the highest communication potential is their *home language*. These effects are captured with $\beta^{Home} = E[Y(Home) - Y(0)]$. Second, the participant can be assigned a partner with whom their highest communication potential is with their partner's home language; we call these *second language*. These effects are captured with $\beta^{Second} = E[Y(Second) - Y(0)]$. Finally, there are pairs who match on a mutual second language *lingua franca*. The use of "communication potential" rather than actual language used gives these estimates an intent-to-treat interpretation. These effects are captured with $\beta^{LinguaFranca} = E[Y(LinguaFranca) - Y(0)]$.

Home language effect: Does using the home language lead to lower ingroup bias and willingness to pay for a co-ethnic partner relative to using other languages? The affirmative to this question is addressed with the hypothesis:

$$\textbf{H1: } \beta^{Home} > \beta^{Second} \text{ and } \beta^{Home} > \beta^{LinguaFranca}$$

There is evidence for a home language effect—-students who speak their own home language have higher cohesion outcomes ($\beta^{Home} - \beta^{Second}$: 95% CI [-0.000128, 0.447]; $\beta^{Home} - \beta^{LinguaFranca}$: 95% CI [0.163, 0.614]).[72] They also report better communication and experiences and rate their partners as better at the game. Drilling down into sub-measures, the cohesion differences come from dictator game sharing. Home-language speakers' dictator bias is 0.5 standard deviations below the co-ethnic-pair mean. In real terms, the bias falls from 23 Denar for control students to 1.5 Denar for home-language speakers. These results concord with recent psychology literature that show positive mother tongue effects when making cross-ethnic peace proposals (Grant et al., 2023).

Is there backlash?: To test for backlash, we test the hypothesis that

$$\textbf{H2: } \beta^{SecondLanguage} < 0$$

[71] More formally, we estimate $\beta_i^{Type}(X_i)$ conditional on baseline covariates and propensity scores derived from the randomization procedure and then aggregate the personalized estimates to an average ITT effect.

[72] These are two-sided t-tests.

.

While second-language speakers have significantly lower outcomes than home-language speakers, the evidence for "backlash" is slim. These students still have positive cohesion outcomes on average, though the two-sided confidence interval covers 0 (β^{Second}: 95% CI [-0.0352, 0.374]).

English and Macedonian as lingua franca: If equal footing matters, then using a *lingua franca*—a shared second language—should yield desirable outcomes. This yields the hypotheses:

$$\textbf{H3: } \beta^{LinguaFrance} > 0$$

.

$$\textbf{H3 Strong: } \beta^{LinguaFrance} - \frac{\beta^{Home} + \beta^{Second}}{2} > 0$$

H3 says that it is beneficial to use a *lingua franca* for contact and H3 Strong says it is better to use a shared second language than some student's home language. We fail to reject the null in both cases.[73] Pairs who speak a *lingua franca* have similar communication potential, on average, to those who speak a second language with a home language speaker. These results suggests there is no specific gain from students having equal status in terms of the language used.

Macedonian or English as the language used: The previous results pooled English and Macedonian as match languages. Discussions with teachers and school administrators suggested that English was preferred by students. Moreover, the history of language policy in schools suggests there might be a distaste for using Macedonian. To test this, we focus on Albanian and Turkish students who match with partners with their highest communication potential in either English or Macedonian. We define treatment effects where the potential outcomes are now indexed by the language of the language with the top communication potential.[74] and test the hypothesis

$$\textbf{H4: } \beta^{English} > \beta^{Macedonian}$$

[73] For the cohesion index: H3: 95% CI [-.209, .258], H3 Strong: [-.434, .009]

[74] E.g. $\beta^{English} = E[Y(English) - Y(0)]$

This hypothesis is motivated by recent history that suggests that English would be preferred for contact. Figure 10 shows that we cannot reject the null hypothesis. Differences in communication, experience, and cohesion results are indistinguishable from 0.[75] Students using English with a partner have productivity gains. Directionally, teachers' predictions that English would outperform Macedonian holds for communication and experience indices. However, if anything, students have better cohesion results when using Macedonian. These noisy differences come from the dictator games, where the average bias is -0.203 (se = 0.228) standard deviation units lower for students paired with a Macedonian-speaking top match than with an English-speaking one.

In summary, the language used matters, but not in the manner suggested by theory and contextual history. We find evidence that using local languages—particularly one's home language—leads to stronger cohesion outcomes. Since these results are driven by in-group/out-group bias measured in dictator games rather than through the demand for segregation, we theorize this reflects an identity channel. Hearing your home language or even a local language appears to be associated with lower bias in dictator games.

9 Policy Experiments

Finally, we imagine a school implementing paired video gameplay as a tool to build cohesion across linguistic groups.[76] We estimate the range of potential average treatment effects under the possible matching of partners. We draw on matching algorithms from network theory to find the optimal pairings.[77] To sketch the idea, we create an edge between each cross-ethnic pair within a strata; then, we calculate the "communication potential", depending on the match type, we assign a weight to the edge that equals the sum of the treatment effect of the two students. Formally, for edge (i, j), if the linguistic match is above the threshold, we assign edge (i, j) weight $w_{ij} = \beta_i^H + \beta_j^H$.[78] If the two students have a low match, we assign them the sum of their respective low-match treatment effects $w_{ij} = \beta_i^L + \beta_j^L$. Then, we find the optimal matching which maximizes the effects, thereby maximizing the cohesion gains using Edmond's blossom algorithm (Edmonds, 1965).[79]

[75] We focus on the overlap sample with some probability of being assigned a top match in English or Macedonian.

[76] Video game events occur in the high schools, though they do not seem to be focused on integration, *per se*.

[77] Specifically we use Edmond's Blossom algorithm for weighted graphs which is implemented in the Python project *networkx* (**networkx**; Edmonds, 1965).

[78] These treatment effect estimates come from the causal forests

[79] In Appendix E, we describe the process in greater detail.

Since we run the algorithm within strata, the language skills of the students within the strata constrain the feasible matches (i.e. not everyone will get a high match in the optimal matching). We compare the optimal matching to the expected cohesion gains of random matching, and the infeasible scenario where every pair has a high communication potential or every pair has a low communication potential.

Table 7 shows the range of treatment effects under various scenarios. Compared to the randomized benchmark, optimal matching gives a 10 percent boost to cohesion gains (and nearly 50 percent better than the worst-case feasible match). These are small gains – on average, in levels for WTP it's roughly 2.1 denar or an extra 0.8 denar decline in ingroup bias in the dictator game). Nevertheless, this shows that educators with an eye toward integration could improve the impact of local cohesion-building initiatives by attending to student skill sets. While we use unobserved treatment effect heterogeneity here, the number of high language matches is the main difference between the best and worst matching. Within a cohort, the optimal pairing could be approximated by hand. On the other hand, the optimal feasible matching only provides one-third of the cohesion gains of the scenario where every student has a high match partner. This points to an additional benefit to longer-term investments in skills, an idea we return to in the conclusion.

10 Conclusion

We investigate the role of language and identity in inter-ethnic contact using a field-based experiment where students are randomly assigned a partner of the same or a different ethnicity. In line with previous work, we find a moderate positive impact on cohesion outcomes after inter-ethnic contact that come with non-pecuniary costs but with no productivity loss on average. The aggregate effects mask heterogeneity: inter-group contact has better cohesion outcomes and lower costs for students assigned partners with whom they have high communication potential. These correlations are relevant in a world of endogenous friendship formation and "benign self-selection". Supposing cross-ethnic pairs meet with some probability and determine whether to match based on expected utility, then the relevant estimate is the unadjusted estimates for high-match pairs in the full sample.

We then consider the role of language as a communication tool and conveyor of identities. We cannot rule out match effects based on communication potential or that heterogeneity reflects "selection"–individuals

with a higher probability of a high communication potential match are more receptive to inter-group contact ("selection on gains"). Further, we find the strongest cohesion and experience outcomes are associated with having the highest communication potential in one's home language in a mixed-ethnicity pair.

Nevertheless, after inter-group contact, the average willingness to pay for segregation is still 165 denar, nearly the expected payout from participating in the game. Under strong assumptions, the counterfactual exercise suggests that improving language skills would lead to 30 percent larger cohesion gains, but in practice, the average willingness to pay would still be over 155 denars. This analysis highlights that cohesion cannot be easily built from short one-off experiences. Without repeated exposure, preferences for homophily are too strong.

The results hint at a complementary link between language policy and cohesion, suggesting that policies promoting home language (L1) instruction should be paired with additional rigorous language training and, importantly, opportunities for inter-ethnic contact. The causal arrow likely goes in both directions. Recent literature highlights the linguistic impact of integration. Rao (2019) find wealthier students in India substitute away from English when integrated with poorer students who tend to exclusively use Hindi at home. Alan et al. (2021) and Boucher et al. (2024) find Turkish skills increase for Syrian refugees after participating in the perspective-taking course or pre-school camp. Moreover, in Iraq, Christian soccer players start speaking Arabic more when Muslim soccer players are added to their team (Mousa, 2020). We provide a point-in-time snapshot of how schools might create cohesive communities, but language and integration may form a positive feedback loop whereby repeated cooperative inter-group contact becomes self-reinforcing.

Shared language alone does not create cohesive communities. Early proponents of nationalism presupposed that language was necessary and perhaps sufficient to form a cohesive state (Anderson, 1983).[80] During the negotiations to end World War I, Woodrow Wilson and his academic advisors advocated for the creation of the first Yugoslavia writing that: "the political and linguistic affiliations of the peoples whom it is proposed to unite in a single state are so well known that a detailed argument is unnecessary." (David Hunter Miller, 1924) Nevertheless, these same "peoples" took arms against one another during World War II and again at the dissolution of the second Yugoslavia in the 1990s. Shared language is not a cure-all. Rather, shared language reduces the costs of integration–the benefits still must be high enough for integration to be worthwhile. This

[80] See Chapter 5.

insight may be useful in various settings where members of various language groups coexist, which is an increasingly common feature in communities around the world.

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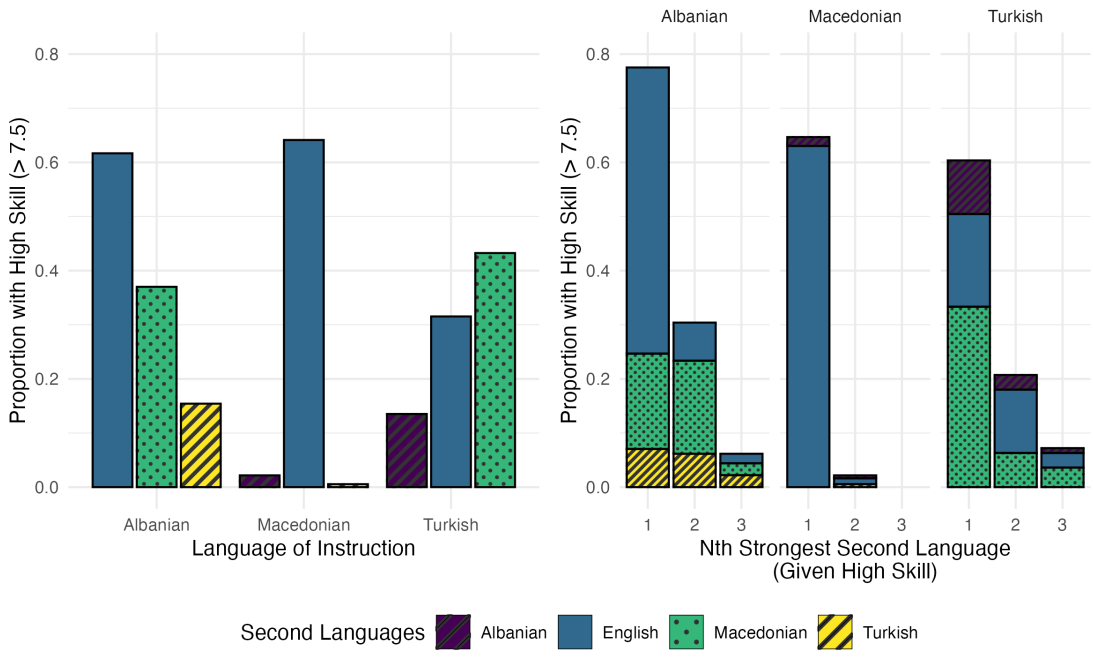
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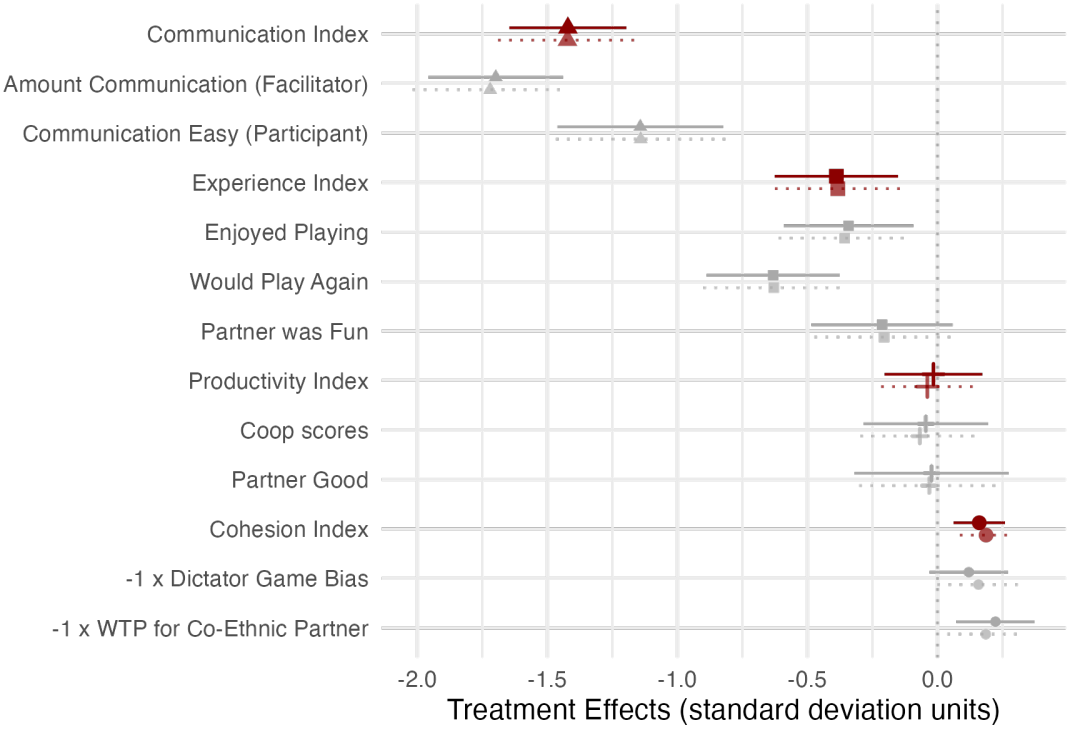
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Figure 1: Second Language Skills by Language of Instruction



Notes: On the vertical axis both figures show the proportion of students within a language of instruction group who meet some criteria. The left plot shows self-reported language proficiency for the three local languages and English. Proficiency is defined as self-reporting an average of 7.5 fluency on a scale of 0 to 10 averaged over the four language domains: reading, writing, speaking, and listening. The left plot shows how those languages rank within student skill sets. The sample consists of students who completed the baseline survey and participated in the video game session. The sample is 44 percent Albanian, 35 percent Macedonian and 21 percent Turkish.

Figure 2: Treatment Effects: Assigned Cross-Ethnic Partner

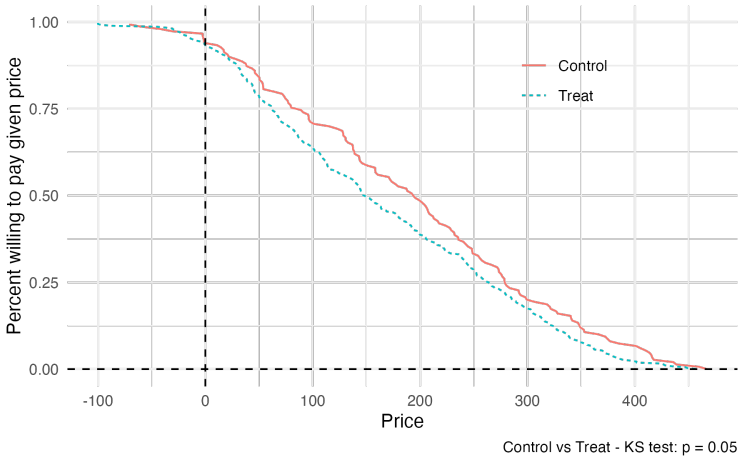


Notes: Average treatment effects of having a different ethnicity partner compared to having a same ethnicity partner. Calculated with causal forests with covariates listed in paper (solid lines) and with regression with no covariates (dotted lines). All sub-outcomes (grey) are normalized around the control mean and control standard deviation. Indices (red) are simple averages of normalized sub-outcomes. Cohesion measures are multiplied by negative one so that positive numbers reflect gains and negative numbers loses. Standard errors clustered by strata. The sample consists of all students who participated in the video game sessions. For some sub-measures, students do not answer the questions and so the sample is restricted.

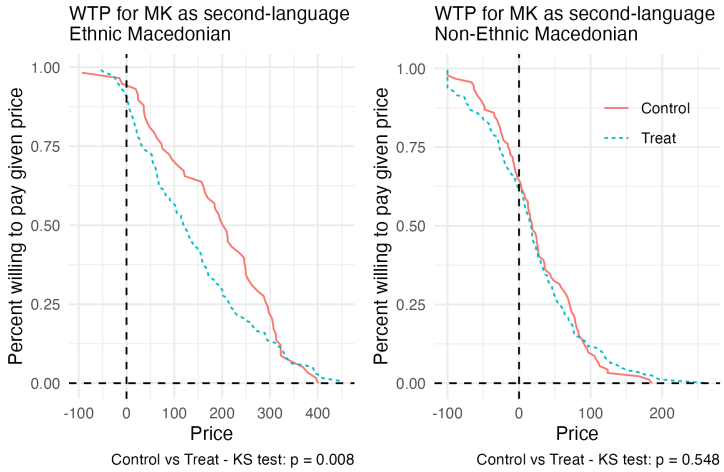
Outcomes: Amount communication reported by two facilitators on 7-point Likert scale. Communication Easy, Enjoyed Playing, Would Play Again, Partner Fun, and Partner Good reported by students on post-play survey on 5-point Likert scale. Cooperative video game scores come from the average scores of partners in the incentivized rounds of play of "Overcooked! 2". The mean solo scores normalized is the average of 3 solo rounds. The quiz score was derived from a short quiz on the video game rules and controls. Dictator game bias is the average difference in money shared in dictator games with anonymous representative of ingroup compared amount shared with anonymous representative of outgroup. WTP for Co-Ethnic Partner comes from the adaptive choice experiment.

Figure 3: Demand for partner characteristics

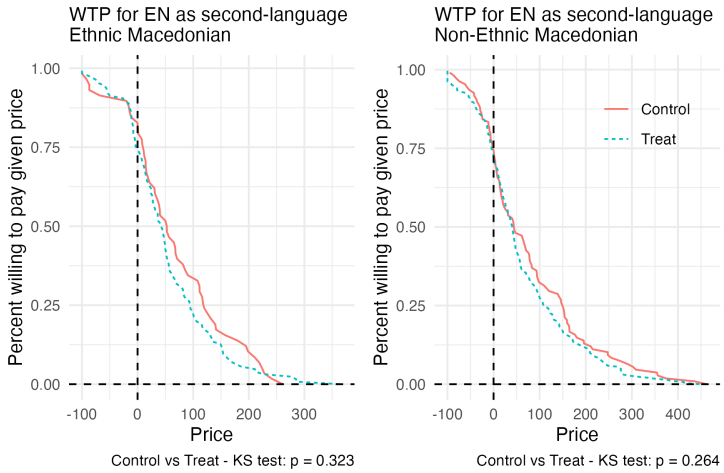
(a) Demand Curve: Co-ethnic Partner



(b) Demand Curve: Macedonian as a Second-language

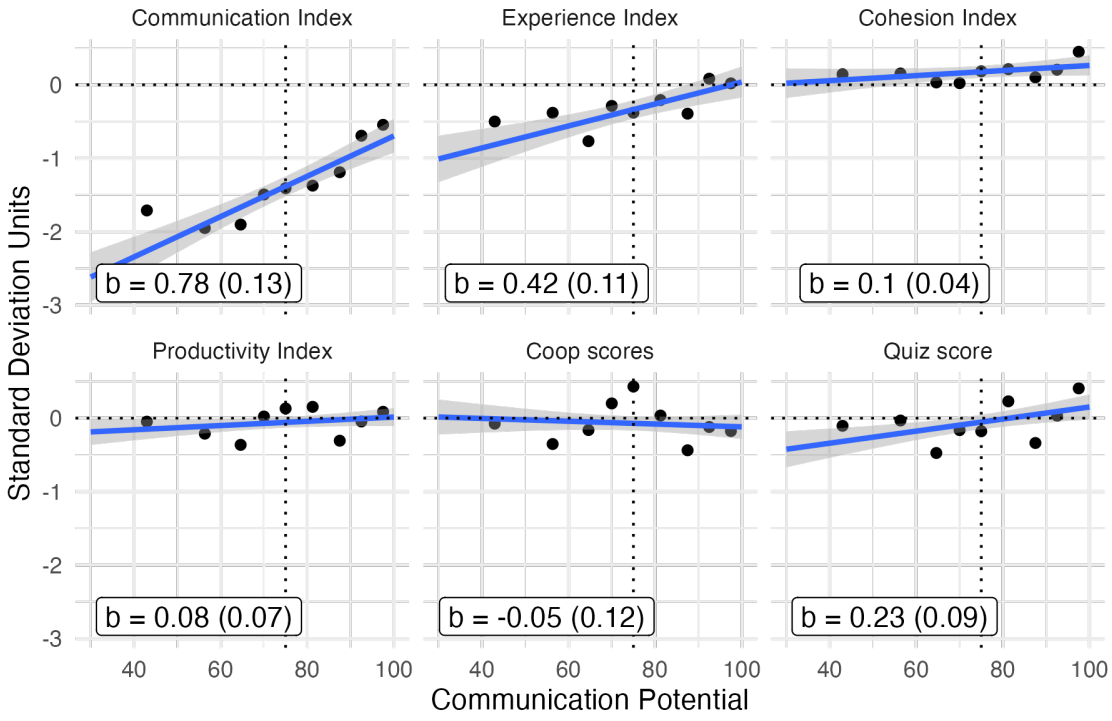


(c) Demand Curve: English as a Second-language



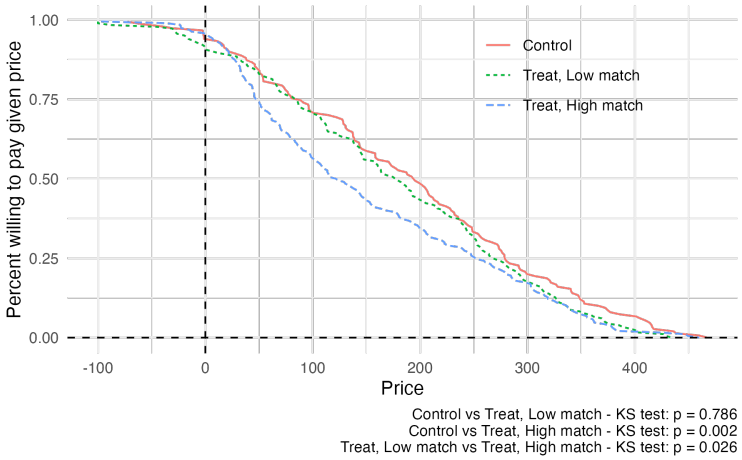
Notes: Demand curves are the cumulative distribution of individual-level willingness to pay estimates. Willingness to pay estimated from adaptive choice experiments in which respondents made binary choices between partners based on ethnicity, second language skills and a sure payout.

Figure 4: Associations Between Communication Potential and Outcomes



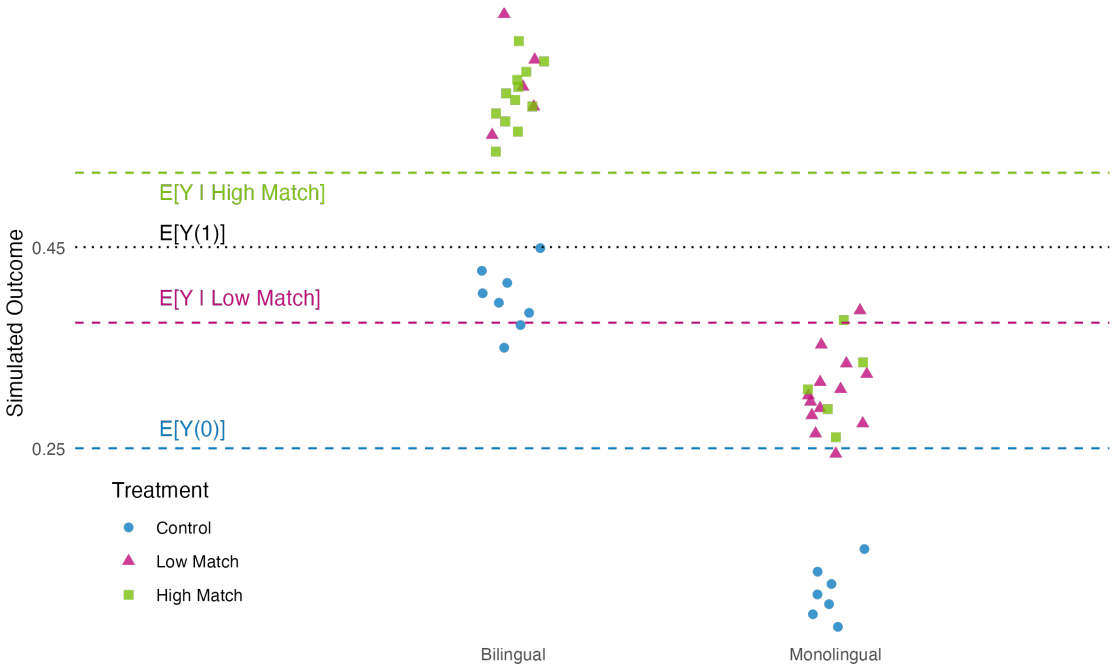
Notes: Lines show linear regression of indices against communication potential. Communication potential is calculated using student's self-reported fluency scores in Albanian, Macedonian, Turkish, and English. Within a pair, the communication potential is the largest geometric mean (squared) of fluency scores (i.e. the maximum of the individual's scores multiplied by each other component-wise).
The regression $\text{Outcome}_i = a + b\frac{\text{Communication Potential}_{ij}}{IQR} + \epsilon_i$. Where IQR is the interquartile range of communication potential. The estimated b shows the average change observed in an outcome when moving from the 25th to 75th percentile of communication potential (from roughly 60 to 90). While partners are random, $\text{Communication Potential}_{ij}$ is a function of endogenous skills; hence these results should be interpreted as associations. b are significantly different from zero for the Cohesion, Communication, Enjoyment Indices, but not the Productivity index. Cooperative video game scores and quiz scores are the components of the productivity index. Points show averages binned by dividing communication potential at quantiles. Standard errors clustered by strata.

Figure 5: Demand Curve: Co-ethnic Partner, by Communication Potential



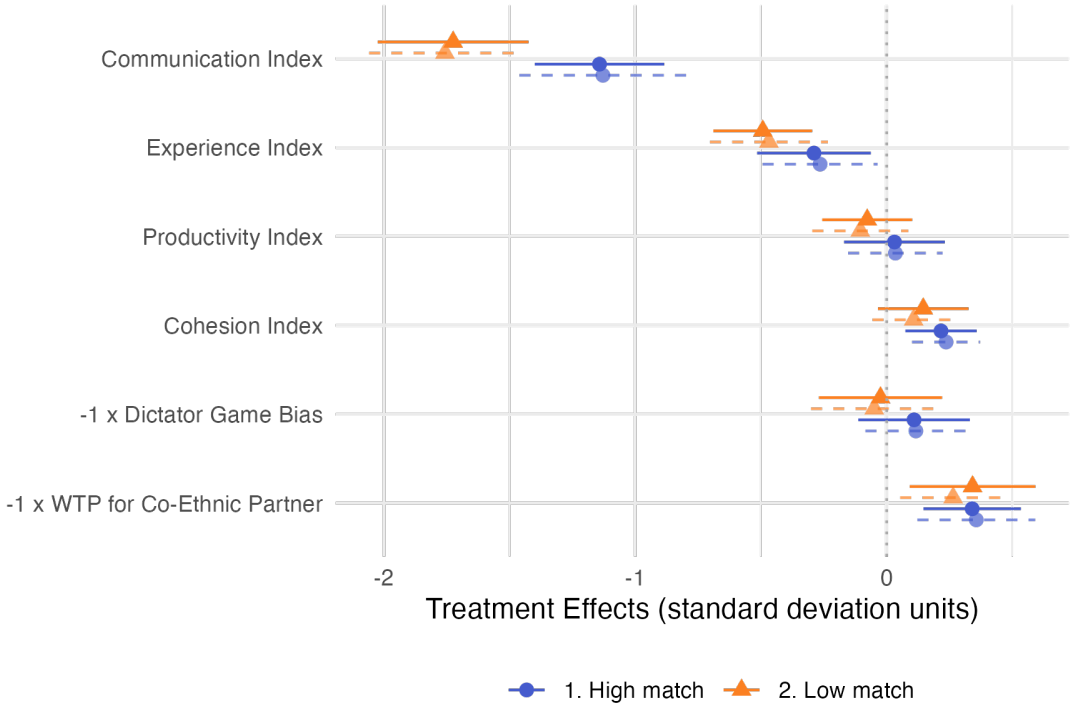
Notes: Demand curves are the cumulative distribution of individual-level willingness to pay estimates. Willingness to pay is estimated from adaptive choice experiments in which respondents made binary choices between partners based on ethnicity, second language skills, and a sure payout. A high match is defined as having communication potential above the median communication potential (for treated units). Low matches are below the median.

Figure 6: Simulation Demonstrating Selection Bias when Sample Splitting



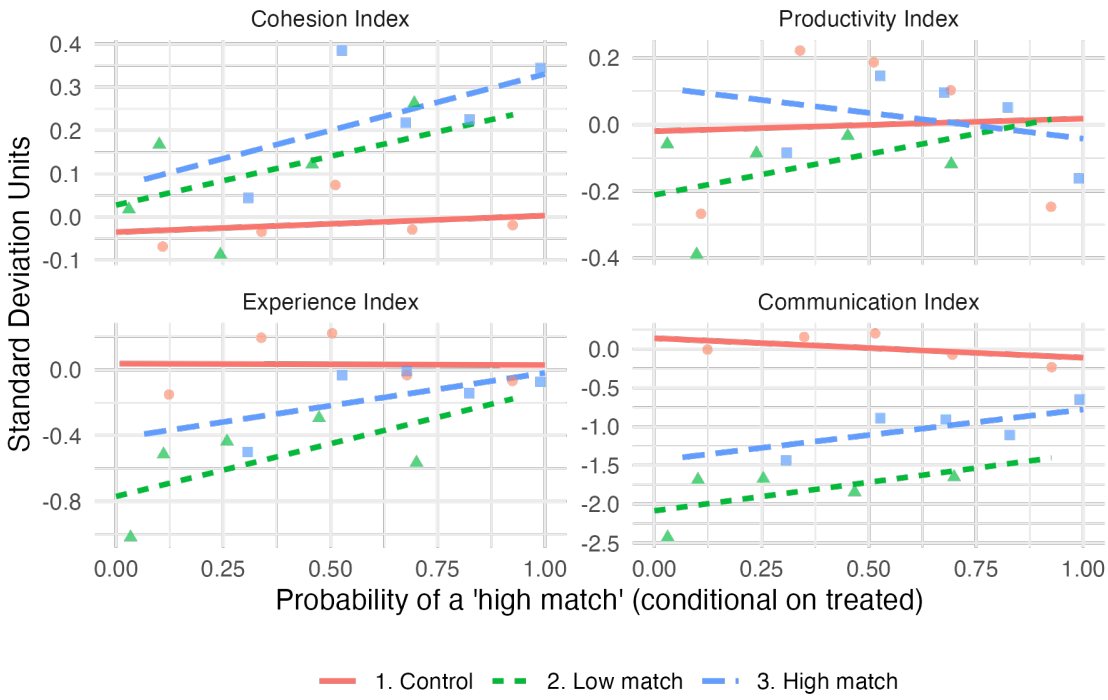
Notes: This stylized simulation shows a stratum with two types of students–bilingual or monolingual–in a trilingual environment. There is a constant treatment effect $\tau = .2$. Each point represents roughly 2000 students randomized into control, high match, or low match as shown with shapes and colors. The x-axis divides students by type and the y-axis shows the realized outcome. A regression run on the whole sample returns $\hat{\tau} = .2$, but the regression $y_i = \alpha + \beta^L \mathbf{1}_{lowmatch} + \beta^H \mathbf{1}_{highmatch} + \epsilon$ returns $\beta^H = \mathbb{E}[Y(1)|highmatch] - E[Y(0)] > \tau > \mathbb{E}[Y(1)|lowmatch] - E[Y(0)] = \beta^L$. Using inverse propensity weighting where $p = \mathbb{E}[highmatch|type]$ addresses the bias.

Figure 7: Heterogeneity by Communication Potential of Cross-Ethnic Pairs



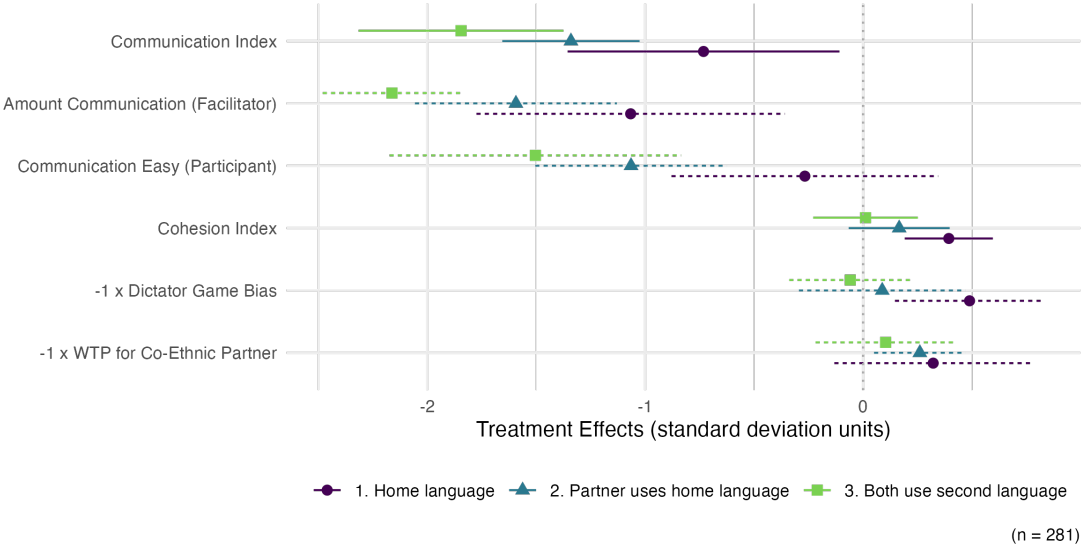
Notes: Estimates of average treatment effects of having a cross-ethnic partner with high or low language match compared to having a co-ethnic partner. Estimated with causal forests with covariates listed in the paper (bold color, solid lines) and with regression with indicators for match type and no covariates (light color, dashed lines). The sample is restricted to ensure overlap, such that students in the sub-sample have a non-zero probability of being in a high or low match. We define a high match as at or above the median "communication potential" score (75), and a low match as below the median. All sub-outcomes are normalized around the control mean and control standard deviation. Indices are simple averages of normalized sub-outcomes.

Figure 8: Relationship Between Propensity to Have a High Match and Outcomes



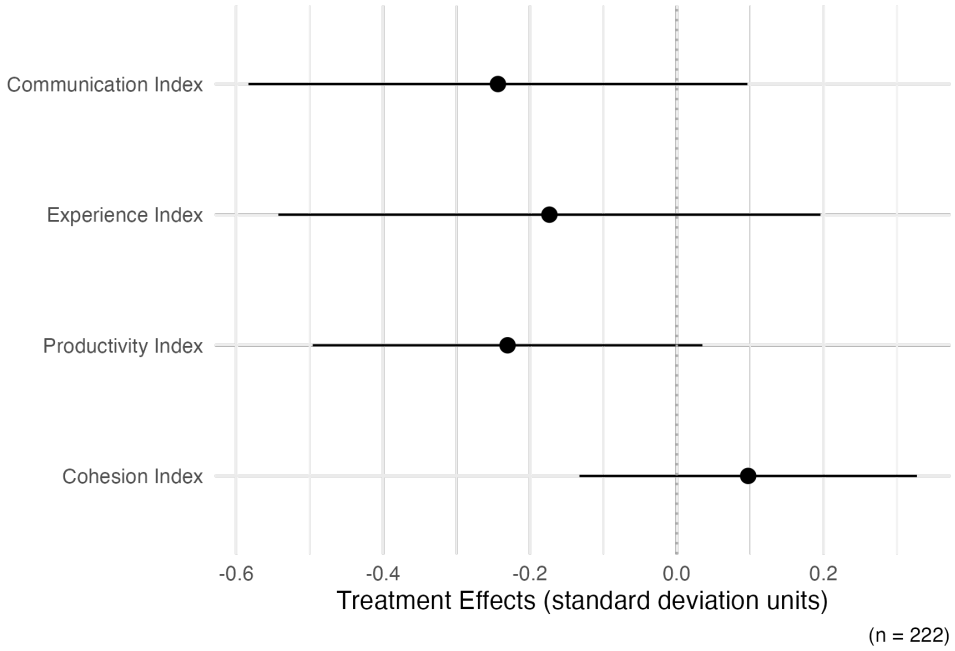
Notes: Lines show linear regression of indices against the probability of getting a high match partner $\pi(1,1)$. The regression Index$_i = \beta_{0,z,l} + \beta_{0,z,l}\pi_i(1,1) + \epsilon_i$ with ($\beta_{0,z,l}$, $\beta_{1,z,l}$) specific to treatment assignment (z,l). Each line shows the best linear projection of potential outcomes $Y(z,l)$ so that steep slopes imply selection and gaps between high match and low match capture match effects.

Figure 9: Home and Second Language Effects



Notes The plot shows treatment effects compared to the control group for students whose 1) top "communication potential" is the student's home language, 2) top "communication potential" is the student's partner's home language, or 3) top "communication potential" is both students' second language (*lingua franca*). Estimated with causal forests with covariates listed in the paper. Points with solid lines are indices and dotted lines are submeasures. The sample is not restricted. See Appendix figure A4b for results with an overlap sample and for the productivity and experience outcomes.

Figure 10: Macedonian versus English as Second Language



Notes The plot shows Albanian and Turkish students' treatment effects when the "communication potential" is in Macedonian compared to English. Thus, negative effects imply matching in Macedonian language results in worse outcomes than English and positive effects imply matching in Macedonian language results in better outcomes. Estimated with causal forests with covariates listed in paper. The sample is restricted so all observations have a positive probability of matching on either English or Macedonian.

11 Tables

Table 1: Demographics: Sample characteristics and balance

Covariate	Sample Mean	(sd)	Control Mean	(se).	Treat Diff	(se)	p-value	N
Female	0.56	0.50	0.57	0.11	-0.01	0.06	0.81	522
Grade 1	0.37	0.48	0.31	0.10	0.08	0.05	0.11	522
Grade 2	0.28	0.45	0.31	0.10	-0.05	0.06	0.38	522
Grade 3	0.18	0.39	0.21	0.08	-0.04	0.05	0.43	522
Grade 4	0.16	0.37	0.16	0.08	0.01	0.03	0.81	522
Albanian LOI	0.43	0.50	0.47	0.04	-0.04	0.04	0.32	522
Macedonian LOI	0.35	0.48	0.39	0.05	-0.05	0.04	0.28	522
Turkish LOI	0.21	0.41	0.15	0.04	0.09	0.03	0.01	522
LOI maps to ethnicity	0.97	0.18	0.95	0.02	0.02	0.02	0.33	521
English fluency	7.49	2.26	7.57	0.17	-0.11	0.18	0.55	522
Local language fluency (not LOI)	4.88	3.60	4.73	0.42	0.21	0.37	0.57	522
Attended Gostivar middle school	0.70	0.46	0.70	0.04	0.01	0.04	0.88	521
Has Cellphone	0.97	0.17	0.96	0.01	0.02	0.01	0.30	521
Parent has college	0.44	0.50	0.39	0.04	0.08	0.05	0.10	521
Plans to go to college	0.89	0.31	0.87	0.03	0.02	0.03	0.47	521
Life satisfaction (0-10)	8.45	2.01	8.25	0.26	0.28	0.20	0.18	521
Trust (0-10)	6.71	2.51	6.85	0.26	-0.21	0.25	0.42	521

Notes: Table shows full sample characteristics (Sample Mean), characteristics for pairs who are assigned a same ethnicity partner and have completed the video game session (Control Mean), and the mean differences between control students and students assigned different ethnicity partners and who have completed the video game (Treat Diff) along with associated p-values from a t-test of mean differences where standard errors are clustered by school-grade-gender-date of randomization.
Covariate details LOI is the language of instruction. LOI maps to ethnicity equals 1 if the student self-identifies with the ethnicity associated with their language of instruction (the measure excludes students with more than one local ethnicity). Self-reported English fluency and Local language fluency for non-LOI are on a scale from 0-10. Language fluency is imputed with a random forest regression for 1 student missing baseline data using information from the recruitment data.

Table 2: Baseline cohesion and video game proclivities: Sample characteristics and balance

Covariate	Sample Mean	(sd)	Control Mean	(se).	Treat Diff	(se)	p-value	N
N peers in named for network	5.12	3.07	5.23	0.36	-0.15	0.26	0.58	521
N out-group links named	0.64	0.97	0.54	0.06	0.14	0.06	0.02	521
Closeness to own LOI (baseline)	6.42	1.28	6.43	0.13	-0.01	0.14	0.96	521
Closeness to out LOI (baseline)	2.03	1.12	2.00	0.07	0.05	0.07	0.51	521
Dictator game denar to outgroup (baseline)	224.71	130.81	236.33	13.83	-16.32	8.89	0.08	521
Dictator game bias (baseline)	27.50	80.42	30.50	10.19	-4.22	9.67	0.67	521
Enjoys video games	0.72	0.45	0.72	0.05	-0.01	0.05	0.90	521
Makes video game rec	0.62	0.49	0.67	0.06	-0.07	0.04	0.07	521
Solo score	477.40	220.41	452.43	33.01	35.04	17.54	0.05	522
Solo score (demeaned)	0.34	211.79	-23.40	30.41	33.31	19.46	0.10	522

Notes: Table shows full sample characteristics (Sample Mean), characteristics for pairs who are assigned a same ethnicity partner and have completed the video game session (Control Mean), and the mean differences between control students and students assigned different ethnicity partners and who have completed the video game (Treat Diff) along with associated p-values from a t-test of mean differences where standard errors are clustered by school-grade-gender-date of randomization.

Covariate details Network data comes from 4 questions where students name other students as friends or sources of information and academic support. One question specifically asks for cross-ethnic links. Each question has 6 slots. 148 students (28 percent) use all the slots for friend cliques. Closeness measures come from the Inclusion of Other-in-Self measures (scale from 1-7). Dictator game bias is the difference between the amount of money an individual shares with anonymous ingroup (same ethnicity) recipients compared to what is shared with anonymous outgroup recipients. Enjoys video games equals 1 if the student responded to the question of "How well does 'I love video games' describe you" with 5 or greater on a scale from 0 to 10. Makes video game rec equals 1 if the student names at least one video game when asked for a recommendation, and zero otherwise. Solo score is the average score in solo rounds of "Overcooked! 2". Solo score demeaned demeans the score by gender-specific solo score means.

Table 3: Communication and experience treatment effects: Assigned cross-ethnic partner

Dependent Variables:	Communication Index		Amount Communication (Facilitator)		Communication Easy (Participant)	
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Constant	0.0075		5.96×10^{-15}		-4.33×10^{-15}	
	(0.0732)		(0.0984)		(0.0906)	
Cross-ethnic partner	-1.423***	-1.394***	-1.720***	-1.677***	-1.141***	-1.120***
	(0.1363)	(0.1591)	(0.1520)	(0.1750)	(0.1663)	(0.1853)
Fixed-effects						
school-grade-gender		Yes		Yes		Yes
Fit statistics						
Observations	505	505	524	524	505	505
R^2	0.21055	0.30706	0.24012	0.34495	0.09297	0.16826
Within R^2		0.20584		0.23730		0.08733

Clustered (strata) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent Variables:	Experience Index		Enjoyed Playing		Would Play Again		Partner was Fun	
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Constant	0.0328		-2×10^{-15}		-2.18×10^{-15}		-1.37×10^{-15}	
	(0.0876)		(0.0865)		(0.0954)		(0.1084)	
Cross-ethnic partner	-0.3833***	-0.3575***	-0.3567***	-0.3294**	-0.6296***	-0.5972***	-0.2055	-0.2126
	(0.1235)	(0.1299)	(0.1299)	(0.1377)	(0.1385)	(0.1495)	(0.1369)	(0.1491)
Fixed-effects								
school-grade-gender		Yes		Yes		Yes		Yes
Fit statistics								
Observations	492	492	512	512	504	504	508	508
R^2	0.02339	0.12662	0.01319	0.09745	0.03408	0.10960	0.00832	0.11192
Within R^2		0.02011		0.01098		0.02966		0.00889

Clustered (strata) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Indices are simple averages of normalized sub-outcomes. All sub-outcomes are normalized around the control mean and control standard deviation. Communication easy and experience sub-outcomes derived from participant survey items on a 5-point Likert scale. Amount communication is derived from the average of two facilitator responses on a 7-point Likert scale. Standard errors clustered by strata.

Table 4: Productivity and cohesion treatment effects: Assigned cross-ethnic partner

Dependent Variables:	Productivity Index			Coop scores			Quiz score		
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Variables									
Constant	-0.0006			-1.66×10^{-16}			1.43×10^{-15}		
	(0.1041)			(0.1431)			(0.1071)		
Cross-ethnic partner	-0.0616	-0.0284	-0.0603	-0.0679	-0.0301	-0.0664	-0.0666	-0.0460	-0.0731
	(0.0717)	(0.0872)	(0.0791)	(0.1170)	(0.1221)	(0.1134)	(0.0790)	(0.1044)	(0.1008)
Mean solo score (Normalized)			0.2847***			0.3248***			0.2426***
			(0.0333)			(0.0412)			(0.0502)
Fixed-effects									
school-grade-gender		Yes	Yes		Yes	Yes		Yes	Yes
Fit statistics									
Observations	522	522	522	524	524	524	522	522	522
R^2	0.00144	0.24853	0.34121	0.00104	0.33635	0.40713	0.00094	0.12225	0.15966
Within R^2		0.00037	0.12366		0.00027	0.10690		0.00045	0.04306

Clustered (strata) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent Variables:	Cohesion Index			-1 x Dictator Game Bias			-1 x WTP for Co-Ethnic Partner		
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Variables									
Constant	-0.0152			-1.23×10^{-16}			-0.0305		
	(0.0492)			(0.0764)			(0.0768)		
Cross-ethnic partner	0.1868***	0.1560***	0.1465***	0.1577*	0.1362	0.1223	0.2170**	0.1764**	0.1707**
	(0.0516)	(0.0476)	(0.0493)	(0.0809)	(0.0862)	(0.0858)	(0.0825)	(0.0821)	(0.0825)
Baseline DG Bias			0.1439**			0.2116			0.0762
			(0.0664)			(0.1261)			(0.0509)
Fixed-effects									
school-grade-gender		Yes	Yes		Yes	Yes		Yes	Yes
Fit statistics									
Observations	521	521	521	521	521	521	522	522	521
R^2	0.01220	0.07475	0.09938	0.00392	0.06762	0.09162	0.00958	0.06249	0.06633
Within R^2		0.00817	0.03456		0.00280	0.02848		0.00601	0.01022

Clustered (strata) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Indices are simple averages of normalized sub-outcomes. All sub-outcomes are normalized around the control mean and control standard deviation. Cooperative video game scores come from the average scores of partners in the incentivized rounds of play of "Overcooked! 2". The mean solo scores normalized is the average of 3 solo rounds. The quiz score was derived from a short quiz on the video game rules and controls. Dictator game bias derives from the difference between the amount shared with same- and distinct-mother-tongue students. Baseline dictator game bias is the same measure taken at baseline. Willingness to pay for a co-ethnic partner derives from response to 12 questions in the adaptive choice experiment. Standard errors clustered by strata.

Table 5: Communication and experience heterogeneity by language match

Dependent Variables:	Communication Index		Amount Communication (Facilitator)		Communication Easy (Participant)	
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Constant	0.0075		5.28×10^{-15}		-1.24×10^{-14}	
	(0.0733)		(0.0985)		(0.0907)	
Low match	-1.865***	-1.795***	-2.095***	-2.053***	-1.648***	-1.543***
	(0.1434)	(0.1582)	(0.1553)	(0.1708)	(0.1954)	(0.2082)
High match	-1.007***	-1.042***	-1.362***	-1.338***	-0.6645***	-0.7490***
	(0.1479)	(0.1666)	(0.1871)	(0.2002)	(0.1462)	(0.1704)
Fixed-effects						
school-grade-gender		Yes		Yes		Yes
Fit statistics						
Observations	505	505	524	524	505	505
R^2	0.27666	0.34976	0.27772	0.37489	0.15264	0.20076
Within R^2		0.25478		0.27215		0.12299

Clustered (strata) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent Variables:	Experience Index		Enjoyed Playing		Would Play Again		Partner was Fun	
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Constant	0.0328		-3.9×10^{-15}		-5.16×10^{-15}		-2.2×10^{-15}	
	(0.0877)		(0.0866)		(0.0955)		(0.1085)	
Low match	-0.6003***	-0.4988***	-0.5761***	-0.4706***	-0.9746***	-0.8641***	-0.3834**	-0.3426**
	(0.1469)	(0.1354)	(0.1534)	(0.1480)	(0.1725)	(0.1683)	(0.1635)	(0.1622)
High match	-0.1852	-0.2367*	-0.1501	-0.2052	-0.3105**	-0.3628**	-0.0381	-0.0976
	(0.1242)	(0.1349)	(0.1459)	(0.1565)	(0.1266)	(0.1417)	(0.1278)	(0.1435)
Fixed-effects								
school-grade-gender		Yes		Yes		Yes		Yes
Fit statistics								
Observations	492	492	512	512	504	504	508	508
R^2	0.04744	0.13469	0.02956	0.10279	0.06720	0.12547	0.02859	0.12051
Within R^2		0.02916		0.01683		0.04696		0.01847

Clustered (strata) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Indices are simple averages of normalized sub-outcomes. All sub-outcomes are normalized around the control mean and control standard deviation. Communication easy and experience sub-outcomes derived from participant survey items on a 5-point Likert scale. Amount communication is derived from the average of two facilitator responses on a 7-point Likert scale. Standard errors clustered by strata.

Table 6: Productivity and cohesion heterogeneity by language match

Dependent Variables:	Productivity Index			Coop scores			Quiz score		
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Variables									
Constant	-0.0006			-1.46×10^{-16}			1.9×10^{-15}		
	(0.1042)			(0.1432)			(0.1072)		
Low match	-0.1365	-0.0985	-0.1204	-0.0224	-0.0433	-0.0681	-0.2452**	-0.1570	-0.1756*
	(0.0829)	(0.0961)	(0.0858)	(0.1270)	(0.1395)	(0.1279)	(0.0916)	(0.0999)	(0.0934)
High match	0.0101	0.0344	-0.0060	-0.1116	-0.0182	-0.0649	0.1045	0.0536	0.0194
	(0.1001)	(0.1032)	(0.0904)	(0.1491)	(0.1499)	(0.1375)	(0.1133)	(0.1289)	(0.1258)
Mean solo score (Normalized)			0.2829***			0.3248***			0.2395***
			(0.0328)			(0.0420)			(0.0488)
Fixed-effects									
school-grade-gender		Yes	Yes		Yes	Yes		Yes	Yes
Fit statistics									
Observations	522	522	522	524	524	524	522	522	522
R^2	0.00854	0.25341	0.34483	0.00257	0.33645	0.40714	0.02338	0.12907	0.16550
Within R^2		0.00686	0.12847		0.00043	0.10691		0.00822	0.04971

Clustered (strata) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent Variables:	Cohesion Index			-1 x Dictator Game Bias			-1 x WTP for Co-Ethnic Partner		
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Variables									
Constant	-0.0152			3.62×10^{-16}			-0.0305		
	(0.0492)			(0.0764)			(0.0769)		
Low match	0.1114*	0.1025*	0.0879	0.1027	0.0771	0.0558	0.1230	0.1292	0.1201
	(0.0593)	(0.0563)	(0.0562)	(0.1028)	(0.1158)	(0.1068)	(0.0855)	(0.0786)	(0.0804)
High match	0.2585***	0.2039***	0.1987***	0.2100**	0.1891*	0.1816	0.3071***	0.2187**	0.2159**
	(0.0581)	(0.0624)	(0.0637)	(0.0896)	(0.1037)	(0.1088)	(0.1028)	(0.1028)	(0.1018)
Baseline DG Bias			0.1454**			0.2133*			0.0775
			(0.0664)			(0.1259)			(0.0502)
Fixed-effects									
school-grade-gender		Yes	Yes		Yes	Yes		Yes	Yes
Fit statistics									
Observations	521	521	521	521	521	521	522	522	521
R^2	0.01877	0.07737	0.10250	0.00549	0.06906	0.09344	0.01558	0.06368	0.06769
Within R^2		0.01098	0.03791		0.00434	0.03042		0.00727	0.01166

Clustered (strata) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Indices are simple averages of normalized sub-outcomes. All sub-outcomes are normalized around the control mean and control standard deviation. Cooperative video game scores come from the average scores of partners in the incentivized rounds of play of "Overcooked! 2". The mean solo scores normalized is the average of 3 solo rounds. The quiz score was derived from a short quiz on the video game rules and controls. Dictator game bias derives from the difference between the amount shared with same- and distinct-mother-tongue students. Baseline dictator game bias is the same measure taken at baseline. Willingness to pay for a co-ethnic partner derives from response to 12 questions in the adaptive choice experiment. Standard errors clustered by strata.

Table 7: Counterfactual matchings

All low match	Min matching	$E[\tau]$	Max matching	All high match
0.121	0.133	Assumes $E[\tau] = \hat{\tau} = .173$	0.190	0.223

Notes: This table presents the range of counterfactual average treatment effects based on feasible and infeasible matchings. For each pair of cross-ethnic students within a strata, we assign a weight that is the sum of their estimated treatment effects (from the causal forest). If the pair have high mutual language skills we assign $w_{ij} = \beta_i^H + \beta_j^H$ and if they are a low-match we assign $w_{ij} = \beta_i^L + \beta_j^L$. The 'Max matching' is the matching that selects edges that maximize the weights and the table presents $\tilde{\tau} = \frac{1}{N} \sum_{(i,j) \in \text{Max matching}} w_{ij}$. Min matching is defined similarly. We approximate $E[\tau]$ from re-randomizations of the treatment assignment. For each randomization r, we calculate a $\tilde{\tau}_r$ and average these to get $E[\tau]$. Finally, we take the average of β_i^H for "All high matches" and the average of β_i^L for "All low matches".

Table 8: Baseline covariates and the propensity to have a high language match

Covariate	Sample Mean	(sd)	Intercept	(se).	Slope	(se)	p-value	N
Female	0.59	0.49	0.39	0.17	0.37	0.23	0.11	377
Grade 1	0.34	0.48	0.45	0.16	-0.21	0.22	0.34	377
Grade 2	0.28	0.45	0.41	0.16	-0.24	0.18	0.19	377
Grade 3	0.20	0.40	-0.09	0.07	0.54	0.20	0.01	377
Grade 4	0.17	0.38	0.22	0.16	-0.10	0.20	0.63	377
Albanian LOI	0.38	0.49	0.39	0.09	-0.02	0.17	0.90	377
Macedonian LOI	0.45	0.50	0.35	0.09	0.19	0.15	0.22	377
Turkish LOI	0.17	0.38	0.26	0.07	-0.16	0.11	0.16	377
LOI maps to ethnicity	0.97	0.18	0.97	0.02	-0.01	0.03	0.68	377
English fluency	7.92	2.03	6.37	0.33	2.92	0.58	0.00	377
Local language fluency (not LOI)	4.47	3.71	3.43	0.78	1.96	1.28	0.14	377
Attended Gostivar middle school	0.73	0.44	0.56	0.09	0.32	0.14	0.03	377
Has Cellphone	0.98	0.15	0.99	0.02	-0.03	0.04	0.40	377
Parent has college	0.45	0.50	0.46	0.06	-0.02	0.12	0.89	377
Plans to go to college	0.90	0.30	0.81	0.05	0.16	0.07	0.04	377
Life satisfaction (0-10)	8.45	2.08	8.56	0.46	-0.20	0.68	0.77	377
Trust (0-10)	6.71	2.49	7.22	0.35	-0.96	0.63	0.14	377

Notes: Table shows overlap sample characteristics (Sample Mean), where the overlap sample is defined based on the values for $\pi(1,1)$ where we see overlap in the data. We condition the probabilities on being treated so they range from 0 to 1. Then, for this sample we report characteristics for pairs whose probability of a high language match approaches zero (Intercept), and for pairs whose probability of a high language match approaches 1 (Slope) along with associated p-values from a t-test of the hypothesis that there is no correlation between standard errors are clustered by school-grade-gender-date of randomization.
Covariate details LOI is the language of instruction. LOI maps to ethnicity equals 1 if the student self-identifies with the ethnicity associated with their language of instruction (the measure excludes students with more than one local ethnicity). Self-reported English fluency and Local language fluency for non-LOI are on a scale from 0-10. Language fluency is imputed with a random forest regression for 1 student missing baseline data using information from the recruitment data.

Table 9: Baseline cohesion and video game proclivities: Correlations with the propensity to have a high language match

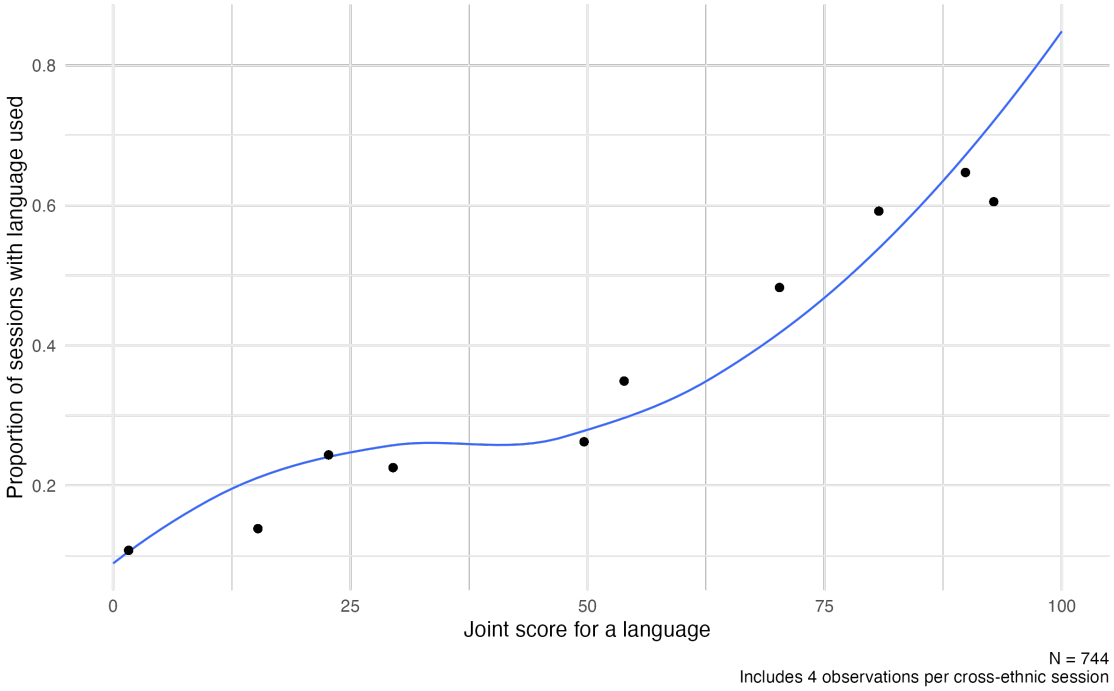
Covariate	Sample Mean	(sd)	Intercept	(se).	Slope	(se)	p-value	N
N peers in named for network	5.30	3.17	5.16	0.56	0.27	1.00	0.79	377
N out-group links named	0.60	1.00	0.59	0.18	0.03	0.30	0.93	377
Closeness to own LOI (baseline)	6.48	1.20	6.44	0.21	0.09	0.36	0.80	377
Closeness to out LOI (baseline)	2.02	1.11	1.62	0.14	0.74	0.27	0.01	377
Dictator game denar to outgroup (baseline)	228.45	132.73	223.37	28.41	9.55	38.96	0.81	377
Dictator game bias (baseline)	28.98	79.42	17.54	13.57	21.54	26.66	0.42	377
Enjoys video games	0.72	0.45	0.74	0.08	-0.04	0.13	0.78	377
Makes video game rec	0.62	0.49	0.59	0.09	0.06	0.15	0.70	377
Solo score	481.92	222.74	496.73	58.44	-27.87	84.44	0.74	377
Solo score (demeaned)	7.66	210.38	-1.54	53.52	17.33	78.24	0.83	377

Notes: Table shows overlap sample characteristics (Sample Mean), where the overlap sample is defined based on the values for $\pi(1,1)$ where we see overlap in the data. We condition the probabilities on being treated so they range from 0 to 1. Then, for this sample we report characteristics for pairs whose probability of a high language match approaches zero (Intercept), and for pairs whose probability of a high language match approaches 1 (Slope) along with associated p-values from a t-test of the hypothesis that there is no correlation between standard errors are clustered by school-grade-gender-date of randomization.

Covariate details Network data comes from 4 questions where students name other students as friends or sources of information and academic support. One question specifically asks for cross-ethnic links. Each question has 6 slots. 148 students (28 percent) use all the slots for friend cliques. Closeness measures come from the Inclusion of Other-in-Self measures (scale from 1-7). Dictator game bias is the difference between the amount of money an individual shares with anonymous ingroup (same ethnicity) recipients compared to what is shared with anonymous outgroup recipients. Enjoys video games equals 1 if the student responded to the question of "How well does 'I love video games' describe you" with 5 or greater on a scale from 0 to 10. Makes video game rec equals 1 if the student names at least one video game when asked for a recommendation, and zero otherwise. Solo score is the average score in solo rounds of "Overcooked! 2". Solo score demeaned demeans the score by gender-specific solo score means.

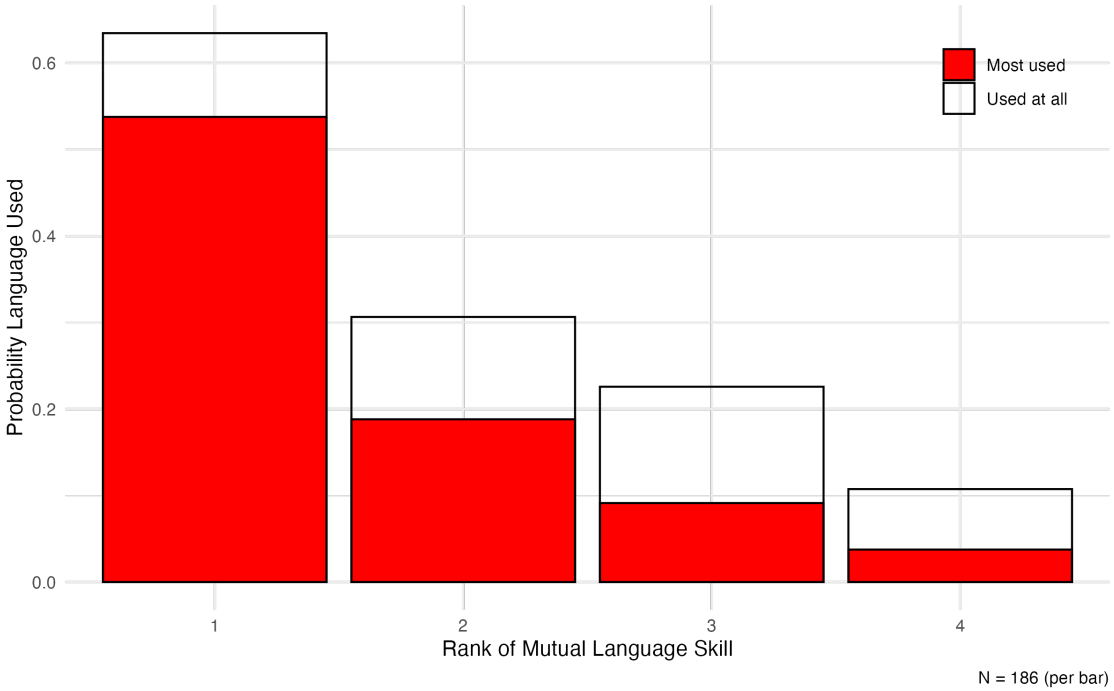
A Appendix

Figure A1: Language usage by joint score



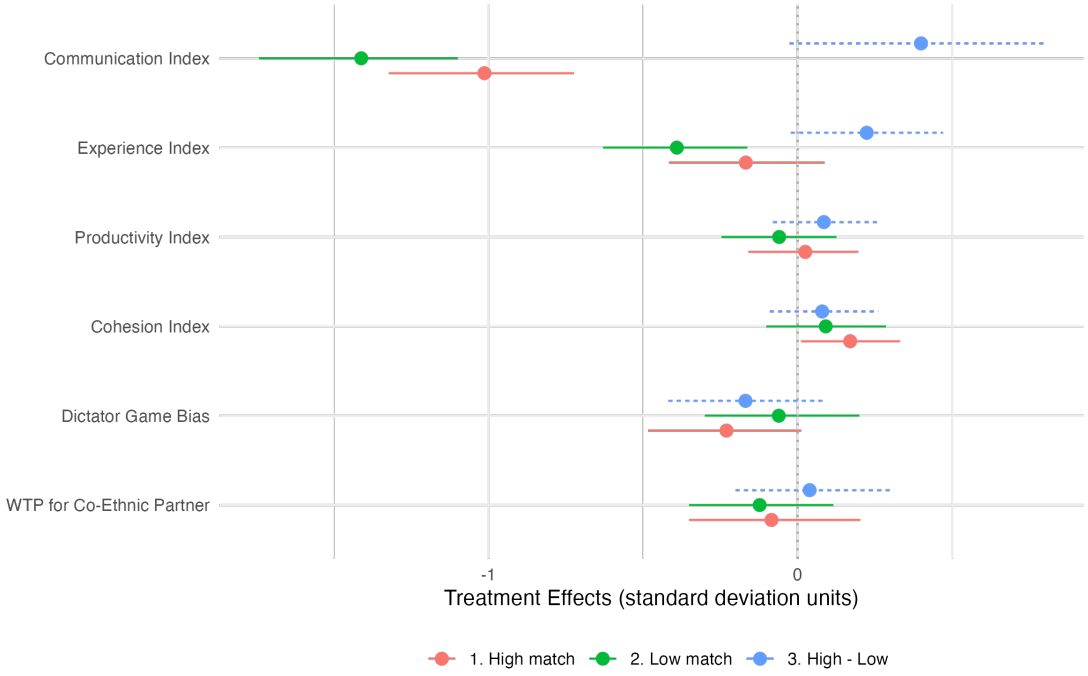
Notes: The plot shows the positive relationship between joint scores and language usage in the cross-ethnic sessions. Joint scores are the product of partners fluency measure for the language; each pair has a score for Albanian, English, Macedonian and Turkish.

Figure A2: Language usage based on the rank of mutual language skills



Notes: Within a pair, we rank mutual language skills based on joint scores (x-axis). The y-axis shows the proportion of sessions in which a facilitator reports the language is used (black outlines) or most used (red).

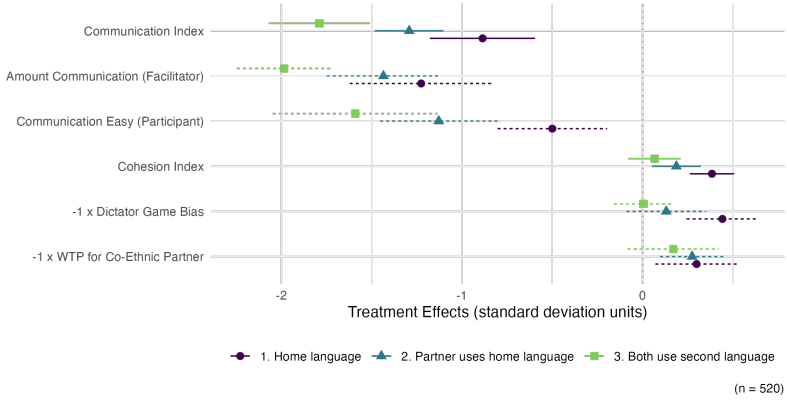
Figure A3: Treatment effects disaggregated by language match (IPW)



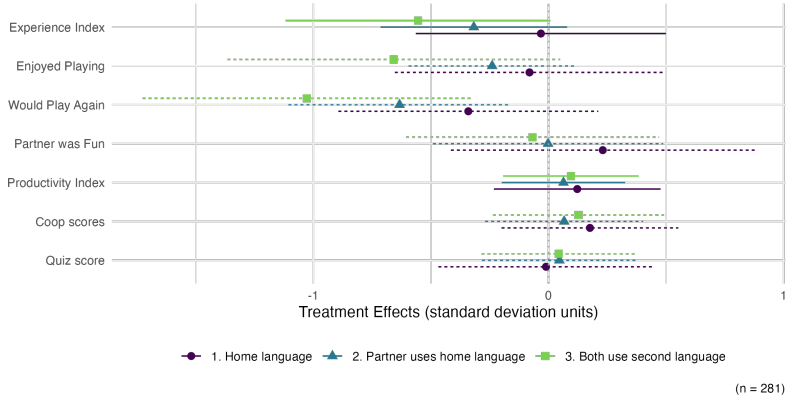
Notes: Inverse-probability weighted mean contrasts. Weights determined by simulating the treatment assignment mechanism. High (low) language skill match defined as above (below) median language match score. All sub-outcomes are normalized around control mean and control standard deviation. Indices are simple averages of normalized sub-outcomes. Bootstrapped standard errors.

Figure A4: Home and Second Language Effects

(a) Non-overlap Sample



(b) Productivity and Experience Outcomes (Overlap)



Notes The plot shows treatment effects compared to the control group for students whose 1) top "communication potential" is the student's home language, 2) top "communication potential" is the student's partner's home language, or 3) top "communication potential" is both students' second language (*lingua franca*). Estimated with causal forests with covariates listed in the paper. Points with solid lines are indices and dotted lines are submeasures.

Table A1: Full Sample Balance: Demographics

Covariate	Sample Mean	(sd)	Control Mean	(se).	Treat Diff	(se)	p-value	N
Female	0.53	0.50	0.54	0.10	-0.01	0.03	0.67	822
Grade 1	0.32	0.47	0.30	0.09	0.03	0.03	0.29	822
Grade 2	0.33	0.47	0.36	0.10	-0.04	0.03	0.23	822
Grade 3	0.22	0.42	0.22	0.08	0.00	0.02	0.99	822
Grade 4	0.13	0.34	0.13	0.07	0.01	0.02	0.81	822
Albanian LOI	0.44	0.50	0.48	0.03	-0.05	0.02	0.05	822
Macedonian LOI	0.33	0.47	0.32	0.03	0.02	0.02	0.38	822
Turkish LOI	0.22	0.42	0.20	0.03	0.03	0.02	0.19	822
LOI maps to ethnicity	0.94	0.23	0.96	0.01	-0.01	0.02	0.44	798
English fluency	7.37	2.31	7.43	0.14	-0.08	0.14	0.57	799
Local language fluency (not LOI)	4.88	3.63	4.98	0.31	-0.13	0.23	0.58	799
Attended Gostivar middle school	0.68	0.47	0.67	0.04	0.02	0.04	0.67	798
Has Cellphone	0.95	0.21	0.94	0.02	0.02	0.01	0.20	798
Parent has college	0.43	0.50	0.39	0.04	0.05	0.04	0.16	798
Plans to go to college	0.87	0.34	0.88	0.02	-0.01	0.02	0.65	798
Life satisfaction (0-10)	8.45	2.06	8.36	0.21	0.11	0.16	0.49	798
Trust (0-10)	6.66	2.52	6.76	0.23	-0.14	0.20	0.48	798

Notes: LOI is the language of instruction. LOI maps to ethnicity equals 1 if the student self-identifies with the ethnicity associated with their language of instruction (the measure excludes students who identify with more than one local ethnicity). English fluency and Local language fluency for non-LOI are on a scale from 0-10 described in the text. Language skills are imputed for 9 students missing baseline data. Clustered by strata.

Covariate	Sample Mean	(sd)	Control Mean	(se).	Treat Diff	(se)	p-value	N
N peers in named for network	4.93	2.94	4.96	0.29	-0.04	0.19	0.85	798
N out-group links named	0.61	0.90	0.62	0.06	-0.01	0.06	0.85	798
Closeness to own LOI (baseline)	6.36	1.39	6.40	0.10	-0.05	0.10	0.66	798
Closeness to out LOI (baseline)	2.08	1.18	2.08	0.07	0.00	0.07	1.00	798
Dictator game denar to outgroup (baseline)	219.89	128.76	227.94	12.63	-10.81	8.04	0.19	798
Dictator game bias (baseline)	29.26	80.96	32.48	8.20	-4.32	7.39	0.56	798
Enjoys video games	0.71	0.45	0.70	0.05	0.02	0.04	0.64	798
Makes video game rec	0.65	0.48	0.66	0.05	-0.02	0.03	0.45	798

Table A2: Full Sample Balance: Other baseline characteristics

Notes: Network data comes from 4 questions where we ask students to elect other students as friends or sources of information and academic support. One question specifically asks for cross-ethnic links. Each question has 6 slots. 148 students use all the slots for friend clique. Closeness measures come from the Inclusion of Other-in-Self measure (scale from 1-7). Dictator game measures are as described in the text. Solo score is the average score in solo rounds of "Overcooked! 2". Solo score demeaned demeans the score by gender-specific solo score means. Clustered by strata.

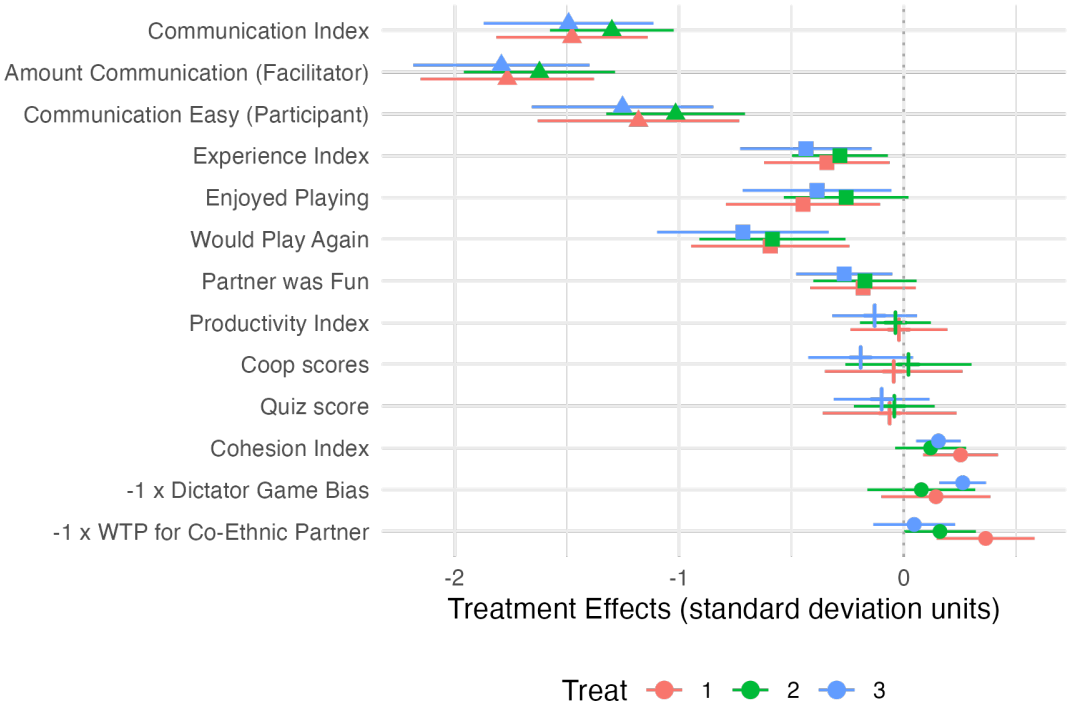


Figure A5: Treatment effects disaggregated by identity prime

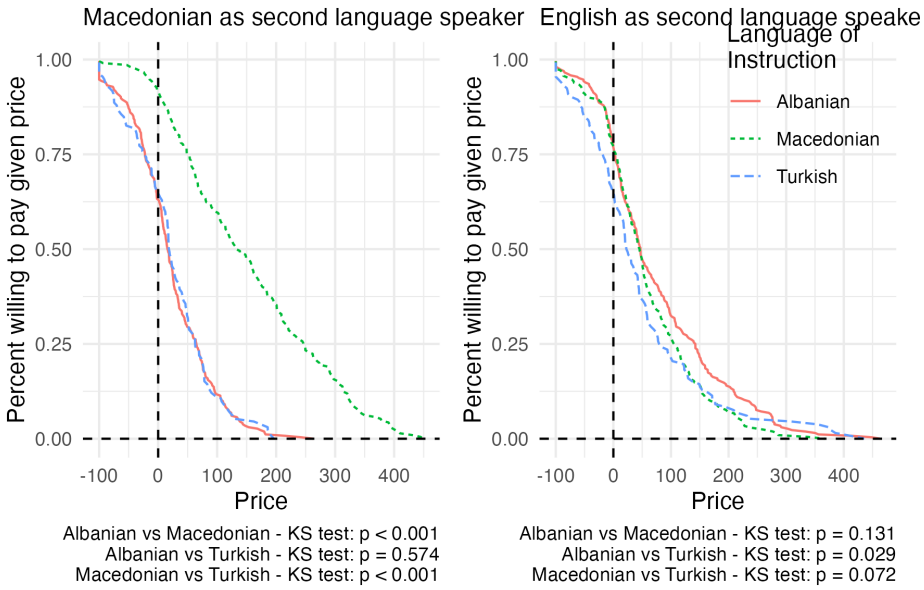


Figure A6: Caption

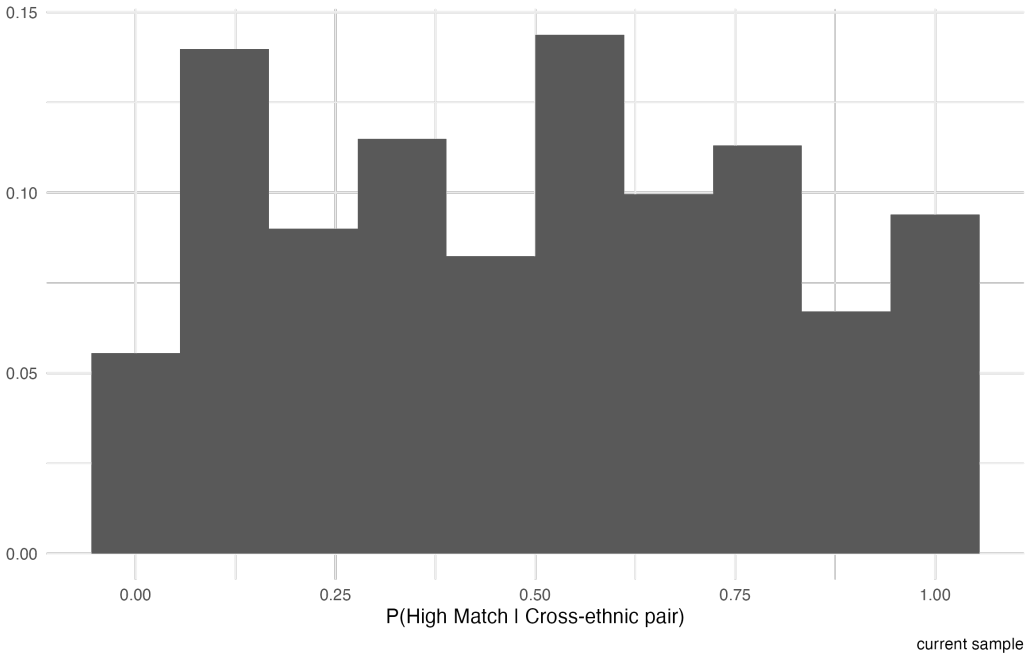


Figure A7: The probability distribution of a high language match conditional on being assigned a cross-ethnic partner. In the notation of exposure mappings this plot shows $\frac{\pi(1,1)}{\pi(1,1)+\pi(1,0)}$.

B Procedures

B.1 Randomization

Randomization into pairs that depend on baseline characteristics adds complexity. We face the following goals and constraints. First, we stratify by school, grade level, and gender; keeping gender and grade constant within pairs helps isolate identity and language's role. Moreover, we hope to split treatment-control groups at certain proportions to maintain power.[81] Finally, due to tight timelines, we randomized within strata on a rolling basis to provide steady work for the facilitation teams.

The first problem arises when some ethnic group is a supermajority meaning that the number of recruits from one ethnicity is larger than the other two combined. This occurs in 9 of 23 grade x school x gender strata.[82] To see why, consider an exaggerated case with 96 participants from E1 and 8 from E2. If one were to assign individuals to co- or cross-ethnic pairs at random with $\frac{3}{4}$ probability, there would be 72 E1's assigned cross-ethnic partners, but only 6 E2's to match them with. We can thus make 6 treatment pairs. Meanwhile, we create 13 control pairs (12 from E1 and 1 from E2). So, naively randomizing in this way, the realized probability of assignment to control is $\frac{13}{19} = 68\%$. This problem arises because we do not match students "assigned" to treatment in the largest ethnic group within the strata.

To overcome this challenge, we designed an algorithm that ensures participants have an equal *a priori* probability of assignment to *control* condition regardless of the composition of the strata. To address the issue mentioned above, we *adjust the control assignment probability* to avoid "over-assignment" to control. An additional nuisance is that strata may not be evenly divisible into pairs. To address this we use *probabilistic rounding to round to the nearest even number* while maintaining the target assignment probabilities.

Problem 1: Adjusting the assignment probabilities

As seen in the example above, we need a solution to randomization when we have a super-majority so that the control group size is roughly proportional to the target assignment probability p. We could either (1) *a*

[81] Moreover, maintaining equal assignment probabilities in expectation within strata helps reduce biases in estimation CITE AZEEM.

[82] This may reflect the underlying cohort populations, recruitment effort of the field team, truancy, or the preferences of students. Reflecting the underlying population, these tend to be Albanian supermajorities. Macedonians or Turks represent a plurality of students in 6 of the strata.

priori randomly drop students from the largest ethnic group or (2) adjust the initial assignment probability to $\tilde{p} \leq p$, where we select $\tilde{p}$ so that in expectation the control group size is proportional to p.

We consider both approaches and their implications using the running example. Under approach (1), we drop 96 - 8 students from ethnicity E1. Now we have 8 E1 and 8 E2. We then assign 4 to control and 12 to treatment. Under approach (2) we assign control units with probability $\tilde{p} \simeq .05$. Then 4.8 E1 units and .4 E2 units are assigned to control. We use probabilistic rounding (described below) to deal with non-even numbers, which provides a range of scenarios, with realized $p_{control}$ as one of $4\frac{}{20=20\%, \frac{6}{22}=27\%, \frac{6}{18}=33\% and \frac{8}{20}=40\%.}$

We use the second approach. While the solution is more complicated it allows us to maximize the sample for a given level of recruitment effort. In each scenario, we use more than 16 recruits. Both solutions provide expected assignment probability of roughly $\frac{1}{4}$.

To determine $\tilde{p}$, the following equation must hold

$$\tilde{p}N = p * (N - N_{dropped})$$

Where $N = |E1| + |E2| + |E3|$ is the total sample within strata with the $E1$ the largest group, p is the desired assignment probability and $N_{dropped}$ is the number of individuals left unassigned.

1. If $|E1| \leq |E2| + |E3|$ and N is even, we assign all units ($N_{dropped} = 0$).
2. If $|E1| \leq |E2| + |E3|$ and N is odd, we assign all but one unit ($N_{dropped} = 1$).
3. When $|E1| > |E2| + |E3|$, let $L = [|E1| - (|E2| + |E3|)]$ and $N_{dropped} = (1 - \tilde{p})L$

Notice that when $|E1| \geq |E2| + |E3|$, all cross-ethnic pairs have one member of E1 (i.e. there are no E2-E3 pairs).

Then we define $\tilde{p}$ for each of the scenarios as follows:

1. $\tilde{p} = p$

2. $\tilde{p}N = p(N-1) \implies \tilde{p} = p\frac{N-1}{N}$

3. $\tilde{p}N = p(N-(1-\tilde{p})L) \implies \tilde{p} = p * \frac{N-L}{N-p*L}$

Probabilistic rounding We use probabilistic rounding to address strata whose control units do not divide evenly into pairs. Say the strata of size N and target probability is p. If pN is not divisible by 2, we round up with a probability that is proportional to the remainder of pN divided by 2. There is an even number n such that $pN \in [n, n+2]$. Then, q is the probability of rounding up that solves the equation $(n+2)q+n(1-q) = p*N$. We round up with probability $q = \frac{pN-n}{2}$. Say $N = 13$. Then, we round up with probability $q = .25*(3.25-2)/2 = .625$ and down with probability $(1-q) = .375$.

For purposes of coding, we use $q = p*r/2$, where $r = mod(N, \frac{2}{p})$ is the number of remaining people in a block after the first $\frac{2}{p}$ students have been assigned to pairs. This avoids the need to determine n.

Returning to our running example, we round up E1 with probability $q_1 \simeq .8/2$ and we round up E2 with probability $q_2 \simeq .4/2$, so that the expected control probability is

$$\frac{4}{20}*(1-q_1)*(1-q_2) + \frac{6}{22}*q_1*(1-q_2) + \frac{6}{18}*(1-q_1)*q_2 + \frac{8}{20}*q_1*q_2 \simeq .25$$

Assigning cross-ethnic pairs

After making control assignments, we assign the remaining students to treatment. To do so we arrange the list first by the size of the ethnic group in the strata and then by a random number. If we have an even number of students and the triangle inequality holds ($|E1| \leq |E2|+|E3|$), we split the list in half and match partners by their location in the list.[83] When $|E1| \geq |E2|+|E3|$, we drop the first $|E1|-(|E2|+|E3|)$ students from E1 and match the remaining students from $E1$ with the students from the other ethnicities, matching by list order. Similarly, we drop one student from $E1$ if the total treatment population is odd but the triangle inequality holds. Ordering by ethnic group size first ensures the matches are cross-ethnic. We then randomly assign the pair a specific treatment assignment. The assignments are then input into Qualtrics.

[83] i.e. The first person in the top half of the list matches with the first person in the bottom half of the list, the second with the second, and so on.

Assessment of randomization procedure On average, the procedure achieved its goal: The realized control assignment probability is 25.3 percent. Moreover, in 5000 simulations of the estimation procedure, conditional on any assignment there's a 24.8 percent probability of being assigned control on average across individuals. (There is a 5.5 percent probability that an individual is not assigned treatment or control due to oversubscription of some ethnic group within some strata or lack of parity.) Moreover, within strata, we simulate control probability conditional on any assignment and find all but 4 strata with expected control probability within 0.5 percent of the target (min = 22.5%, max = 25.2%).

Individuals have control probabilities that are not equal to 25 percent by design and also due to logistical constraints in the field. Recall that $\tilde{p}$ is the individual level treatment probability and so within imbalanced strata, individuals will face treatment probabilities less than p. Further, for 12 of 23 strata, we ran randomization in waves–in follow-up waves, we included E1 students who were passed over to maximize our sample size while staying within budget. Such students have a slightly higher expected probability of control assignment. When using inverse propensity weighting we account for these discrepancies. Finally, 6 individuals were the sole representatives of their ethnicity within a strata during a randomization wave. Dropping these individuals does not affect the results.

B.2 Data collection

Participation consisted of several activities. First, enumerators from our survey firm conducted baseline surveys, collecting data about language skills, social networks, behavioral measures, and questions about socio-economic status and well-being. For the most part, once both partners in a pair completed the baseline survey, we sent the session information to the facilitation team who organized the video game sessions described in this paper. In addition to learning and playing "Overcooked! 2", students answered questions about their experience in the session, completed the choice experiment, provided additional behavioral measures, and decided which pin to take as a reward either one reflecting the common identity (town or school insignia) or one representing the students' ethnic identities reflecting their mother tongue alphabet.[84] Finally, students participate in a midline survey where we ask students to sign up for the interethnic course and re-ask social network and behavioral measure questions from the baseline. These three steps constitute the major

[84] Initially we thought to provide pins with the flags of Albania, Macedonia or Turkey, which are commonly used to represent ethnicity in Macedonia. Our field partners advised us against this, as the flags are divisive.

data sources for this paper. Many of the same participants were invited to participate in our companion study, wherein students in groups of 6 participated in a social learning game, where students pass information via a website we built for the study. The second study involves very limited cross-group interaction as students are instructed to remain quiet. Nevertheless, we strive to have students complete the first three steps before participating in Study 2.

C Appendix: Bayesian Adaptive Choice Experiment: Theory and Implementation

Here we present the theory behind the Bayesian adaptive choice experiment (BACE) and our implementation thereof. We implement BACE using software developed by Drake et al. (2022) and our discussion of theory relies on the accompanying paper (Drake et al. (2024); Hereafter DPTT).

The goal is to estimate preference parameters $\theta_i \in \mathbb{R}^k$ for individual $i \in N$ over k attribute classes. In our case, we measure willingness to pay for partner ethnicity and second language skills. We estimate θ in a Bayesian framework retrieving posterior $Pr(\theta_i|x_i^{(1:T)}, d_i^{(1:T)})$ where x is histories of choices (option A or option B) and d are the choice scenarios that the agent faced. (*e.g.* Option A provides 50 more Denar than Option B but unlike Option B, Option A does not speak English and otherwise they are the same.)

DPTT provides a method to efficiently search the parameter space. The researcher's problem is to choose d_{t+1}^*, the optimal choice scenario to show next, given a prior $Pr(\theta_i|x_i^{(1:t)}, d_i^{(1:t)})$. d_{t+1}^* is best in that it maximizes the mutual information gained (or, alternatively, minimizes entropy). The solution provides a multidimensional analogy to binary search in that d_{t+1}^* splits the current prior into two "half-spaces" in which the student is equally likely to choose either option in the menu.

$$d_{t+1}^* = \arg\max_{d_{t+1}} \int_\theta \sum_{x^{t+1}\in\{0,1\}} \left[\log \frac{Pr(\theta|x^{(1:t+1)}, d_{t+1})}{Pr(\theta|x^{(1:t)})}\right] Pr(x^{t+1}|\theta, d_{t+1}) Pr(\theta|x^{(1:t)}) d\theta$$

To help focus on the current design choice d_{t+1}, we suppress reference to the design history $d^{(1:t)}$ on which all the $Pr(.|.)$ objects depend. We also suppress reference to the decision maker i though each individual has their own parameters and choice scenarios.

The learning process follows the following *Algorithm* 1) sample θ from the current prior distribution, 2) numerically integrate over θs over a range of possible designs to determine d_{t+1}^*, 3) provide choice scenario d_{t+1}^* to the student to learn x^{t+1} and 4) update the prior. (ie. the posterior is the prior for the next iteration.)

To sample from the distribution, we use population Monte Carlo (PMC). Section 9.2 of DPTT (2024) provides

a thorough explanation of the method; we highlight a few key points. At a high level, PMC provides a sample "approximately" from the target distribution and "approximately unbiased" estimates of an integral under the distributionCappé et al., 2004. It does so by drawing thousands of points from the proposal distribution and then "pruning" them using importance sampling weights. This is fast enough to compute on a server and send the next choice to the decisionmaker.

C.1 Implementation

Students derive expected utility from a reward r and a partner's ethnic and linguistic attributes captured in z. When running BACE for data collection, we include specific ethnicity markers (i.e. Turkish, Albanian, Macedonian); While we collapse these to own ethnicity or not for analytic purposes.:

$$u(z) = 1 * r + \theta' z + \epsilon$$

z includes an indicator for whether or not the potential partner is from the same ethnicity and whether they speak English, Macedonian, or neither as a second language. We normalize the coefficient on the reward to 1, which puts θ into Denar units. Students are told to assume both options are students in their cohort, who have the same gender and are otherwise identical. ϵ is a Type-1 extreme value shock term, which we interpret as the decision makers' uncertainty and attention

Students chooses between A and B, choosing A if

$$1 * r_A + \theta' z_A + \epsilon_A > 1 * r_B + \theta' z_B + \epsilon_B$$

and B otherwise. Equivalently, they choose A if

$$(r_A - r_B) + \theta'(z_A - z_B) > -(\epsilon_A - \epsilon_B)$$

Likelihood: This corresponds to the logit likelihood:

$$Pr(x^{t+1} = 1|\theta) = \frac{1}{1 + \exp(-\mu(\Delta r + \theta' \Delta z))}$$

we replace differences with Δ, and introduce μ, a dispersion parameter that we estimate that captures attention or indifference. As μ gets large, it magnifies small differences in utility so that the probability the student picks the option with higher utility goes to 1. We estimate μ is smaller if students make more "mistakes".

Prior: The prior for each z is a normal distribution centered at 0. We set the variance to 100 Denar for ethnic matches and 200 Denar for language skills. This prior is somewhat conservative as we center the prior estimates around 0. The prior over the dispersion parameter μ is uniform between 0.01 and 0.09.

Choice scenarios/Designs: The set of choices includes 1275 distinct designs, or vectors $(\Delta r, \Delta z)$. The reward Δr takes one of 25 values between -100 to 500 Denar (roughly -\$2 to \$10) with the options taking values divisible by 25 Denar (about 50 cents USD). There are dependencies between ethnic and linguistic factors in the design which lead to 51 distinct designs. Δz takes a value in $-1, 0, 1$ reflecting differences in indicator variables. An exemplary $\Delta z = (-1, 0, 1, 0, 1)$ with the z_1, z_2 and z_3 corresponding to ethnicities (Albanian, Macedonian, and Turkish), z_4 corresponding to English ability and z_5 corresponding to Macedonian ability. Then, this example corresponds to option A being Turkish and a second-language Macedonian speaker. Option B is Albanian and we randomly decide if they both speak English or not.[85]

We learn individuals' preferences using the algorithm discussed above. At each iteration, we take the optimal design and translate it into a choice as described above. They are told to imagine the students are identical in other respects and equally good at playing "Overcooked! 2". Students face 12 choice scenarios using the decision problem discussed above to select the next menu of options. We chose 12 to balance the need to collect sufficient data for estimation with the attention span of the students. Simulations show a large gain in terms of mean square error over the first 10 choices after which the learning curve flattens out.

[85] Why are there 51 options? For each ethnic pairing, an ethnicity can be shown in option A or option B (1 or -1). Thus, there are four sets of ethnic pairings with Macedonians. Each of these pairings have 2 MK as second language options (the Macedonian cannot be a second language speaker) and 3 ESL options $4 \cdot 2 \cdot 3 = 24$ such choice scenarios and 3 sets that (potentially) exclude Macedonians (including the option where ethnicity is the same for either option) $3 \cdot 3 \cdot 3 = 27$ choice scenarios providing 51 total possible vectors that are z. If we pull vectors of $z = (0, 0, 0, 0, 1)$ we randomly choose an ethnicity for both option A and B and if it's Macedonian we ignore Macedonian as a second language. In practice, this quirk affects roughly 2 percent of choice scenarios.

C.2 Preference estimation

For estimation, we use a similar population Monte Carlo procedure to estimate individual-level parameters. We adjust the choice problem so that z now includes the two second language indicators as before and an "own ethnicity" indicator, which equals 1 if the ethnicity shown matches the student's language of instruction and 0 otherwise. For each student i, we have a vector $x_i^{(1:T)}$ of binary choice indicators and a T-by-3 matrix of designs $d_i^{(1:T)}$. We feed these data, the likelihood function, and a prior through a procedure that returns the posterior distribution for the parameter vector θ_i.

We use a two-step procedure. We first run the analysis with a prior that is composed of three independent distributions $N(0, 100)$. Then, we take the resulting set of $\{\theta_i\}_{i \in N}$ to create priors that are normally distributed with mean and variance calculated from the leave-one-out set of thetas (*e.g.* For individual i, the prior is centered at $\bar{\theta}_{-i} = \frac{1}{N-1} \sum_{-i} \theta_k$).

D Appendix: Language Skill Match

This section argues that $f_{\max}$ is an appropriate measure to capture the complementary language skills of a pair. Recall that we students self-rate their English, Macedonian, Albanian, and Turkish skills on a scale of 0 to 10. Then, for pair i and j, we determine a joint score in a given language k by multiplying i and j's scores: $f_k = f_{ik}\dot{f}_{jk}$. And we defined:

$$f_{\max}_{\text{language skill match}} = \max(f_a, f_e, f_m, f_t)$$

where the subscripts correspond to the first letter of the languages under consideration.

When f_k is large, students are more likely to use that language as reported by the session facilitators. Language use is distinct from the "amount of communication" outcome reported in the main result. The session facilitators record which languages they hear the students use during gameplay. They also report an intensity metric which is supposed to capture the proportion of time each language was used. Though in practice, roughly 60 percent of reports add up to 100 percent and often facilitator pairs are not in agreement on this measure. We average over the facilitators' measures and assume that the relative ranking is accurate. So, a language is "most used" if it has the highest usage proportion within a session. Figure A1 shows the proportion of sessions where language k is used given the joint score of a pair f_k. The plot shows a sharp increase in the likelihood a language is used as the joint score increases. Roughly one in three pairs use a language if their mutual skills are 50; this jumps to 80 percent when both students report top fluency in a language. On the other extreme, it may seem odd that 10 percent of pairs use a language when the joint score is 0; this may reflect a student who talks in a language despite not being understood.

While sessions are multilingual affairs, the ordering of the joint skills provides a strong signal as to whether students use the language. To explore this further, index the joint skills in order so that $f_{\max=f_1 \geq f_2 \geq f_3 \geq f_4}$. Figure A2 demonstrates two things: First, students are most likely to use the language they jointly know the best (black outlined bars). The probability a language is used declines along with its position in the ordering. Second, this pattern is more extreme when considering the "top match" among languages used (red bars). Pairs use their top language match the most roughly 55 percent of sessions.The second red bar shows that

roughly 19 percent of pairs use the language with a joint score f_2 more than their top language match. This tends to happen if f_1 and f_2 are very close; though this could reflect sub-optimal coordination. Roughly 18 percent of pairs use f_1 *and* f_2.

Taken together, the joint score f_k captures communicative capacity generally and $f_{\max}$ provides an easily understood one-dimensional measure that eases the analysis. When considering high or low language match, we lose very little by making this simplification. After all, when students rely on f_2, they typically do so when f_1 and f_2 are very close. What is more, we run regressions and use random forests to predict the amount of communication in a session using simple models that make use of f_1 through f_4. The model that includes f_1 alone outperforms nearly identical to models that use combinations of the joint scores (in terms of mean square error of out-of-bag predictions with random forests). On the downside, the use of $f_{\max}$ adds some measurement error when considering *lingua francas*.

E Appendix: Policy Counterfactuals

In this appendix, we provide details on the policy counterfactuals. In the counterfactual, we imagine that a decisionmaker can assign students to co-ethnic partners based on their observable characteristics. How does re-assignment impact expected changes in cohesion outcomes and non-pecuniary costs?

Let $Y_i(1, d(i,j))$ be student i's potential outcome after being assigned partner j where $d(i,j)$ is an indicator that equals 1 if the pair (i,j) has a high communication potential. Within gender by cohort strata S, we find the assignment vector $\{d(i,j)\}$ that maximizes the following problem:

$$\max_{\{d(i,j)\}: i,j \in S} \sum_{i \in S} Y_i(1, d(i,j))$$

In words, we maximize the aggregate outcome subject to the constraint that there is a feasible matching $\{d(i,j)\}$.

This is a maximum matching problem. The idea is to find the set of edges with the largest sum of weights in a graph such that no two edges share a vertex. To solve the problem, we use Edmond's Blossom algorithm.[86]

First, we create an edge connecting each set of cross-ethnic pairs. For the pair i, j, we calculate

$$d(i,j) = \mathbb{1}_{\text{Communication Potential} \geq m}$$

. Where m is the median communication potential. Then assign the weight to edge:

$$w_{ij} = \begin{cases} \beta_i^H + \beta_j^H, & \text{if } d(i,j) = 1 \\ \beta_i^L + \beta_j^L, & \text{if } d(i,j) = 0 \end{cases}$$

Where β_i^H and β_i^L are the predicted personalized treatment effects returned from passing individual i's co-

[86] Graph theory provides a suite of algorithms for solving maximum matching problems. Because we have three ethnicities, we cannot rely on simpler algorithms. For example, if this were a bipartite graph, we could use the Ford-Fulkerson max flow algorithm or the Hungarian algorithm.

variate profile to the generalized random forest. Edmond's Blossom algorithm then efficiently finds the set of pairs (matching) that maximizes (or minimizes) the sum of the weights.