Bilingual Education Reduces Ethnic Outgroup Discrimination Through Perspective-Taking*

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Abstract

How to reduce intergroup prejudice is one of the most difficult challenges many societies face. Research on prejudice reduction usually focuses on short-term interventions that yield relatively inconclusive results. Instead, I focus on people's formative years and argue that bilingual instruction in schools durably reduces political discrimination against ethnic outgroups. By promoting the acquisition of a second language, bilingual instruction facilitates the cognitive development of perspective-taking ability, which in turn fosters more inclusive political attitudes. I find support for this argument by studying the effects of an education reform in Malaysia, which resulted in those students who were born after the cutoff receiving bilingual instruction, while those born before the cutoff were only taught in their mother tongue. I also provide suggestive evidence that the observed patterns stemmed from improvements in perspective-taking ability. The findings imply that education plays a pivotal role in reducing intergroup prejudice from an early age.

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Ethnic outgroup discrimination and ingroup favoritism are pervasive issues in many multi-ethnic societies. Citizens generally display a strong preference to vote for coethnic candidates (Adida, 2015; Ferree, 2006; McConnaughy et al., 2010) while political elites often implement policies that benefit members of their own ethnic groups (Ejdemyr et al., 2018; Kramon and Posner, 2016). Moreover, this interplay between ethnic identities and political behavior further exacerbates the under-representation of ethnic minorities, which in turn raises the likelihood of civil wars (Buhaug et al., 2014; Cederman et al., 2010). Hence, considerable attention has been paid to the remedies that reduce intergroup prejudice. These include institutional reforms such as the use of quotas to improve minority representation (Chauchard, 2017; Dunning and Nilekani, 2013), and non-institutional factors such as cross-ethnic contact and networks (Adida et al., 2016; Brown et al., 2021). However, the inclusionary impacts of many proposed interventions are either mixed or short-lived (Paluck et al., 2021; Parthasarathy, 2017). This may be because political attitudes are deeply ingrained during childhood and are resistant to change in adulthood (Sears and Funk, 1999; Tesler, 2015). It might therefore be more fruitful to focus on immersive interventions during a person's formative years, such as parental influences (Acharya et al., 2016) and education experiences (Ostwald et al., 2019) to better understand what shapes intergroup prejudice.

In this article, I consider whether bilingual instruction, as an early life intervention, is able to mitigate the political discrimination of ethnic outgroups. Bilingual instruction refers to the use of two languages to teach content subjects such as History or Math (see Liu, 2011).[1] Drawing on insights from the bilingualism and perspective-taking research in psychology (Galinsky et al., 2005; Schroeder, 2018), I argue that this type of instructional method facilitates second language acquisition (Reljić et al., 2015; Rolstad et al., 2005), thereby creating bilingual speakers who are better at understanding other individuals' motives and actions – i.e., they are better at perspective-taking (Javor, 2017; Schroeder, 2018). Being able to speak two languages allows bilinguals to adapt and cater to different listeners'

[1]Hence this concept does not encompass the teaching of a second language as a separate subject, such as German classes in schools.

linguistic demands. For many cognitive scientists, this process of switching between multiple languages is analogous to the cognitive process of shifting from one person's perspective to another. Given the higher rate of language switching at home and/or at school, bilinguals are therefore subconsciously trained to curtail their own predispositions and biases while shifting their attention toward the mental state of the perspective-taking target (Bialystok and Viswanathan, 2009; Carlson and Meltzoff, 2008). More broadly, bilingualism promotes the cognitive development of perspective-taking *regardless of the type of second language spoken* (e.g., ethnic minority or colonial languages). Finally, prior work shows that this ability to take the positions of others helps decrease exclusionary political attitudes toward a variety of social outgroups (Adida et al., 2018; Kalla and Broockman, 2020).

To test these claims, I analyze the impact of an education reform in Malaysia, where the ethnic majority language – i.e., *Bahasa Melayu* – was replaced by English as the language of instruction for Math and Science in all public secondary schools in 2003. Ethnic Malay citizens (i.e., the ethnic majority group in Malaysia) who were born between 1990 and 1995 would have been exposed to a combination of English and *Bahasa Melayu* instruction during their secondary school education, whereas those who were born between 1985 and 1989 would have received monolingual instruction in their mother tongue (i.e., *Bahasa Melayu*) for all subjects including Math and Science. As such, the as-if random assignment of the language of instruction in Malaysian schools (i.e., bilingual versus monolingual instruction) allows me to identify the causal effect of bilingual education on ethnic outgroup prejudice.

I fielded an original survey to more than 2,000 ethnic Malay participants in March 2022, asking a series of questions that measure one's political attitudes toward ethnic minorities in Malaysia. Overall, I find that ethnic Malay participants who received bilingual instruction display less political discrimination against ethnic minorities than those who received monolingual instruction. The results are also robust to a variety of additional tests and alternative explanations such as cross-ethnic contact and socioeconomic status. I further validate the mechanism underlying this empirical pattern by providing evidence suggesting

that bilingual instruction improves perspective-taking. In particular, the reform had the largest impact in improving perspective-taking ability among ethnic Malay participants who only speak their mother tongue at home – i.e., the group of individuals who would have remained as monolingual speakers if not for the language reform in 2003. Finally, I examine the generalizability of the findings through a cross-national analysis of the Asian Barometer Survey (ABS, 2017). Consistent with my theoretical arguments, bilingual speakers, regardless of the type of second language spoken, exhibit less prejudice against outgroup members than those who only speak one type of language at home.

This article makes three contributions. First, it builds on the rich literature that the development of political norms and values can be traced to a person's early upbringing in school (Campbell and Niemi, 2016; Cavaille and Marshall, 2019; Neundorf et al., 2016). Specifically, I argue that the socializing impact of education extends beyond the acquisition of civic skills and political knowledge (Finkel and Ernst, 2005; Nelsen, 2021; Niemi and Junn, 1998). Education also influences our cognitive ability to accept members of ethnic outgroups. Second, this article highlights the importance of studying political attitudes and behavior through a psycholinguistic lens. Previous works have shown that the language we speak can influence our outlook on a variety of salient issues, such as climate change (Pérez and Tavits, 2017), protection of minority rights (Pérez and Tavits, 2019*a*), and gender equality (Liu et al., 2018; Pérez and Tavits, 2019*b*; Tavits and Pérez, 2019). This research underscores the cognitive impact of bilingual education on intergroup tolerance by enhancing citizens' ability to put themselves in the shoes of others. Hence, the proposed theoretical mechanisms can be generalized to other ethnolinguistic settings and ostracized groups such as immigrants and refugees. Lastly, this article speaks to the burgeoning scholarship on perspective-taking and prejudice reduction (Adida et al., 2018; Kalla and Broockman, 2020). I show that perspective-taking is an ingrained cognitive ability that can be cultivated during a person's formative years, as opposed to a one-off response that is induced through reading narratives about other social outgroups. As such, bilingual education may represent a more durable

solution to the challenge of reducing intergroup prejudice in modern societies.

Bilingual Education, Perspective-Taking & Outgroup Attitudes

I argue that bilingual education has important consequences on intergroup relations. Specifically, individuals who were exposed to bilingual instruction in school should possess more inclusive political attitudes than those who received monolingual instruction. This theoretical claim stems from three insights. First, bilingual instruction promotes bilingualism – i.e., the ability to speak two languages. Second, bilinguals possess better perspective-taking ability than monolinguals. Third, the ability to understand other individuals' mental states (e.g., their motives and actions) should be linked to lower levels of political discrimination against ethnic outgroups. I explain the causal pathway in greater detail below.

One of the more immediate consequences of bilingual education is an increase in the number of hours that a student will have to read, write and speak in a second language. As previously mentioned, this article focuses on the effects of language as a *medium of instruction* for content subjects such as History,[2] as opposed to language as a *subject* such as Spanish classes in American schools. One key difference is the level of second language immersion. In particular, the former approach creates a more naturalistic learning environment by providing a purpose for language use in the classroom as well as increasing the amount of exposure to the second language (Dalton-Puffer et al., 2010). This increases the likelihood that students will develop into bilingual speakers in the future (Reljić et al., 2015; Rolstad et al., 2005). In addition, bilingual education can be especially beneficial for students who only converse in their mother tongue at home, by making up for the lack of second language exposure and training in monolingual families (Rumlich, 2020).

By improving their proficiency in the second language, students develop into bilinguals whose perspective-taking ability is widely considered to be superior to monolinguals. Why

[2]This is more commonly referred to as Content and Language Integrated Learning (CLIL) programs in the field of education research. See Dalton-Puffer et al. (2010) for a review of CLIL programs.

are bilinguals better at comprehending other individuals' intentions and actions than monolinguals? Schroeder (2018) summarizes three prevailing explanations based on findings from both observational and experimental studies. First, cognitive scientists argue that bilingualism improves executive functioning such as attention control, working memory, and inhibition abilities – i.e., cognitive functions that are crucial for perspective-taking (Bialystok and Viswanathan, 2009; Carlson and Meltzoff, 2008). Due to the habitual process of frequently switching between two languages and the suppression of one medium to use the other, bilingual speakers are subconsciously trained to control and down-weigh their prior beliefs and knowledge while "up-regulating" and shifting their attention toward understanding another person's mental state (Schroeder, 2018; Zhang et al., 2015).

A second set of explanations focuses on bilinguals' heightened sense of metalinguistic awareness, defined as the ability to dissect and reflect on the properties of a language (Bialystok and Barac, 2012). As an example, Estonian-Russian bilinguals' innate capacity to distinguish between a language that has gender markings (i.e., Russian) and a genderless tongue (i.e., Estonian) might be translated into a more general understanding that two people can interpret and react to the same event differently. Indeed, Pérez and Tavits (2019*b*) find that Estonian-Russian bilinguals displayed more gender unequal attitudes when interviewed in Russian than in Estonian. This finding therefore lends support to the view that bilinguals' cognitive flexibility to alter their predispositions and opinions under different linguistic contexts might explain why they perform better than monolinguals in assuming the beliefs and motivations of a perspective-taking target while downplaying their own mental states (see Liu et al., 2018). Lastly, the socio-pragmatic account postulates that the ability to switch between multiple languages allows bilinguals to operate more effectively in multicultural settings. This leads to a greater appreciation that different language speakers may subscribe to different sets of beliefs and values, as well as an improved ability to consider another person's mental state (Fan et al., 2015; Goetz, 2003; Ringe, 2022).

Taken together, the ability to take the perspectives of others can be improved when

individuals are constantly forced to switch between two (or more) languages.[3] Specifically, the process of adapting and catering to the linguistic needs of different audiences is analogous to the cognitive process of switching and taking the perspectives of others. When individuals switch from one language to another, they are also (subconsciously) training their cognitive flexibility to switch from their own personal thoughts to another person's mental state. The higher the rate of language switching, the better the ability to take the perspectives of others. This might therefore explain why several empirical studies have shown that bilinguals are on average better perspective-takers than monolinguals, given the higher rate of language switching in the former (see Schroeder, 2018).

Moreover, the fact that perspective-taking effects are observed among Arabic-Hebrew speakers in Israel (Bekerman and Horenczyk, 2004), English-Tamil bilinguals in India (Bialystok and Viswanathan, 2009), Hungarian-Serbian bilinguals in Serbia (Javor, 2017) etc., suggests that the link between bilingualism and perspective-taking ability may be applicable across a wide variety of linguistic and country contexts. Put simply, these explanations are agnostic about whether a specific type of second language promotes the development of perspective-taking ability. It does not matter whether the second language spoken by the bilingual is an ethnic minority language (e.g., Russian in Estonia) or a colonial tongue (e.g., English in India); simply being able to speak more than one language should facilitate the cognitive development of perspective-taking ability.

Another important implication is that individuals who were brought up in bilingual families should enjoy an *initial* perspective-taking advantage over those who were raised in monolingual homes, since the frequency of language switching is higher among individuals from bilingual families than those from monolingual backgrounds. This perspective-taking gap however may be narrowed with the use of bilingual instruction in schools. Specifically, bilingual education may compensate for the lack of second language training in monolingual homes by providing an opportunity for these students to immerse themselves in a bilingual

[3]Indeed, the above mechanisms can also be generalized to multilingual contexts where people learn and speak more than two languages.

setting while cultivating their perspective-taking ability through the use of two languages.

This ability to display greater sensitivity toward another person's mental state in turn reduces bilinguals' susceptibility to discriminate against ethnic outgroups. By incorporating the other in the self, perspective-takers develop a sense of connectedness with the perspective-taking target (Galinsky et al., 2005), of which the latter is transformed from a member of an objective outgroup (e.g., ethnicity) into a subjective ingroup (i.e., shared mental states between the perspective-taker and the target). Taking the perspective of an individual from an ethnic minority group who, for instance, has been denied entry into a university because of their ethnic membership, may also lead to a more general realization that this predicament extends to other members of the target's ethnic group as well (Todd et al., 2012). In addition, studies have shown that the propensity to attribute negative facts about an ethnic outgroup to dispositional (e.g., ethnic group A is poor because its members are lazy) as opposed to situational (e.g., ethnic group A is poor because of discriminatory hiring practices) explanations can be reduced by perspective-taking (Todd et al., 2012). Notwithstanding its salutary influence on ethnic attitudes, perspective-taking is linked to more inclusive behavior toward a variety of ostracized social groups, such as refugees and immigrants (Adida et al., 2018; Kalla and Broockman, 2020), the LGBTQ+ community (Broockman and Kalla, 2016), and former perpetrators of political violence (Bilali and Vollhardt, 2013).

Overall, bilinguals' propensity to exhibit less ethnic discrimination may be explained by their perspective-taking ability, which is accumulated over time due to their bilingual training at home and/or at school. In other words, bilinguals are *ex ante* more mindful of other individuals' beliefs and actions, and are more likely to practice perspective-taking as an everyday social function than monolinguals. Indeed, Singh et al. (2020) observe that bilingual English-Mandarin children in Singapore exhibited less implicit bias against African race individuals than children who only spoke Mandarin. This finding is remarkable, considering the fact that people of African descent constitute less than one percent of the resident population in Singapore and that the participants did not receive any perspective-taking

stimuli during the study. This therefore rules out the possibility of cross-ethnic contact or other interventions as intermediary factors inducing perspective-taking among bilinguals. Put simply, perspective-taking may be viewed as a cognitive ability that can be activated even in the absence of intergroup contact or other perspective-taking stimuli.

To summarize the theoretical arguments, the primary expectation of this article is that individuals who received bilingual instruction in schools should display less political discrimination against ethnic outgroups than those who received monolingual instruction. A prolonged exposure to bilingual education increases the likelihood that an individual develops into a bilingual speaker who is more adept at seeing things from other individuals' vantage points. The resulting improvement in perspective-taking ability should in turn reduce bilinguals' propensity to discriminate against ethnic outgroups.

Case Selection: Bilingual Instruction in Malaysia

I test my claims by exploiting the exogenous introduction of an education reform in Malaysia. There are several reasons why Malaysia is an ideal case for analyzing the effect of bilingual instruction on ethnic outgroup discrimination. First, Malaysia is a multi-ethnic country where ethnicity is a salient political issue (Weiss, 1999). It comprises three main ethnic groups: Malays and other indigenous peoples (67.4%), Chinese (24.6%), and Indians (7.3%). Since its independence in 1957, the Prime Minister has always come from the majority Malay ethnic group. Political parties are also organized primarily by ethnic heritage. In addition, the constitution contains provisions that grant a "special position" to Malays while the rights of other ethnic minorities are marginalized. The introduction of the New Economic Policy in 1971 further exacerbated the unequal treatment of ethnic minorities through the use of pro-Malay quotas for admission into public universities, civil service employment and business licenses. These policies continue to garner widespread support within the majority Malay population. According to one survey, 59% of Malay respondents agreed or strongly agreed that Malays should continue to enjoy special rights and privileges because they are

the original inhabitants of the country (Merdeka Center for Opinion Research, 2010). Hence, Malaysia provides an interesting test case to examine whether the reform had any impact in promoting more inclusive political attitudes among members of an ethnic majority group.

The second reason relates to the nature of the education reform in Malaysia. Given that the second language was only used to deliver STEM (i.e., Science, Technology, Engineering, and Math) content, it seems reasonable to expect that the amount of second language immersion in Malaysian schools should be lower than, for instance, the use of the second language to teach humanities subjects such as History or Geography. This can have downstream implications on our ability to detect any discernible differences in the level of second language proficiency, perspective-taking ability and outgroup political prejudice between students who were affected by the reform and those who were not. However, if we can still observe significant effects in the Malaysia case, then it stands to reason that these effects should be considerably larger in other contexts where bilingual education is practiced in disciplines that require a more intensive use of the second language. Put simply, the Malaysia case represents a hard empirical test of my theoretical expectations.

Third, the Malaysia case provides a unique natural experimental setting that helps avoid several empirical challenges for causal inference. This is important because it is difficult to test the proposed hypothesis empirically. For one, the type of language of instruction received may be confounded by other pretreatment factors, such as parents' ethnic attitudes. For instance, individuals may self-select into monolingual schools because their parents do not want them to learn a second language that is not native to their own ethnic group (Huddy and Sears, 1995). Hence, ethnic outgroup discrimination among individuals who received monolingual instruction may simply be a reflection of their parents' outgroup biases. Moreover, analyzing the political effects of bilingual education through an experimental framework may pose ethical concerns for participants, as the type of language of instruction used in schools can have significant future repercussions on the development of human capital and labor outcomes (Laitin and Ramachandran, 2016).

Education Reform in Malaysia

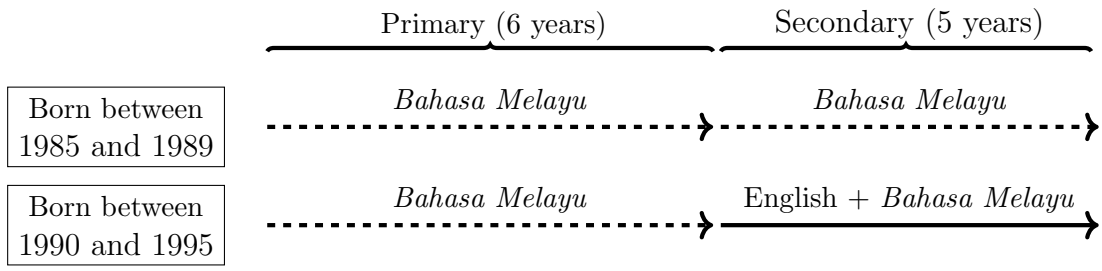
Concerned about the general lack of English literacy among ethnic Malays, the then Prime Minister Mahathir Mohamad announced on May 6, 2002, that Math and Science would be taught in English in all public primary and secondary schools starting 2003.[4] As part of the nationwide reform, students who were beginning their form one education (or the equivalent of grade seven in the US) in 2003 and subsequent cohorts received English instruction for all STEM subjects during their five-year secondary school education. This meant that Malay students who were affected by the reform were exposed to bilingual instruction during their secondary school education – i.e., Math and Science were taught in English while non-STEM classes were delivered in *Bahasa Melayu*. Under the new reform, about 40% of the teaching time was dedicated to English instruction, with the remaining 60% being taught in *Bahasa Melayu* (Gill, 2013). In contrast, the reform did not apply to secondary school students who had already advanced to form two or higher in 2003; hence these students continued to receive *Bahasa Melayu* instruction for all subjects including Math and Science – i.e., monolingual instruction.

Given that children in Malaysia typically begin their formal education at the age of seven as well as the fact that the education system is structured on a $6+5+2$ model,[5] we can identify how the reform affected different birth year cohorts of ethnic Malay students (see Figure 1). The pioneer cohort of form one students who received bilingual instruction during their secondary school education were born in 1990 – i.e., they would have turned thirteen years old in 2003. In contrast, Malay students who were born in 1989 and earlier studied both STEM and non-STEM subjects in *Bahasa Melayu* – i.e., they would have advanced to form two or higher in 2003 thereby missing the cutoff to study STEM subjects in English. Given the narrow timeline between the announcement date (i.e., May 6, 2002) and the implemen-

[4]The rest of the article focuses on ethnic Malay secondary school students who were affected by the new policy. Detailed descriptions of the education system in Malaysia and the language reform can be found in Supplementary Information (SI) section SI1.

[5]Specifically, students in Malaysia generally experience six years of primary school education, five years of secondary school education, and two years of pre-university training.

Figure 1: **Language of Instruction in Malaysian Public Schools**



tation of the reform (i.e., January 2003), it is also unlikely that students manipulated the likelihood of receiving English or *Bahasa Melayu* instruction. Grade skipping and repetition are also uncommon in Malaysian schools. Finally, besides the language reform in 2003, there were no major curricular changes that occurred during this period. Taken together, these observations imply the arbitrariness of the cutoff and the quasi-randomized nature of the reform in assigning the type of language of instruction used in Malaysian schools.

In addition, there are several reasons to think that the reform was successful in improving second language proficiency among affected students in Malaysia. First, the reform substantially increased the number of teaching hours dedicated to instruction in the second language. While their predecessors' exposure to English was limited to just two hours per week, students who were affected by the reform spent more than 10 hours per week learning Math and Science in English (Gill, 2013). This also implies that affected Malay students experienced a higher rate of language switching between their mother tongue (through non-STEM classes) and English (through STEM classes) in the classroom. Second, the reform affected intermediate learners who had acquired a basic level of English literacy since English is a compulsory subject from grades one to six, thereby making the transition from monolingual to bilingual instruction less onerous.[6] Third, the Malaysian government put in place additional measures to ensure that teachers were properly trained to deliver STEM classes in English. These include the development of a national re-training program for STEM teachers, a buddy support program that matched STEM teachers with their language counterparts, and the supply of

[6]However, it must be emphasized that students did not spend a significant amount of time learning English at the primary school level since a majority of the classes were still delivered in their native tongue.

self-teaching materials for instructors to facilitate their own learning (Gill, 2013).

Survey Design & Empirical Strategy

An IRB-approved, pre-registered survey comprising more than 2,000 participants was fielded in March 2022.[7] The survey was administered in *Bahasa Melayu* to ensure consistency in the interpretation of the questions (Pérez, 2017) and to eliminate any unintended language effects that might contaminate the empirical findings (see Pérez and Tavits, 2019*a*,*b*).

The sampling frame of the survey comprises individuals who (1) are ethnic Malays, (2) had completed their secondary school graduation examination (i.e., *Sijil Pelajaran Malaysia*, SPM), and (3) were born between 1985 and 1995. On the first requirement, I exclude other ethnic minorities, given that opinions about outgroup political discrimination may vary significantly across different ethnic groups. For instance, ethnic Malays may display a preference for maintaining their privileged position in Malaysian society, while members of other ethnic groups may favor a more equitable relationship. In addition, ethnic Malays tend to enroll into public secondary schools that were not immune from the reform in 2003.[8] The second prerequisite – i.e., secondary school graduation – ensures that the main reason why a participant was exposed to bilingual or monolingual instruction in school can be explained by their birth year. This eliminates other confounding explanations, such as individuals dropping out of secondary school even though they were born after the cutoff. Lastly, I exclude ethnic Malays who were born after 1995 as they were affected by the reversal of the policy in 2012. Specifically, individuals who were born after 1995 did not receive the full five-year English instruction for STEM subjects at the secondary school level, unlike their peers who were born between 1990 and 1995.[9]

[7] The survey design was pre-registered on February 28, 2022. A copy of the pre-analysis plan is available here. SI section SI2a provides details on the survey design, variable measurements, and descriptive statistics.

[8] Only one percent of the survey respondents reported that they had attended private secondary schools. Responses from these participants were dropped from the empirical analyses.

[9] Beginning 2012, *Bahasa Melayu* became the language of instruction for all subjects including Math and Science in all public secondary schools. See SI section SI1b for a detailed description of the policy reversal in 2012.

Measuring Political Discrimination Against Ethnic Outgroups

I define ethnic outgroup political discrimination as an individual's propensity to deny members of ethnic outgroups to participate in a country's political process (see Sorens, 2010). Specifically, individual displays of political discrimination on the basis of ethnic identity may be manifested through a person's (1) choice of local political representative, (2) choice of the country's chief executive, (3) view on the political rights of non-coethnic members in the country, and (4) opinion regarding the role of ethnic identities in shaping interparty competition in the country. Questions were adapted from existing attitudinal surveys in Malaysia (e.g., Al Ramiah et al., 2017; Merdeka Center for Opinion Research, 2010).

The first aspect relates to an individual's preference in relation to the ethnicity of the political representative. Each respondent was presented with a pair of hypothetical politicians who are competing to be the Member of Parliament (MP) in the district (Table 1).[10] To mitigate the likelihood of eliciting socially desirable responses, the "Ethnicity" attribute was randomized between "Chinese" (non-coethnic) and "Malay" (coethnic) while the other features were not varied (Butler and Tavits, 2021).[11] Participants then rated the likelihood of voting for each candidate on a scale from 1 ("very unlikely") to 10 ("very likely"). The first outcome measure is *Coethnic MP*, which is the difference between a respondent's self-reported likelihood of voting for a Malay candidate and the likelihood of voting for a Chinese candidate. A larger value indicates that the Chinese politician is discriminated at a greater degree relative to the Malay politician.

The second dimension measures an individual's preference with regard to the ethnicity of the country's chief executive. *Coethnic PM* measures a Malay respondent's level of agreement (on a 4-point Likert scale) toward the statement, "The Prime Minister of Malaysia should always be a Malay." In spite of the fact that the sentence was strongly worded, more than 85%

[10] Malaysian MPs are elected in single-member districts using plurality rule.

[11] Table SI2b.3 shows that the impact of the reform, language of instruction used in schools, as well as various demographic variables such as gender and income are balanced across the two profiles.

Table 1: **Hypothetical Profiles of Political Candidates in Malaysia Survey**

	Hypothetical Candidate Profiles	
Attributes	A	B
Malaysian citizen	Yes	Yes
Age	53 years old	61 years old
Marital status	Married	Married
Number of children	3	1
Occupation	Civil servant	Business owner
Ethnicity	Chinese/Malay	Chinese/Malay
Gender	Female	Male
Highest education	Bachelor's Degree	Doctoral Degree

of the participants either agreed or strongly agreed with the statement, which underscores the salience of ethnicity in Malaysian politics and that a majority of Malays may have grown accustomed to the notion of having a coethnic member as the country's chief executive. Third, *Ethnic Rights* asked respondents whether they strongly disagreed, disagreed, agreed, or strongly agreed with the view that "People should be treated and given the same rights in Malaysia regardless of race or religion." The last aspect relates to participants' opinion about the ethnicized nature of party competition in Malaysia – i.e., *Ethnic Parties*. Respondents read the following statement: "There should be no race-based parties in Malaysia." A higher level of disagreement implies that the respondent supports the continued presence of ethnic parties to protect Malay interests. Responses for *Coethnic MP*, *Coethnic PM*, *Ethnic Rights*, and *Ethnic Parties* are recoded and normalized so that larger values reflect greater political discrimination against non-coethnic groups.

Measuring Mechanisms: English Proficiency & Perspective-Taking Ability

To measure second language proficiency, the survey asked participants the following question: "In your opinion, how well do you know English?" The variable *English Proficiency* is then coded on a 5-point Likert scale, ranging from "Do not know the language at all" to "Fluent." Although a more objective measure of English literacy would be to ask respondents to provide English test results from their secondary school exit examinations, it is possible that a large

proportion of respondents might not remember their scores as they had left school for more than a decade, thereby leading to more incorrect or missing responses.

Perspective-taking ability is measured using the perspective-taking sub-scale from the Interpersonal Reactivity Index (IRI, see Davis, 1983). Specifically, participants read a total of seven statements that measure their perspective-taking ability (e.g., "Before criticizing somebody, I try to imagine how I would feel if I were in their place") and answer on a 5-point Likert scale ranging from "Does not describe me well" to "Describes me very well." The responses are normalized and averaged to create a composite score of a respondent's *Perspective-Taking* ability (Cronbach's $\alpha = 0.67$). One benefit of the IRI is that the items can be easily administered in a survey setting, unlike other measures such as the Theory of Mind test (Baron-Cohen et al., 1985). The perspective-taking sub-scale from the IRI has also been shown to correlate strongly with other common measures of cognitive empathy, such as the Basic Empathy Scale (Jolliffe and Farrington, 2006).

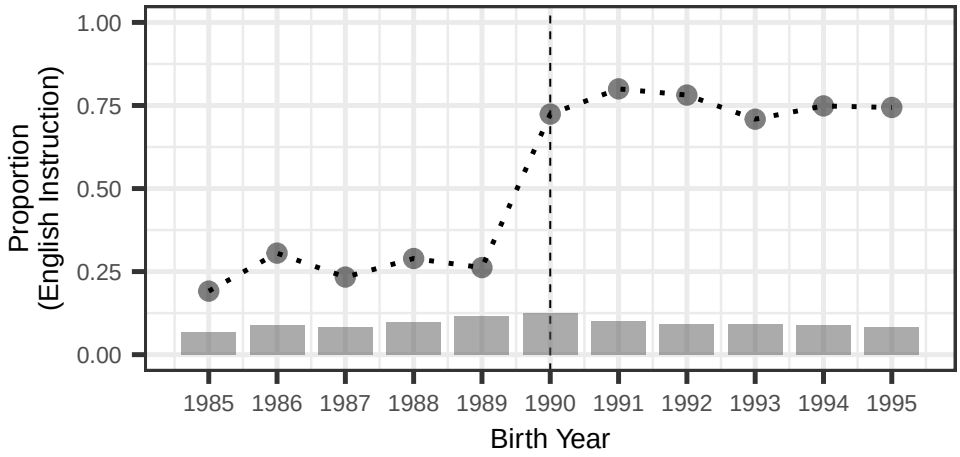
Pretreatment Covariates

The first pretreatment covariate is *Female*, which equals one for female respondents and zero otherwise. Next, *Urban School* equals one if a respondent attended an urban secondary school and zero otherwise. Finally, *Home Language* provides a measure of a respondent's language exposure at home. This is a categorical variable that consists of three mutually exclusive categories: (1) only speak *Bahasa Melayu* at home, (2) only speak English at home, and (3) speak more than one language at home. In particular, I distinguish between respondents from English and *Bahasa Melayu* monolingual families given that the former may possess more cosmopolitan outlooks, or may be exposed to a broader range of positive narratives (e.g., from overseas news outlets) about ethnic outgroups at home (Hu and Liu, 2020). Other demographic variables such as education and income were asked in the survey but are omitted from the baseline models because they are observed after the treatment and hence might induce posttreatment bias in the model estimates (Montgomery et al., 2018).

Empirical Strategy

Recall that the language of instruction for STEM subjects at the secondary school level should be English for Malays who were born between 1990 and 1995, and *Bahasa Melayu* for those who were born between 1985 and 1989. To verify if the reform coincided with participants' birth years, respondents indicated during the survey whether *English* or *Bahasa Melayu* was used to teach STEM subjects during their secondary school education. Figure 2 provides strong evidence that the reform should be regarded as an encouragement intervention, by increasing the probability that a respondent who was born between 1990 and 1995 received English instruction for STEM subjects at the secondary school level. I also provide additional evidence in the Supplementary Information (SI) that the reform significantly predicts the type of language of instruction used in Malaysian schools through both linear and logit regression models (Table SI2c.4).

Figure 2: **Respondents' Birth Year & Language of Instruction for STEM Subjects**



Notes: The vertical bars reflect the frequency distribution of the respondents by their birth years. Each point refers to the proportion of individuals in a cohort who reported that English was used to teach STEM subjects at the secondary school level. The vertical dashed line refers to the cutoff year at 1990.

In light of Figure 2, I implement a set of instrumental variables regression models to estimate the local average treatment effect (LATE) of bilingual instruction on ethnic outgroup attitudes. *Reform* is an instrument that equals one if a respondent's birthdate falls between 1990 and 1995, and zero otherwise. The main predictor variable is *Bilingual In-*

struction, which equals one if the respondent reported that English was used to teach STEM subjects during their secondary school education, and zero otherwise. Note that individuals who received English instruction for STEM subjects also received *Bahasa Melayu* instruction for non-STEM subjects – hence bilingual instruction. In contrast, those who reported *Bahasa Melayu* instruction for STEM classes received the same language of instruction for non-STEM subjects as well – hence monolingual instruction. Formally,

$$\text{Bilingual Instruction}_i = \alpha + \gamma \text{Reform}_i + \psi Z_i + \theta_i + \epsilon_i$$

$$Y_i = \zeta + \bar{\beta} \widehat{\text{Bilingual Instruction}}_i + \delta Z_i + \theta_i + \mu_i$$

where Y_i is the outcome measure of ethnic outgroup political discrimination, Z_i is a vector of pretreatment covariates, θ_i corresponds to birth year fixed effects, and $\bar{\beta}$ is the LATE. Note that the LATE is defined as the average treatment effect for the subset of compliers who were born between 1985 and 1995. Specifically, the compliers include (1) those who complied with the reform and received English instruction, and (2) those who were unaffected by the reform and received *Bahasa Melayu* instruction. The LATE can therefore be interpreted as the difference in mean outcomes between (1) and (2). This identification strategy is feasible, given the quasi-randomized nature of the reform in assigning the type of instructional language in Malaysian schools.

The above empirical strategy is valid insofar as three additional assumptions are satisfied. First, both Figure 2 and Table SI2c.4 demonstrate that the reform significantly increased the probability of receiving bilingual instruction among individuals who were born during or after the cutoff year of 1990 (relevance). Second, the reform only affected ethnic discrimination through its effect on *Bilingual Instruction* (exclusion restriction). This assumption is plausible as it is hard to imagine how a person's birthdate can have downstream effects on ethnic attitudes without working through the language of instruction channel. Lastly, it is unlikely that individuals would make a conscious effort to do the exact opposite of what they were supposed to do before or after the reform ("no-defiers" assumption).

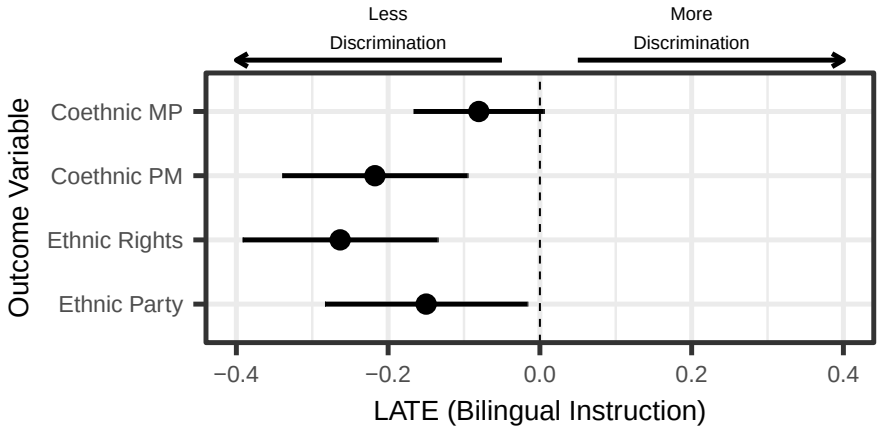
Results

Figure 3 reports the effects of bilingual instruction on ethnic outgroup political discrimination.[12] The results from Figure 3 provide support for the paper's main prediction: bilingual education reduces political discrimination against ethnic outgroups. Except for *Coethnic MP* (which is still significant at the 90% level), the point estimates for *Bilingual Instruction* are negative and statistically significant at the 95% level for three out of the four measures of ethnic outgroup political discrimination. Specifically, bilingual instruction is associated with lower levels of support for a coethnic Prime Minister (*Coethnic PM*), unequal political rights between different ethnic groups in Malaysia (*Ethnic Rights*), and the presence of ethnic parties (*Ethnic Parties*). The estimated effect sizes are also substantively meaningful, with an average LATE of 0.2 for each of the three outcome measures that are significant at the 95% level. To put this into context, recall that *Coethnic PM*, *Ethnic Rights* and *Ethnic Party* were measured on a 4-point Likert scale and rescaled to unit length. As such, compared to those who were taught solely in their native language, respondents who received bilingual instruction register, on average, a 0.8 point less political discrimination against ethnic outgroups on a 4-point Likert scale (i.e., $0.2 \times 4 = 0.8$).

Overall, the results from Figure 3 illustrate the enduring effects of bilingual education in bringing about more inclusive political attitudes among affected students in Malaysia. The fact that we can detect significant and substantial differences in ethnic outgroup attitudes among participants who had left school for more than a decade is remarkable. This further validates the belief that intergroup prejudice can be reliably and durably reduced during a person's formative years. In addition, the findings demonstrate that bilingual instruction is linked to less outgroup discrimination across a variety of political outcomes, from the

[12] I also report findings from another set of models that use a fuzzy regression discontinuity (RD) approach in Tables SI3a.6 and SI3a.7. In general, I observe mixed results. This may reflect a sense of frustration within the inaugural cohort of Malay students who had to adapt to the new requirements under the reform, which in turn might have induced some level of prejudice against ethnic outgroups *at the cutoff*. Given our interest in the overall – and not the immediate – effect of bilingual instruction on outgroup discrimination, the rest of the main text presents results from the instrumental variables regression models as specified previously.

Figure 3: **Bilingual Instruction Reduces Ethnic Outgroup Political Discrimination**



Notes: The plot illustrates the LATE of *Bilingual Instruction* on the four measures of ethnic outgroup political discrimination respectively. Each horizontal line is the 95% confidence interval of the point estimate (robust standard errors). The values for each of the four outcome measures range between zero and one, with larger values reflecting more outgroup discrimination. The pretreatment covariates are *Female*, *Home Language*, and *Urban School*. Full results are reported in Table SI3a.5.

electoral viability of non-coethnic political candidates to the political rights of non-coethnic citizens in the country. Simply put, Malay respondents who received bilingual instruction tend to possess an *unconditional* view that ethnic outgroup members should enjoy the same level of treatment as coethnic members across all aspects of their political lives.

Bilingual Instruction Improves English Literacy & Perspective-Taking Ability

Next, I provide suggestive evidence that the patterns observed above likely stem from improvements in second language literacy and perspective-taking ability among individuals who received bilingual education. One testable implication from the theory is that individuals who received bilingual instruction should report higher levels of second language proficiency than those who received monolingual instruction. A second expectation is that bilingual instruction promotes the development of perspective-taking ability among affected students.

In addition, I investigate the possibility that the positive effect of bilingual instruction on perspective-taking ability should be larger for individuals who were raised in monolingual

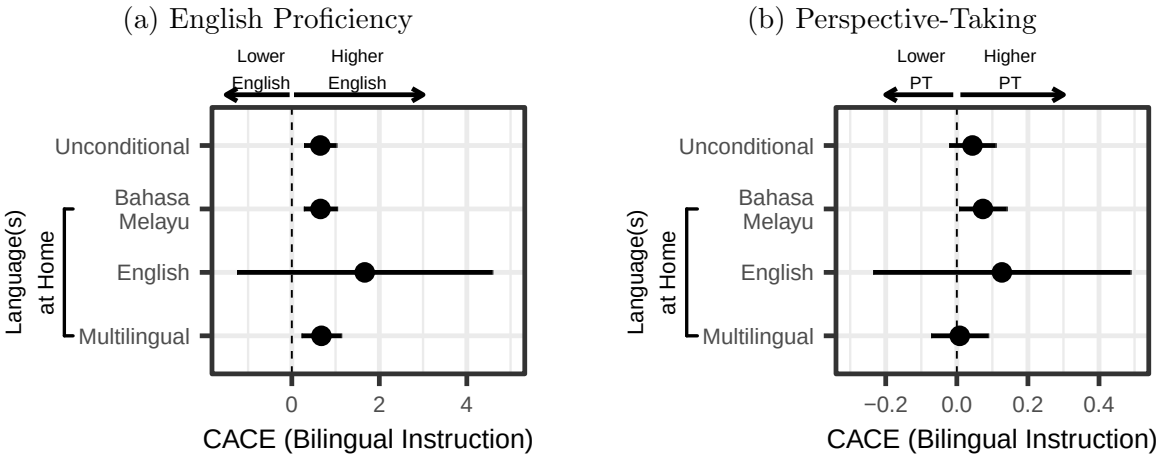
homes than those from multilingual families. If bilingualism is responsible for the development of perspective-taking ability, then there should not be any discernible change in perspective-taking capacity for those from multilingual families. For these individuals, linguistic diversity at home implies a higher rate of language switching, thereby facilitating the cognitive development of perspective-taking. Hence, the effect of bilingual instruction should be smaller for them. In contrast, while individuals from monolingual backgrounds might not receive second language training at home, bilingual instruction offers a compensatory avenue for this group of students by exposing them to two different languages in the classroom while cultivating their perspective-taking ability. Hence, the impact of bilingual instruction should be substantially larger for affected individuals who were raised in monolingual homes.

I test the first two implications by replacing the outcome variable with *English Proficiency* and *Perspective-Taking* in the *unconditional* instrumental variables regression models.[13] I also estimate a separate set of *conditional* models where *Bilingual Instruction* is interacted with *Home Language* to identify the marginal effects of bilingual instruction, conditional on the type and diversity of languages spoken at home. I report the unconditional and conditional LATEs of bilingual instruction on English literacy and perspective-taking ability in Figures 4a and 4b respectively. In line with the first expectation, Figure 4a shows that respondents who were exogenously assigned to bilingual instruction report higher levels of English proficiency. In addition, the provision of bilingual instruction had sizeable effects on self-reported English proficiency (mean = 3.87, sd = 0.91), amounting to a 0.72 standard deviation improvement among affected students.

In contrast, the unconditional impact of *Bilingual Instruction* on perspective-taking ability fails to achieve conventional levels of statistical significance (Figure 4b). This finding could reflect heterogeneity of treatment effects between participants who come from monolingual and multilingual families. Indeed, Figure 4b also shows that there is a significant and

[13]Note that the assumptions should hold even when the outcome variables are changed to *English Proficiency* and *Perspective-Taking*. For instance, it is unlikely that the reform predicts both outcomes through alternative channels other than the language of instruction used in schools (exclusion restriction), or that respondents who were supposed to receive English instruction chose to do the opposite ("no-defiers").

Figure 4: **Unconditional & Conditional Marginal Effects of Bilingual Instruction on English Proficiency & Perspective-Taking**



Notes: The plots show the LATE of *Bilingual Instruction* on *English Proficiency* and *Perspective-Taking*. Each horizontal line is the 95% confidence interval of the point estimate (robust standard errors). The unconditional models estimate the effects of *Bilingual Instruction* without the interaction term *Bilingual Instruction* × *Home Language*. *English Proficiency* is measured on a 5-point scale, where larger values reflect better English fluency. The values for *Perspective-Taking* range between 0 and 1. Larger values reflect better perspective-taking ability. The pretreatment variables are *Female*, *Multilingual Home*, and *Urban School*. Full results are reported in Table SI3b.8.

positive association between bilingual instruction and perspective-taking ability for individuals who only speak *Bahasa Melayu* at home. Meanwhile, this effect is statistically indistinguishable from zero for those from multilingual families. Interestingly, bilingual instruction fails to predict perspective-taking ability among English-speaking monolinguals. Notwithstanding, it should be noted that these individuals would still be exposed to two languages during their formative years even if they received monolingual instruction in school – that is, *Bahasa Melayu* in school and English at home. Hence, the effect of bilingual instruction on perspective-taking ability for this subgroup should be negligible. Overall, the null effect observed among respondents from multilingual families may have cancelled out the larger effect observed among individuals from monolingual families, thereby contributing to the null finding at the aggregate level.

As such, the results from Figure 4 are consistent with the argument that bilingual instruction mitigates ethnic outgroup discrimination by improving second language proficiency

and perspective-taking ability, though they do not represent direct evidence that this relationship is mediated by the two mechanisms. Furthermore, the findings from Figure 4b are consistent with the conjecture that bilingual education may compensate for the lack of perspective-taking training in monolingual families. However, these observations should be interpreted as suggestive, since an ideal test should take the form of a longitudinal study that tracks changes in perspective-taking ability before and after exposure to bilingual instruction, and whether the intervention has helped to narrow the perspective-taking gap between students who come from monolingual and multilingual families (see Neundorf et al., 2016).

Additional Tests

In this section, I conduct additional analyses to verify the main findings. Unless specified otherwise, detailed results of the robustness tests are documented in section SI4.

Profiling Compliers & Non-compliers in the Malaysia Case

How far can we generalize the main results to the population of ethnic Malays who were born between 1985 and 1995? Indeed, the empirical finding that bilingual instruction promotes inclusive political attitudes only apply to a (non-representative) subset of Malay respondents who complied with the assigned treatment – that is, those who received bilingual instruction in school as a direct result of the education reform and those who received monolingual instruction because they were born before the cutoff.

Following Marbach and Hangartner (2020), I present the profiles of compliers and non-compliers from the Malaysia survey based on three pretreatment covariates – *Female*, *Home Language*, and *Urban School* (Figure SI4a.2). Accordingly, 49% of the respondents are identified as compliers, 25% are never-takers (i.e., those who received monolingual instruction regardless of the reform), and 26% are always-takers (i.e., those who received bilingual instruction irrespective of the reform). With regard to the pretreatment covariates, compliers

tend to be females, are more likely to come from families that only speak *Bahasa Melayu* at home, and are more likely to have attended a rural secondary school. As such, while the main findings may not be generalizable to the entire population, the results from Figure SI4a.2 suggest that the reform primarily affected the subpopulation of ethnic Malay compliers who were *ex ante* least likely to have any second language exposure at home and/or receive any proper second language training in school. This speaks to the significance of the paper's main results, as they reflect changes in political attitudes among ethnic Malay participants who stood a high chance of remaining as monolinguals – and hence might display more ethnic outgroup political discrimination later in their lives – if not for the reform in 2003.

Robustness Checks

I perform a number of robustness tests to verify the main results and the exclusion restriction assumption underlying the instrumental variables regression models. First, to improve the "representativeness" of the findings, I compute sampling weights based on the 2010 Malaysian census data. Similar results are obtained even after including the sampling weights into the estimation models (Table SI4b.9). Next, I re-estimate the p-values of the coefficient estimates using robust standard errors clustered by respondents' birth years. The statistical significance of the point estimates remain unchanged (Table SI4b.10).

I also examine the possibility that the reform affected outgroup discrimination through alternative channels other than the language of instruction used in schools (exclusion restriction). One possible violation is that the switch from *Bahasa Melayu* to English instruction for STEM subjects might have persuaded more ethnic Chinese parents to admit their children into public secondary schools.[14] This in turn might facilitate greater cross-ethnic contact between Malay and Chinese students, thereby directly contributing to less intergroup prejudice. Another possible violation is that the reform might have induced policy resentment among Malays who were forced to undergo bilingual instruction in schools. Put differently,

[14]It should be noted that ethnic Chinese parents in Malaysia generally prefer to send their children to Chinese independent high schools, given that these schools offer a Mandarin-based curriculum.

it is not the experience of bilingual instruction that motivates discontent toward the second language; rather, resentment may stem from a sense of injustice that previous cohorts of students, as well as ethnic Chinese students from private secondary schools, were exempted from the reform in 2003. This sense of dissatisfaction toward learning and speaking English might in turn influence an individual's inclination to discriminate against ethnic outgroups.

To assess the viability of these two alternative mechanisms, the survey asked participants to indicate the frequency in which they communicate with members of a different ethnicity, the quality of their cross-ethnic interactions, and their opinions regarding the use of English in their daily lives (e.g., "Malaysians place too much emphasis on learning and speaking English"). The results show that none of the alternative channels are predicted by the reform, thus mitigating the plausibility of the two alternative mechanisms (Table SI4c.11).

Alternative Explanations

I consider three potential alternative explanations for the main findings. The first relates to the bundle of posttreatment measures that were omitted from the baseline models, such as education and employment status. For instance, exposure to English instruction in schools expands the range of higher education options and employment opportunities among affected Malay individuals. It is therefore possible that these socioeconomic mechanisms may mediate the extent in which bilingual instruction affects ethnic outgroup discrimination. I account for this concern by including a broader set of social and economic controls in the models. The same key findings are obtained after accounting for respondents' highest education qualification, marital status, income, and employment situation (Table SI4d.12).

Another alternative explanation is that bilingual instruction may facilitate cross-ethnic contact by providing a common linguistic medium for members of different ethnic groups to communicate with each other, thereby resulting in more positive evaluations of outgroup members (Wright and Tropp, 2005). It is also possible that these cross-ethnic interactions may induce greater perspective-taking among individuals who were affected by the education

reform. To assuage these concerns, I regress the two measures of cross-ethnic contact – as mentioned previously – on *Bilingual Instruction*, with *Reform* as the instrumental variable. I find that bilingual instruction fails to predict both measures of cross-ethnic contact (Table SI4e.13), thereby suggesting that the relationship between bilingual education and ethnic outgroup political discrimination should not be mediated by the frequency or quality of intergroup contact between Malays and other minority groups in Malaysia.

Lastly, another interpretation of the findings is that the observed patterns may be a consequence of using English as a language of instruction in schools, as opposed to the more general expectation that bilingual instruction promotes politically inclusive ethnic attitudes. Specifically, the results may be a reflection of the suggestion that English literacy is associated with more cosmopolitan (ergo more tolerant) attitudes, given its status as a lingua franca in many multicultural and global settings (Hu and Liu, 2020).

I consider two additional – albeit imperfect – tests to address this concern. If the results were primarily influenced by the effect of English instruction as opposed to bilingual instruction, then we could make the parallel argument that English-speaking bilinguals should exhibit less political discrimination against ethnic outgroups than non-English-speaking bilinguals. Table SI4f.14 reports the results from a series of models where the outcome measures of ethnic discrimination are regressed on a binary indicator of whether a respondent speaks English among a subset of bilingual speakers.[15] Accordingly, differences in ethnic discrimination cannot be explained by whether the second language spoken by a bilingual respondent is English or not. This observation is also consistent with the theoretical claim that there should not be any meaningful difference in outgroup attitudes between bilinguals who, for instance, speak an ethnic minority language and those who are fluent in a colonial tongue as a second language.

Next, I compare between bilinguals and monolinguals within a subset of non-English-speaking respondents to ascertain whether the effects of bilingualism hold even if the second

[15]The survey also asked participants to list the languages that they speak outside their homes. As such, I measure bilingualism based on whether a respondent speaks more than one language at home or outside.

language spoken by a bilingual respondent is not English. Most of the coefficient estimates are negatively signed, albeit only one is significant at the 95% level (Table SI4f.15). This provides some evidence that bilingualism – regardless of whether the second language is English or not – is related to less ethnic outgroup discrimination. Admittedly, the two tests are not ideal, especially in light of the fact that the proportion of non-English-speaking ethnic Malay bilinguals in the sample is considerably small.

Generalizability of Findings

Before concluding, it is useful to consider the generalizability of the findings beyond the Malaysia case. Recall that the theoretical link between bilingual instruction and inclusive attitudes stems from the primary claim that bilinguals are better perspective-takers and hence should display less outgroup discrimination than monolingual speakers. As such, this expectation should also apply to (1) a broader set of bilinguals who do not speak *Bahasa Melayu* or English, and (2) a wider set of social outgroups such as immigrants.

To test these implications, I conduct a cross-national analysis using the latest wave of the Asian Barometer Survey (ABS, 2017), which spans over 15,000 participants from more than 10 East and Southeast Asian countries including China and Indonesia. Unlike most global and regional surveys such as the World Values Survey, one benefit of analyzing the ABS is that the survey asked respondents whether they speak only local language, only official language, or a mixture of local and official languages at home.[16] This allows for the construction of the main predictor variable in the cross-national analysis – i.e., *Bilingualism*, which equals one if a respondent speaks a mixture of local and official languages at home, and zero otherwise. Another benefit is that the region is home to a large proportion of bilinguals for whom English is neither their first nor second language. It is estimated that less than 10 percent of the population in the 14 ABS countries speak English as their first or second language (Eberhard et al., 2022). Indeed, one lingering concern from the previous

[16]Indeed, the latest wave of the World Values Survey (Haerpfer et al., 2022) specifically requests interviewers to only code one answer to the question "What language do you normally speak at home?"

analyses relates to whether the main results were driven by the effect of bilingual or English instruction. As such, the ABS analysis provides a more robust test of the expectation that bilinguals should exhibit less outgroup prejudice regardless of the type of languages spoken.

I rely on two items to measure outgroup attitudes. The first is *Ethnic Inequality*, which asked whether respondents strongly agreed (1), agreed (2), disagreed (3), or strongly disagreed (4) with the view that "all citizens from different ethnic communities in your country are treated equally by the government." I expect that bilinguals should display stronger disagreement toward this statement, given their heightened sensitivity toward the perspectives of other ethnic outgroups in their country. The second is *Anti-Immigrant*, a 4-point ordinal variable measuring respondents' opinions on whether the government should increase or decrease the flow of immigrants into their country. Larger values reflect stronger anti-immigrant sentiments. Accordingly, bilinguals should exhibit less anti-immigrant tendencies than monolinguals.

Table 2: **Bilinguals Display Less Outgroup Discrimination**

DV =	Ethnic Inequality		Anti-Immigrant	
	All Respondents	Exclude English-Speaking Countries	All Respondents	Exclude English-Speaking Countries
Bilingualism	0.049^{*}	0.048^{*}	-0.074^{**}	$-0.055^{\dagger}$
	(0.022)	(0.023)	(0.026)	(0.031)
N	12,726	9,616	10,586	7,516

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors are displayed in parentheses. The values for *Ethnic Inequality* and *Anti-Immigrant* range between 1 and 4. The larger the *Ethnic Inequality*, the stronger the respondent feels that different ethnic groups are treated unfairly by the government. In contrast, the larger the *Anti-Immigrant*, the weaker the respondent discriminates against immigrants in their country. English-speaking countries refer to countries where a substantial proportion of their residents speak English as their first or second language. These are Hong Kong, Malaysia, Singapore, and Philippines. Control variables include education, gender, age, marital status, income, whether the respondent lives in an urban or rural area, and whether the respondent identifies with a religion. Country fixed effects are also included in the models. Full results are reported in Table SI4g.16. $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\dagger}p < 0.1$.

Table 2 presents the results from a series of linear models where *Ethnic Inequality* and *Anti-Immigrant* are regressed on *Bilingualism* and a set of socioeconomic controls such as education and income, as well as country fixed effects. Consistent with my theory, bilingual

respondents tend to perceive that some ethnic communities are treated unfairly by their government in their country. They also display less xenophobic sentiments than monolinguals. Similar findings are observed even when respondents from English-speaking countries – i.e., where a substantial proportion of their citizens speak English as the first or second language (i.e., Hong Kong, Malaysia, Singapore, and Philippines) – are excluded from the analysis.

Conclusion

In this article, I show that differences in ethnic outgroup attitudes can be traced to an individual's language training in school. Specifically, I demonstrate the causal effects of bilingual education on ethnic discrimination by leveraging the as-if random assignment of the language of instruction in Malaysian schools due to the sudden introduction of an education reform in 2003. I find that ethnic Malays who received bilingual instruction displayed less political discrimination against ethnic outgroups. The empirical analyses also provide suggestive evidence that this relationship might be mediated by increases in second language proficiency and perspective-taking ability among affected students. In particular, the findings allude to the possibility of bilingual education as a compensatory avenue through which the initial perspective-taking gap between individuals who come from monolingual and bilingual families may be bridged. When examining the profile of compliers, I show that the reform mainly targeted the subgroup of Malays who were *ex ante* least likely to receive adequate second language training at home or at school, and hence would benefit immensely from the reform in terms of their second language literacy and perspective-taking ability. I also rule out alternative explanations such as cross-ethnic contact and whether the findings stemmed from the effect of English instruction. Finally, I triangulate the results from the Malaysia case with cross-national evidence from the ABS, and find preliminary support for the link between bilingualism and inclusive political attitudes across a broader set of non-English-speaking bilinguals and social outgroups such as immigrants.

The findings from this paper make several important contributions. First, this study

expands the traditional understanding that education influences political attitudes and behavior primarily through the acquisition of civic skills and political knowledge. I demonstrate that education also improves our cognitive ability to assume the vantage points of ethnic outgroup members in our midst. More broadly, the findings indicate that a person's education represents a bundle of experiences inside and outside the classroom, from the language of instruction used to the types of extracurricular activities offered. Hence, unpacking this bundle of experiences – through a rigorous analysis of other facets of the education system – represents an important step for future scholars to resolve some of the theoretical issues that remain unsolved in the education literature (Cavaille and Marshall, 2019; Persson, 2015).

Second, this article contributes to the more general understanding that the language(s) we speak can influence our social and political outlooks. The finding that bilingual instruction promotes the development of perspective-taking ability and lowers intergroup prejudice in Malaysia speaks to recent calls for a wider investigation of language's impacts on affective, cognitive, and behavioral political outcomes (Pérez and Tavits, 2017). To my knowledge, this research is also one of the first that uncovers an empirical connection between bilingualism and outgroup tolerance at a cross-national level. Admittedly, the statistical tests are not perfect, primarily because we lack a good empirical measure of a person's linguistic diversity at home and/or outside. As such, future iterations of cross-national surveys should allow respondents to freely specify the language(s) they speak at home and outside, thereby enabling future researchers to test this phenomenon more robustly.

Finally, the results also bear lessons for what types of policy interventions might have long-lasting impacts on prejudice reduction. The fact that this study was able to detect significant differences in perspective-taking ability and ethnic attitudes among Malays who had left school for more than a decade underscores the persistent influence of pre-adult factors in shaping a person's later-life political attitudes. Hence, a more durable solution to the problem of intergroup prejudice may not lie in inducing one-off perspective-taking by reading about marginalized groups, but in cultivating perspective-taking ability through

early life interventions such as bilingual education. Future works should devote attention toward investigating the efficacy of other agents of political socialization and how these inclusionary effects might also apply to other ostracized groups such as the LGBT community.

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Supplementary Information: Bilingual Education Reduces Ethnic Outgroup Discrimination Through Perspective-Taking

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Contents

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SI1 Case Description: Bilingual Instruction in Malaysia

SI1a Background Information: Malaysia's Education System

Since its independence in 1957, the Malaysian education landscape has remained deeply segregated. At the primary level, public schools are classified into two official types: (1) "national schools" (*Sekolah Kebangsaan*) that use *Bahasa Melayu* as their main medium of instruction, and (2) "national-type schools" (*Sekolah Jenis Kebangsaan*) that adopt either Mandarin or Tamil as the medium. While English and *Bahasa Melayu* are compulsory subjects for all primary school students, Math and Science were taught in accordance with the school's vernacular language before the 2003 reform – i.e., *Bahasa Melayu*, Mandarin, or Tamil. On the other hand, *Bahasa Melayu* was the sole language of instruction in all public secondary schools before 2003, though there also exists several Chinese independent high schools that are privately funded and that offer a Mandarin-based curriculum for their students. Similarly, English and *Bahasa Melayu* are compulsory subjects in both private and public secondary schools in Malaysia.

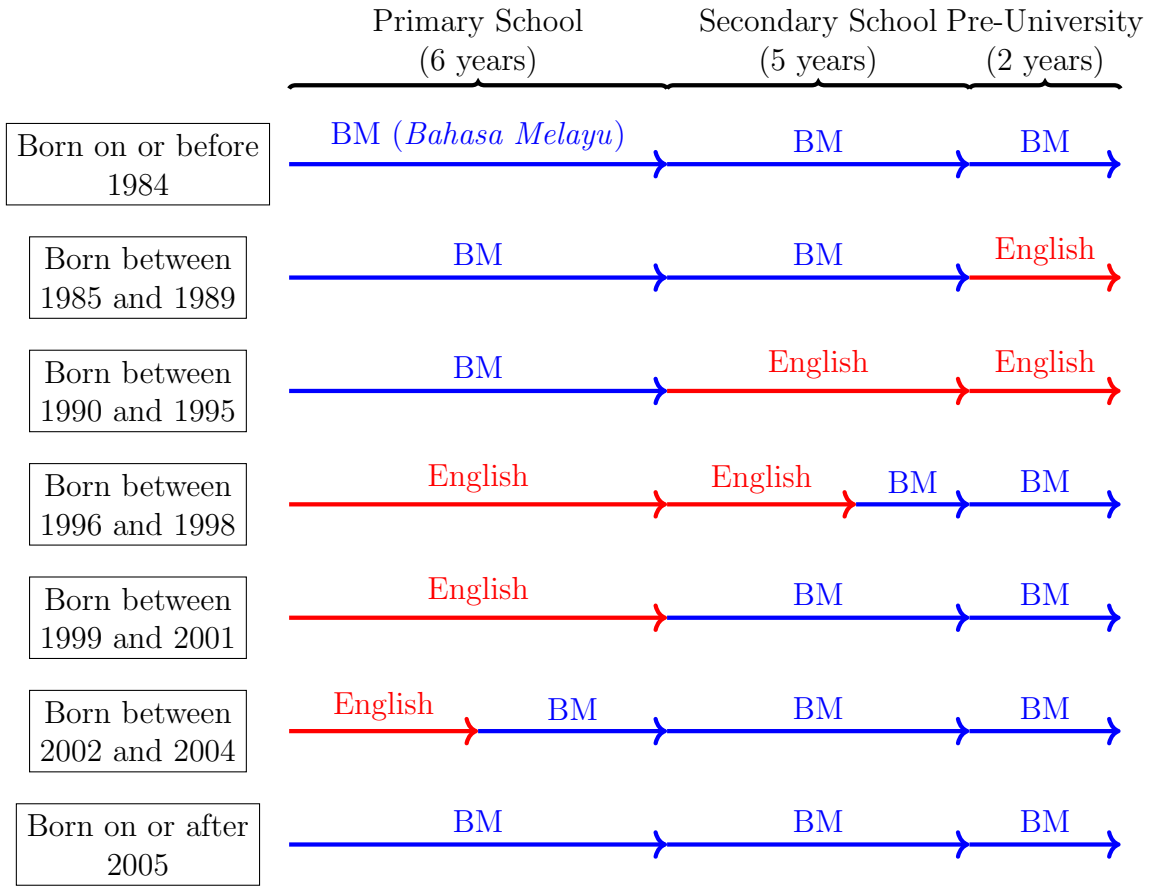
Unsurprisingly, parents in Malaysia typically enroll their children in schools that reflect their ethnic groupings. For instance, about 15 percent of the children enrolled in Chinese primary schools are Malay, while Indians comprise another 2.8 percent. Indian students make up 99 percent of all students in Tamil vernacular primary schools. Finally, less than one percent of the students in national primary schools – i.e., *Bahasa Melayu*-medium schools – are ethnic Chinese (The Straits Times, 2020). Although official figures for the racial composition of secondary school students are not available, our survey results indicate that less than one percent of the Malay respondents reported being enrolled in Chinese independent high schools. These figures represent a stark contrast to the country's ethnic composition: Malays and other indigenous peoples (67.4%), Chinese (24.6%), and Indians (7.3%).

SI1b Language Reforms in Malaysian Schools: 2003 and 2012

On May 6, 2002, then Prime Minister of Malaysia, Mahathir Mohamad, announced that English would replace the three vernacular languages as the sole medium of instruction for Math and Science in all public primary and secondary schools from 2003. This policy (known in *Bahasa Melayu* as *Pengajaran dan Pembelajaran Sains dan Matematik Dalam Bahasa Inggeris* or PPSMI) was implemented in a staggered fashion. Beginning 2003, Math and Science would be taught in English for all primary one, form one (i.e., grade seven in US schools), and lower form six (i.e., grade 12) students. For instance, primary two students in 2003 would continue their Math and Science training in their respective vernacular languages but would switch to English when they promote to form one in 2008. More importantly, this reform significantly increased the number of English-based teaching hours in schools. While their predecessors' exposure to English-medium classes was limited to just two hours per week, affected students spent at least 13 hours per week learning Math and Science in English, in addition to their English and vernacular language classes (Gill, 2013). Similarly, public secondary schools in Malaysia were required to implement the policy change from 2003, though it must be emphasized that Chinese independent high schools were exempted from PPSMI since their curriculum was distinct from that taught in public secondary schools.

However, amid mounting opposition from various sectors such as Chinese educationists and a coalition of Malay intellectuals under the banner of *Gerakan Mansuhkan PPSMI* (or Movement to Abolish PPSMI in *Bahasa Melayu*), the Malaysian cabinet decided in 2008 that PPSMI would be abolished. From 2012, the teaching of Math and Science would be reverted to the pre-2003 format: that is, both subjects would be taught in one of the three ethnic languages in primary schools – depending on the school's vernacular – whereas *Bahasa Melayu* would be used to teach both subjects at all public secondary schools. This policy reversal would also be implemented in stages. All primary one, primary four, form one, and form four students in 2012 would be affected by the policy reversal while the rest would continue with English as the medium of instruction for Math and Science until their promotion to one of the four grades. For example, form two students in 2012 would switch from English to *Bahasa Melayu* when they advance to form four in 2014.

Figure SI1b.1: **Language of Instruction for Math & Science Based on Birth Year Cohorts of Ethnic Malay Students**



Given that Malaysian children typically begin their primary education at the age of seven as well as the fact that the education system is structured on a $6 + 5 + 2$ model, we can identify how the introduction and subsequent reversal of the language reform affected different cohorts of ethnic Malay students. This is illustrated in Figure SI1b.1. For instance, the pioneer batch of primary one students who were affected by the 2003 language reform

would be born in 1996; however, this same group of students would have also experienced the policy reversal when they began their form four education in 2012, hence they received two years of *Bahasa Melayu* instruction for STEM (i.e., Science, Technology, Engineering, and Math) subjects during their secondary school education.

For simplicity, I exclude ethnic Chinese citizens from the empirical analysis given their preference to enroll into Chinese independent high schools which, as mentioned earlier, were exempted from PPSMI. Moreover, there is also the possibility that ethnic Chinese students may manipulate the likelihood of receiving English instruction by avoiding public secondary schools. Conversely, ethnic Malay students should have experienced the greatest shock from the language reform, as they generally enroll into public secondary schools that were not immune from the policy changes. In addition, there do not exist any alternative schools that offer only *Bahasa Melayu* instruction during the time when the policy change was introduced.

The following provides brief descriptions on the variation in the length of exposure to English instruction across different birth year cohorts of ethnic Malay students in Malaysia:

- *Born on or before 1984*: This group of students would have completed their pre-university training at the time when the policy reform was introduced in 2003. As such, *Bahasa Melayu* was the only medium of instruction used to teach all subjects for this group of citizens.

- *Born between 1985 and 1989*: Recall that the policy reform in 2003 affected lower form six (or grade 12) students in Malaysia. As such, the first cohort of students to experience English instruction at the pre-university level were those who were born in 1985 (i.e., they would be eighteen years old in 2003). On the other hand, those who were born in 1989 missed the cutoff to experience English-based education for STEM subjects during their secondary school years i.e., they would be form two students in 2003 hence continuing their Math and Science training in *Bahasa Melayu*. As such, this group should have the shortest exposure duration to English-based instruction, either because (1) they left school after completing their secondary education i.e., zero exposure, or (2) they advanced to the pre-university level and experienced one and a half years of English education. Put simply, students in this group should have experienced no more than 2 years of English education.

- *Born between 1990 and 1995*: This group of citizens would have been exposed to English-based instruction for STEM classes during their five-year secondary education. For those who were born in 1990, they just made the cutoff to experience the language reform at the secondary level i.e., they would be form one students in 2003. Some citizens, who continued their education into the pre-university level, would have experienced an additional two years' worth of English education i.e., a total of seven years. The same could be said about the cohort of students who were born in 1995; however, this latter group just missed the cutoff to experience Math and Science education in English at the primary level i.e., they would be primary two students in 2003. Taken together, this group of students should have received a minimum of five years of English instruction for STEM subjects (but not more than seven years).

- *Born between 1996 and 1998*: Those who were born in 1996 would be the first cohort of students to experience English as the medium of instruction at the primary level (i.e.,

a total of six years). This policy remained the same for the first three years of their secondary education until the policy reversal in 2012, when the medium was switched from English to *Bahasa Melayu*. Overall, they would have spent a total of 9 years of English-based instruction for STEM subjects.

- *Born between 1999 and 2001*: Similar to the previous group, this group experienced six years' worth of English education for Math and Science at the primary level. However, because of the policy reversal in 2012, those who were born in 1999 would receive *Bahasa Melayu* instruction for STEM subjects when they advance to form one in 2012. This also applies to students who were born in the years of 2000 and 2001.

- *Born between 2002 and 2004*: For this group of students, the language of instruction was switched from English to *Bahasa Melayu* midway through their primary education. Specifically, those who were born in 2002 would be year four (or grade four) students in 2012 and hence received *Bahasa Melayu* instruction for Math and Science. In total, they would have spent three years' worth of English education for Math and Science during their primary education (i.e., from year one to year three).

- *Born on or after 2005*: Those who were born in 2005 entered primary school in 2012 (i.e., they would be seven years old); hence the medium of instruction for this group of students would be *Bahasa Melayu* throughout their entire education experience.

SI2 Survey Design & Empirical Strategy

SI2a Survey Design, Variables & Descriptive Statistics

Survey participants were recruited by Rakuten Insight through their consumer panel. Participants were then redirected to an online survey that was hosted at Qualtrics. In addition, the survey implemented the following procedures that are in line with the guidelines and rules set out in the APSA Ethics Guide and Guidance for Human Subjects Research, as well as those outlined by the Institutional Review Board (IRB) from my home university:

- Guaranteeing that participation in any research activities is voluntary:
  - Each participant signed a consent form that emphasizes the voluntary nature of the activity. They were informed that their participation is voluntary, and that any refusal to participate will involve no penalty or loss of benefits.
  - Participants are also free to skip or refuse to answer any questions during the activity without incurring any loss of benefits.

- Guaranteeing fair compensation:
  - Participants were compensated for their efforts upon completion of the survey. The compensation rate was US$2. Given that the survey took an average of 10 minutes to complete, this translates to an hourly rate of US$10, which is

considerably higher than the average hourly rate among Malaysian workers in 2020 (i.e., about US$4.50 per hour).[1]

- Protecting the identities of participants:
  - The survey did not collect any information that can be traced to any individual respondent i.e., participants' names and their IP addresses.
- Mitigating any potential harm or damages to participants:
  - Any information shared by participants are stored securely in the author's password protected hard drive and only the author has access to the data
  - In addition, records of survey responses will not be shared with any government institutions, other researchers, and for-profit entities. Data will only be shared on a need-to-know basis (e.g., for replication purposes).

As mentioned in the main text, the sampling frame consisted of individuals who (1) are ethnic Malays, (2) had completed their secondary school exit examinations (i.e., *Sijil Pelajaran Malaysia*, SPM), and (3) were born between the years of 1985 and 1995 (both inclusive). The survey design and questions were pre-registered on February 28, 2022. All questions and answer choices were written in *Bahasa Melayu* to ensure consistency in the interpretation of the questions and choices. Tables SI2a.1 and SI2a.2 provide a description and the descriptive statistics of the variables used in the empirical analyses respectively.

Table SI2a.1: **Variable Description and Measurements**

Variables	Description and Measurements
Instrumental Variable	
Reform	0 = Born before January 1, 1990 1 = Born on or after January 1, 1990
Key Predictor	
Bilingual Instruction	0 = Received *Bahasa Melayu* instruction for Math and Science during secondary school education 1 = Received English instruction for Math and Science during secondary school education

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[1]The average hourly rate is based on the average salary in 2020 reported by the Department of Statistics Malaysia, which was RM2,933 (or US$680).

Table SI2a.1 – continued from previous page

Variables	Description and Measurements
Outcome Variables (larger values reflect more ethnic outgroup political discrimination)	
Coethnic MP	Normalized difference in self-reported likelihood of voting for a Malay candidate (from 1 to 10) and likelihood of voting for a Chinese candidate (from 1 to 10). Values range between 0 (i.e., non-coethnic favoritism) and 1 (i.e., coethnic favoritism).
Coethnic PM	Normalized response to the statement "The Prime Minister of Malaysia should always be a Malay." 1 = Strongly disagree 2 = Disagree 3 = Agree 4 = Strongly agree
Ethnic Rights	Normalized response to the statement "People should be treated and given the same rights in Malaysia regardless of race or religion." 1 = Strongly agree 2 = Agree 3 = Disagree 4 = Strongly disagree
Ethnic Party	Normalized response to the statement "There should be no race-based parties in Malaysia." 1 = Strongly agree 2 = Agree 3 = Disagree 4 = Strongly disagree
Pretreatment Covariates	
Female	0 = Male 1 = Female
Home Language	What language(s) do you normally speak at home? (categorical variable) 0 = Only speak *Bahasa Melayu* at home 1 = Only speak English at home 2 = Speak more than one language at home
Urban School	Where was your secondary school located at? 0 = Countryside 1 = City
Posttreatment Covariates	
Married	0 = Single, divorced, or others 1 = Married

Continued on next page

Table SI2a.1 – continued from previous page

Variables	Description and Measurements
Income	What is your monthly salary? 0 = Less than RM1,000 1 = RM1,000 - RM1,999 2 = RM2,000 - RM2,999 3 = RM3,000 - RM3,999 4 = RM4,000 - RM4,999 5 = RM5,000 - RM5,999 6 = RM6,000 - RM6,999 7 = RM7,000 - RM7,999 8 = RM8,000 - RM8,999 9 = RM9,000 - RM9,999 10 = RM10,000 and above
Education	1 = SPM or equivalent 2 = STPM or equivalent 3 = SKM, polytechnic/university certificate, or equivalent 4 = DKM, DLKM, polytechnic/university diploma, or equivalent 5 = Bachelor's degree 6 = Master's degree or Doctoral degree
Employed	0 = Student, homemaker, unemployed, or retired/pensioned 1 = Full time employee, part time employee, or self employed
Primary & Alternative Mechanisms	
Contact Quality	To what extent would you describe your experiences with members of a different race as positive? Values range from 1 ("Not at all positive") to 7 ("Very positive")
Contact Quantity	How often do you engage in informal conversations with members of a different race? 1 = Never 2 = At least once a year 3 = Once every few months 4 = At least once a month 5 = At least once a week 6 = Daily

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Table SI2a.1 – continued from previous page

Variables	Description and Measurements
Perspective-Taking	Average of normalized responses to seven items from the Interpersonal Reactivity Index's perspective-taking sub-scale (Davis, 1983), with each item measured on a 5-point Likert scale ranging from 1 ("Does not describe me well") to 5 ("Describes me very well"): • I sometimes find it difficult to see things from the "other guy's" point of view. (*reverse coded*) • I try to look at everybody's side of a disagreement before I make a decision. • I sometimes try to understand my friends better by imagining how things look from their perspective. • If I'm sure I'm right about something, I don't waste much time listening to other people's arguments. (*reverse coded*) • I believe that there are two sides to every question and try to look at them both. • When I'm upset at someone, I usually try to "put myself in his shoes" for a while. • Before criticizing somebody, I try to imagine how I would feel if I were in their place.
English Proficiency	In your opinion, how well do you know English? 1 = Do not know the language at all 2 = Can understand a little, but cannot speak 3 = Can understand and speak a little 4 = Can understand, speak, and write 5 = Fluent
Other Variables	
English Emphasis	Malaysians place too much emphasis on learning and speaking English. 1 = Strongly disagree 2 = Disagree 3 = Agree 4 = Strongly agree
English Disrespect	I feel disrespected when a fellow Malaysian speaks to me in English 1 = Strongly disagree 2 = Disagree 3 = Agree 4 = Strongly agree
English	0 = Does not speak English at home and outside 1 = Speak English at home and/or outside

Continued on next page

Table SI2a.1 – continued from previous page

Variables	Description and Measurements
Bilingual	0 = Speak only one language at home and outside 1 = Speak more than one language at home and/or outside

Table SI2a.2: **Descriptive Statistics of Variables**

	N	Mean	SD	Median	Min	Max	Skew
Instrumental Variable							
Reform	2071	0.56	0.50	1.00	0.00	1.00	-0.25
Key Predictor							
Bilingual Instruction	2071	0.54	0.50	1.00	0.00	1.00	-0.14
Main Outcome Variables							
Coethnic MP	2063	0.56	0.20	0.56	0.00	1.00	0.03
Coethnic PM	2061	0.80	0.27	1.00	0.00	1.00	-1.24
Ethnic Rights	2060	0.29	0.29	0.33	0.00	1.00	0.63
Ethnic Party	2061	0.31	0.29	0.33	0.00	1.00	0.58
Pretreatment Covariates							
Female	2071	0.57	0.50	1.00	0.00	1.00	-0.27
Home Language (Malay)	2071	0.52	0.50	1.00	0.00	1.00	-0.08
Home Language (English)	2071	0.02	0.14	0.00	0.00	1.00	6.89
Home Language (Multi)	2071	0.46	0.50	0.00	0.00	1.00	0.16
Urban School	2063	0.68	0.47	1.00	0.00	1.00	-0.76
Posttreatment Covariates							
Education	2071	4.46	1.35	5.00	1.00	6.00	-1.33
Married	2070	0.67	0.47	1.00	0.00	1.00	-0.73
Employed	2071	0.90	0.29	1.00	0.00	1.00	-2.74
Income	2062	3.33	2.53	3.00	0.00	10.00	0.86
Primary & Alternative Mechanisms							
English Proficiency	2071	3.87	0.91	4.00	1.00	5.00	-0.62
Perspective-Taking	2047	0.68	0.16	0.68	0.00	1.00	-0.10
Contact Quantity	2047	5.17	1.18	6.00	1.00	6.00	-1.64
Contact Quality	2047	5.93	1.22	6.00	1.00	7.00	-1.24
Other Variables							
English Emphasis	2058	2.90	0.82	3.00	1.00	4.00	-0.25
English Disrespect	2056	2.25	0.92	2.00	1.00	4.00	0.52
English	2071	0.67	0.47	1.00	0.00	1.00	-0.74
Bilingual	2071	0.64	0.48	1.00	0.00	1.00	-0.59

SI2b Balance Checks for Randomized Hypothetical Profiles

During the survey, respondents were presented with two hypothetical candidate profiles where the ethnicity attribute was randomized between Chinese (non-coethnic) and Malay (coethnic) while the remaining attributes did not vary (Table 1 of the main text). To check whether the profiles were randomly assigned to respondents, I regress the two profiles on the set of pretreatment and posttreatment covariates using linear, logit, and instrumental variables regression models. The results are displayed in Table SI2b.3. None of the variables predict the assignment of candidate profiles at the 95% level, thereby suggesting that the pretreatment and posttreatment covariates are balanced across the two profiles.

Table SI2b.3: **Balance Checks for Randomized Hypothetical Profiles**

DV =	Candidate Profile		
	OLS	Logit	Instrumental Variable
Bilingual Instruction	−0.006	−0.025	−0.059
	(0.026)	(0.106)	(0.046)
Reform	−0.026	−0.103	
	(0.026)	(0.103)	
Female	0.008	0.033	0.009
	(0.023)	(0.093)	(0.023)
Education	0.007	0.030	0.010
	(0.010)	(0.039)	(0.010)
Married	0.039	0.155	0.037
	(0.025)	(0.100)	(0.025)
Employed	0.003	0.014	0.003
	(0.041)	(0.166)	(0.041)
Income	−0.003	−0.010	−0.002
	(0.005)	(0.022)	(0.005)
Home Language (English)	−0.017	−0.068	−0.007
	(0.082)	(0.327)	(0.082)
Home Language (Multi)	−0.016	−0.065	−0.010
	(0.024)	(0.095)	(0.024)
Constant	0.467**	−0.131	0.463**
	(0.052)	(0.208)	(0.051)
N	2061	2061	2061
Log Likelihood		−1425.760	

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. The outcome variable (i.e., *Candidate Profile*) is binary, where one equals the pair of Chinese candidate A and Malay candidate B, and zero equals the pair of Malay candidate A and Chinese candidate B. The reference category for the variable *Home Language* corresponds to individuals who only speak *Bahasa Melayu* at home. $^{**}p < 0.01$; $^{*}p < 0.05$.

SI2c Probability of Bilingual Instruction

To verify whether the reform significantly predicts whether an individual received bilingual instruction, I regress *Bilingual Instruction* on *Reform*, together with the pretreatment covariates (i.e., *Female*, *Home Language*, and *Urban School*) using both linear and logit models. The results are shown in Table SI2c.4. As expected, survey respondents who were born on or after the cutoff date at January 1, 1990 were more likely to study STEM subjects in English and non-STEM subjects in *Bahasa Melayu*. The coefficient estimates for *Reform* are also statistically significant at the 95% level.

Table SI2c.4: **Reform Increases Probability of Bilingual Instruction**

DV =	Bilingual Instruction	
	OLS	Logit
Reform	0.533**	2.565**
	(0.047)	(0.289)
Female	−0.009	−0.049
	(0.019)	(0.107)
Home Language (English)	0.250**	1.417**
	(0.069)	(0.418)
Home Language (Multi)	0.162**	0.904**
	(0.019)	(0.109)
Urban School	0.099**	0.562**
	(0.019)	(0.108)
Constant	0.055	−2.311**
	(0.034)	(0.238)
Cohort FE	Yes	Yes
N	2063	2063
Log Likelihood		−1103.224

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. Both the main predictor (i.e., *Reform*) and the outcome variables (i.e., *Bilingual Instruction*) are binary. The reference category for the variable *Home Language* corresponds to individuals who only speak *Bahasa Melayu* at home. **p < 0.01; *p < 0.05.

SI3 Main Results

SI3a Bilingual Instruction Reduces Ethnic Outgroup Political Discrimination

Table SI3a.5 presents the full regression results that were summarized in Figure 3 of the main text. With the exception of *Coethnic MP* (which is still significant at the 90% level),

respondents who received bilingual instruction in school report lower levels of political discrimination against ethnic outgroups across three out of the four outcome measures.

Table SI3a.5: **Local Average Treatment Effect of Bilingual Instruction on Ethnic Outgroup Political Discrimination**

	Local Average Treatment Effects (LATE) Models			
DV =	Coethnic MP	Coethnic PM	Ethnic Rights	Ethnic Party
Bilingual Instruction	−0.081	−0.217**	−0.263**	−0.150*
	(0.044)	(0.062)	(0.066)	(0.068)
Female	0.011	0.021	−0.020	−0.005
	(0.009)	(0.012)	(0.014)	(0.013)
Home Language (English)	−0.047	−0.109	−0.031	0.038
	(0.039)	(0.059)	(0.044)	(0.047)
Home Language (Multi)	−0.014	−0.028	0.005	−0.011
	(0.011)	(0.016)	(0.018)	(0.018)
Urban School	0.010	−0.011	−0.016	0.017
	(0.011)	(0.015)	(0.017)	(0.016)
Cohort FE	Yes	Yes	Yes	Yes
N	2056	2053	2052	2053

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. The values for each of the four outcome measures range between zero and one, with larger values reflecting more outgroup discrimination. The reference category for the variable *Home Language* corresponds to individuals who only speak *Bahasa Melayu* at home. $^{**}p < 0.01$; $^{*}p < 0.05$.

I also report findings from another set of models that use a fuzzy regression discontinuity (RD) approach in Table SI3a.6. Formally,

$$\text{Bilingual Instruction}_i = \alpha + \gamma \text{Reform}_i + f(x_i) + \psi Z_i + \epsilon_i$$
$$Y_i = \zeta + \tilde{\beta}\widehat{\text{Bilingual Instruction}}_i + f(x_i) + \delta Z_i + \mu_i$$

The running variable x_i is the difference in the number of days between a respondent's birth date and January 1, 1990. Next, $f(x_i)$ is a linear function of the running variable. The LATE is then estimated by picking an optimal bandwidth within which the functional form between the running variable and the outcome measure can be approximated using $f(x_i)$, with larger triangular kernel weights assigned to observations that are closer to the cutoff at January 1, 1990 (Calonico et al., 2014).

According to Table SI3a.6, the coefficient signs of the point estimates vary from one component measure to another. For instance, exposure to bilingual instruction is associated with less ingroup favoritism with regard to the choice of the country's chief executive (*Coethnic PM*), but higher levels of outgroup discrimination in terms of the political rights of ethnic minority groups in Malaysia (*Ethnic Rights*). Note that these estimates correspond to the

average treatment effects for a subset of individuals who were born just before and after the birth date cutoff at January 1, 1990. As such, these findings could be indicative of a sense of frustration and resentment within the inaugural cohort of Malay students (and their parents) who had to suddenly adapt to the new curricular requirements under the reform, which in turn might have induced some level of political prejudice against ethnic outgroups at the cutoff. Similar mixed findings are observed when a quadratic function of the running variable was employed (Table SI3a.7).

Table SI3a.6: **Local Average Treatment Effect (LATE) of Bilingual Instruction on Ethnic Outgroup Political Discrimination (Fuzzy RD)**

	Local Average Treatment Effects (LATE) Models			
DV =	Coethnic MP	Coethnic PM	Ethnic Rights	Ethnic Party
Bilingual Instruction	0.054	−0.097**	0.097*	−0.153**
	(0.030)	(0.037)	(0.041)	(0.036)
Running	0.00001	0.0001*	0.00004	0.0002**
	(0.00002)	(0.00003)	(0.00003)	(0.00003)
Female	−0.008	0.013	−0.022**	−0.011
	(0.007)	(0.008)	(0.008)	(0.008)
Home Language (English)	−0.063*	−0.064*	−0.093**	0.009
	(0.031)	(0.028)	(0.026)	(0.023)
Home Language (Multi)	−0.037**	−0.067**	−0.012	−0.033**
	(0.007)	(0.008)	(0.009)	(0.008)
Urban School	0.013	−0.020*	−0.038**	0.015
	(0.007)	(0.009)	(0.011)	(0.009)
Cohort FE	No	No	No	No
N	808	603	684	602
Bandwidth	[-674,674]	[-517,517]	[-605,605]	[-520,520]
Kernel	Triangular	Triangular	Triangular	Triangular

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. The values for each of the four outcome measures range between zero and one, with larger values reflecting more outgroup discrimination. The reference category for the variable *Home Language* corresponds to individuals who only speak *Bahasa Melayu* at home. The *Running* variable is the difference in the number of days between a respondent's birth date and the reform cutoff date at January 1, 1990. The bandwidth refers to the number of days before and after the cutoff at January 1, 1990, the latter of which is set at zero. $^{**}p < 0.01$; $^{*}p < 0.05$.

Taken together, the results from Tables SI3a.5, SI3a.6, and SI3a.7 are illustrative of the growing pains of a policy reform that ultimately brought about more inclusive political attitudes among affected Malay students. Although the immediate impact of the reform at the cutoff appears mixed, the inclusionary effects of bilingual instruction become more apparent as subsequent cohorts of students acclimate to the new language requirements. Given our interest in the overall – and not the immediate – effect of bilingual instruction on outgroup discrimination, the rest of the empirical section presents results from the instrumental

variables regression models as specified in the main text.

Table SI3a.7: **LATE of Bilingual Instruction on Ethnic Outgroup Political Discrimination (Fuzzy RD, Quadratic Function)**

	Local Average Treatment Effects (LATE) Models			
DV =	Coethnic MP	Coethnic PM	Ethnic Rights	Ethnic Party
Bilingual Instruction	0.066*	−0.066	0.078	−0.093*
	(0.030)	(0.040)	(0.041)	(0.040)
Running	−0.00000	0.00003	0.0001	0.0001**
	(0.00002)	(0.00003)	(0.00003)	(0.00003)
Running^2	0.00000*	0.000	−0.00000**	−0.00000**
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Female	−0.005	0.016	−0.021*	−0.013
	(0.007)	(0.009)	(0.008)	(0.010)
Home Language (English)	−0.064	−0.071*	−0.088**	−0.011
	(0.033)	(0.035)	(0.026)	(0.028)
Home Language (Multi)	−0.038**	−0.070**	−0.011	−0.026*
	(0.007)	(0.010)	(0.009)	(0.010)
Urban School	0.011	−0.020	−0.038**	0.014
	(0.008)	(0.010)	(0.011)	(0.011)
Cohort FE	No	No	No	No
N	893	823	697	806
Bandwidth	[-751,751]	[-691,691]	[-612,612]	[-677,677]
Kernel	Triangular	Triangular	Triangular	Triangular

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. The values for each of the four outcome measures range between zero and one, with larger values reflecting more outgroup discrimination. The reference category for the variable *Home Language* corresponds to individuals who only speak *Bahasa Melayu* at home. The *Running* variable is the difference in the number of days between a respondent's birth date and the reform cutoff date at January 1, 1990. The bandwidth refers to the number of days before and after the cutoff at January 1, 1990, the latter of which is set at zero. $^{**}p < 0.01$; $^{*}p < 0.05$.

SI3b Bilingual Instruction Improves English Proficiency & Perspective-Taking

Table SI3b.8 displays the full regression results that were summarized in Figure 4 of the main text. For the unconditional models, I estimate the same set of instrumental variables regression (or LATE) models except that the outcome variables are replaced by *English Proficiency* or *Perspective-Taking*. With regard to the conditional models, I include two additional interaction terms: (1) *Reform* × *Home Language* in the first stage, and (2) *Bilingual Instruction* × *Home Language* in the second stage. This allows me to estimate the marginal effects of bilingual instruction, conditional on the type and diversity of language(s)

spoken at home. The reference category for *Home Language* belongs to those who speak only *Bahasa Melayu* at home.

Table SI3b.8: **Unconditional and Conditional LATE of Bilingual Instruction on English Proficiency and Perspective-Taking Ability**

DV =	English Proficiency		Perspective-Taking	
Models	Unconditional	Conditional	Unconditional	Conditional
Bilingual Instruction	0.651**	0.656**	0.044	0.073*
	(0.189)	(0.194)	(0.033)	(0.034)
Bilingual Instruction ×		1.011		0.053
Home Language (English)		(1.411)		(0.177)
Bilingual Instruction ×		0.021		−0.065*
Home Language (Multi)		(0.149)		(0.031)
Female	0.022	0.026	0.020**	0.023**
	(0.036)	(0.037)	(0.007)	(0.007)
Home Language (English)	0.332	−0.381	−0.041	−0.085
	(0.213)	(1.102)	(0.031)	(0.123)
Home Language (Multi)	0.578**	0.564**	0.026**	0.062**
	(0.047)	(0.102)	(0.009)	(0.020)
Urban School	0.158**	0.152**	0.010	0.009
	(0.043)	(0.045)	(0.009)	(0.009)
Cohort FE	Yes	Yes	Yes	Yes
N	2063	2063	2040	2040

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. *English Proficiency* is measured on a 5-point Likert scale, where larger values reflect higher levels of self-reported English fluency. The values for *Perspective-Taking* range between 0 and 1, and it is computed by taking the mean of the normalized responses to the perspective-taking sub-scale items from the Interpersonal Reactivity Index (Davis, 1983). Larger values reflect higher perspective-taking ability. The reference category for the variable *Home Language* corresponds to individuals who only speak *Bahasa Melayu* at home. **p $<$ 0.01; *p $<$ 0.05.

SI4 Additional Tests

SI4a Profiling Compliers and Non-Compliers in the Malaysia Case

Before presenting the results, I describe the intuition underlying the methods used to estimate the profiles of compliers and non-compliers in the Malaysia case.[2] Assuming that there are no-defiers, there are three groups of survey respondents. First, always-takers would always opt for English instruction for STEM classes regardless of their birth dates. Second, never-takers would always choose to undergo *Bahasa Melayu* instruction for STEM subjects regardless of their birth dates. Lastly, compliers are ethnic Malays who received bilingual

[2]Interested readers may refer to Marbach and Hangartner (2020) for a detailed explanation of their model.

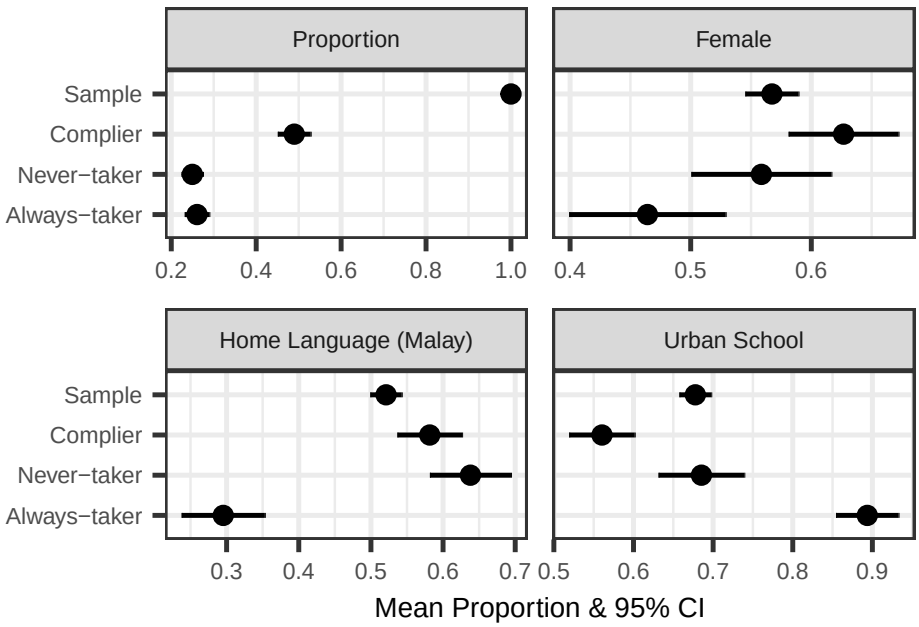
instruction in school as a direct result of the education reform *and* those who received monolingual instruction because they were born before the reform cutoff date at January 1, 1990.

To estimate the proportion of always-takers, we only need to look at the proportion of respondents who received English instruction for STEM subjects and who were born before January 1, 1990 ($\approx 25\%$ based on Figure 2 from the main text). Similarly, we can estimate the proportion of never-takers by looking at the proportion of respondents who did not receive English instruction for STEM classes (hence *Bahasa Melayu* instruction) and who were born on and after January 1, 1990 ($\approx 25\%$ based on Figure 2). Overall, non-compliers comprise about half of the survey respondents while compliers make up the other half.

Building on the above intuition and the assumption that the instrument (i.e., *Reform*) was randomly assigned to ethnic Malay citizens in Malaysia, we can also estimate the profiles of compliers and non-compliers based on their pretreatment covariates. The covariate mean for compliers is simply the covariate mean weighted by the share of compliers in the sample. Since the shares of compliers, never-takers, and always-takers are unknown, I use the bootstrap method to obtain standard errors to reflect the uncertainty in the estimates.

Figure SI4a.2 visualizes the profiles of compliers and noncompliers in the Malaysia survey based on three pretreatment covariates. The top left panel shows that about half of the survey respondents are identified as compliers. With regard to the pretreatment covariates, compliers tend to be females, are more likely to come from families that speak only *Bahasa Melayu* at home, and were more likely to attend rural secondary schools.

Figure SI4a.2: **Complier and Noncomplier Profiles Based on Pretreatment Covariates**



Notes: Each point in the plot corresponds to the mean proportion of respondents who are compliers, never-takers, or always-takers in the survey. The horizontal lines are the 95% bootstrapped confidence intervals for each subpopulation.

SI4b Inclusion of Sampling Weights & Cluster-Robust SEs

I estimate sampling weights based on three demographic indicators from the 2010 Malaysian census data: (1) proportion of residents by state, (2) highest education qualification, and (3) gender. These are included in the instrumental variables regression models as part of a robustness test, and the results are displayed in Table SI4b.9. All the coefficient estimates are signed in the correct direction, and two out of the four outcome measures are statistically significant at the conventional level of 95%.

Table SI4b.9: **LATE of Bilingual Instruction on Ethnic Outgroup Political Discrimination (Sampling Weights)**

DV =	Coethnic MP	Coethnic PM	Ethnic Rights	Ethnic Party
Bilingual Instruction	−0.065	−0.179*	−0.241**	−0.067
	(0.053)	(0.080)	(0.076)	(0.077)
Female	0.019	0.041*	−0.026	−0.009
	(0.011)	(0.017)	(0.017)	(0.016)
Home Language (English)	0.019	−0.190**	−0.044	−0.009
	(0.061)	(0.064)	(0.047)	(0.053)
Home Language (Multi)	−0.010	−0.026	0.010	−0.047*
	(0.015)	(0.023)	(0.023)	(0.021)
Urban School	0.006	−0.008	−0.008	0.002
	(0.012)	(0.018)	(0.019)	(0.018)
Constant	0.571**	0.819**	0.421**	0.425**
	(0.025)	(0.037)	(0.035)	(0.037)
Cohort FE	Yes	Yes	Yes	Yes
N	2043	2040	2039	2040

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. Sampling weights are computed based on three demographic indicators from Malaysia's 2010 census data: (1) state, (2) education, and (3) gender. **p $<$ 0.01; *p $<$ 0.05.

Next, I re-estimate the p-values and statistical significance of the coefficient estimates using robust standard errors clustered by birth year. This is motivated by the possibility that the pretreatment covariate and outcome measures for respondents who were born in a particular birth year cohort might be correlated with each other. As shown in Table SI4b.10, the substantive findings remain unchanged – i.e., all four point estimates are negatively signed and statistically significant at the 95% level. In fact, the point estimate for *Bilingual Instruction*, which was previously significant at $p < 0.1$ in the baseline LATE model (see Table SI3a.5), becomes significant at $p < 0.01$ after accounting for cluster-robust standard errors.

Table SI4b.10: **LATE of Bilingual Instruction on Ethnic Outgroup Political Discrimination (Cluster-Robust SEs)**

DV =	Coethnic MP	Coethnic PM	Ethnic Rights	Ethnic Party
Bilingual Instruction	−0.081**	−0.217**	−0.263**	−0.150**
	(0.003)	(0.005)	(0.009)	(0.009)
Female	0.011	0.021	−0.020	−0.005
	(0.010)	(0.019)	(0.017)	(0.019)
Home Language (English)	−0.047	−0.109*	−0.031	0.038
	(0.037)	(0.054)	(0.050)	(0.043)
Home Language (Multi)	−0.014	−0.028	0.005	−0.011
	(0.012)	(0.017)	(0.018)	(0.018)
Urban School	0.010	−0.011	−0.016	0.017
	(0.009)	(0.019)	(0.042)	(0.025)
Constant	0.586**	0.857**	0.440**	0.415**
	(0.007)	(0.011)	(0.022)	(0.022)
Cohort FE	Yes	Yes	Yes	Yes
N	2056	2053	2052	2053

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors clustered by birth year are displayed in parentheses. The values for each of the four outcome measures range between zero and one, with larger values reflecting more outgroup discrimination. The reference category for the variable *Home Language* corresponds to individuals who only speak *Bahasa Melayu* at home. **p < 0.01; *p < 0.05.

SI4c Tests of Exclusion Restriction

I test two possible alternative channels in which the instrumental variable *Reform* might affect ethnic outgroup political discrimination, other than the language of instruction used in schools. One possibility is that the reform might have increased the number of ethnic Chinese students who were enrolled into public secondary schools, thereby increasing the amount of cross-ethnic contact between Malay and Chinese students. A second possibility is that the reform might have induced resentment toward learning and speaking English since students from private secondary schools (e.g., Chinese students in Chinese independent high schools) were exempted from the language reform. If these alternative channels were true, then we should observe statistically significant associations between the instrumental variable and the (1) measures of cross-ethnic contact (i.e., *Contact Quantity* and *Contact Quality*) and (2) measures of individual opinions toward the use of English in daily activities (i.e., *English Emphasis* and *English Disrespect*). Put simply, the reform should have directly affected respondents' contact with members of other ethnic outgroups as well as their views on the use of English in their daily lives.

I regress the outcome measures of cross-ethnic contact and individual opinions about the use of English on *Reform*, the three pretreatment covariates, and cohort fixed effects using a linear model. According to Table SI4c.11, *Reform* fails to predict the outcome measures across the two alternative channels, thereby mitigating the plausibility of these

two alternative mechanisms.

Table SI4c.11: **Tests of Exclusion Restriction**

Alternative Channels	Contact		English Views	
DV =	Contact Quantity	Contact Quality	English Emphasis	English Disrespect
Reform	0.189	0.112	−0.133	−0.123
	(0.138)	(0.139)	(0.097)	(0.100)
Female	−0.182**	0.139*	−0.004	−0.202**
	(0.051)	(0.055)	(0.037)	(0.041)
Home Language (English)	0.134	0.373	−0.107	−0.237
	(0.187)	(0.217)	(0.130)	(0.150)
Home Language (Multi)	0.415**	0.355**	0.012	−0.140**
	(0.051)	(0.053)	(0.037)	(0.041)
Urban School	0.193**	0.075	−0.055	−0.070
	(0.060)	(0.057)	(0.039)	(0.045)
Constant	4.831**	5.457**	2.876**	2.472**
	(0.128)	(0.114)	(0.077)	(0.086)
Cohort FE	Yes	Yes	Yes	Yes
N	2039	2039	2050	2048

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. Both *English Emphasis* and *English Disrespect* are measured on a 4-point Likert scale, where larger values correspond to greater resentment toward the use of English. The values for *Contact Quantity* range between 1 ("Never") and 6 ("Daily"), whereas the minimum and maximum values for *Contact Quality* are 1 ("Not at all positive") and 7 ("Very positive") respectively. The reference category for the variable *Home Language* corresponds to individuals who only speak *Bahasa Melayu* at home. $^{**}p < 0.01$; $^{*}p < 0.05$.

SI4d Alternative Explanation 1: Posttreatment Covariates

Recall that the baseline models did not include the bundle of posttreatment covariates such as education and income due to the concern that the inclusion of these variables might induce posttreatment bias (Montgomery et al., 2018). Notwithstanding, it is entirely possible that the relationship between bilingual instruction and outgroup attitudes may be explained by these socioeconomic mechanisms, as opposed to the proposed perspective-taking mechanism.

In light of the above concern, I re-estimated the same set of instrumental variables regression models by including a broader set of social and economic controls such as income and highest education qualification. Table SI4d.12 reports the same key findings even after accounting for respondents' highest education qualification, marital status, income, and employment situation.

Table SI4d.12: **LATE of Bilingual Instruction on Ethnic Outgroup Political Discrimination (with Posttreatment Covariates)**

DV =	Coethnic MP	Coethnic PM	Ethnic Rights	Ethnic Party
Bilingual Instruction	−0.074	−0.208**	−0.306**	−0.168*
	(0.046)	(0.067)	(0.070)	(0.071)
Female	0.012	0.010	−0.029	−0.004
	(0.010)	(0.013)	(0.015)	(0.014)
Home Language (English)	−0.052	−0.120*	−0.002	0.052
	(0.038)	(0.057)	(0.043)	(0.046)
Home Language (Multi)	−0.019	−0.041**	0.010	−0.008
	(0.011)	(0.015)	(0.018)	(0.017)
Urban School	0.008	−0.013	−0.010	0.018
	(0.010)	(0.014)	(0.017)	(0.015)
Education	−0.004	0.012	0.029**	0.012
	(0.005)	(0.007)	(0.008)	(0.007)
Married	0.014	0.044**	0.001	−0.021
	(0.010)	(0.015)	(0.016)	(0.015)
Employed	0.020	−0.023	0.049	0.059*
	(0.016)	(0.021)	(0.026)	(0.023)
Income	0.001	−0.001	−0.014**	−0.006
	(0.002)	(0.003)	(0.003)	(0.003)
Constant	0.574**	0.810**	0.335**	0.347**
	(0.024)	(0.031)	(0.038)	(0.035)
Cohort FE	Yes	Yes	Yes	Yes
N	2047	2044	2043	2044

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. **p < 0.01; *p < 0.05.

SI4e Alternative Explanation 2: Cross-Ethnic Contact

Another possible alternative explanation is that bilingual instruction facilitates cross-ethnic contact by providing a common language that members of different ethnic groups can communicate with each other (Wright and Tropp, 2005). It is also possible that these cross-ethnic interactions may induce greater perspective-taking among individuals who were affected by the education reform.

To address this concern, I replicate the instrumental variables regression models by replacing the outcome variables with *Contact Quantity* and *Contact Quality*. If this alternative mechanism was true, then we should expect a positive and statistically significant correlation between bilingual instruction and cross-ethnic contact. However, Table SI4e.13 shows that the main predictor does not predict the frequency and quality of contact between ethnic Malays and other ethnic minorities in Malaysia.

Table SI4e.13: **Bilingual Instruction Does Not Increase Cross-Ethnic Contact Quantity or Quality**

DV =	Contact Quantity	Contact Quality
Bilingual Instruction	0.353	0.210
	(0.258)	(0.259)
Female	−0.179**	0.141**
	(0.050)	(0.054)
Home Language (English)	0.046	0.321
	(0.208)	(0.226)
Home Language (Multi)	0.358**	0.322**
	(0.065)	(0.068)
Urban School	0.158*	0.055
	(0.066)	(0.063)
Constant	4.811**	5.445**
	(0.140)	(0.124)
Cohort FE	Yes	Yes
N	2039	2039

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. The values for *Contact Quantity* range between 1 ("Never") and 6 ("Daily"), whereas the minimum and maximum values for *Contact Quality* are 1 ("Not at all positive") and 7 ("Very positive") respectively. **p $<$ 0.01; *p $<$ 0.05.

SI4f Alternative Explanation 3: English Instruction

A final alternative explanation is that the observed patterns may stem from the effect of English instruction, as opposed to the general expectation that bilingual instruction – regardless of the languages used – should lead to less political discrimination against ethnic outgroups. In addition, the results may be a reflection the finding that English literacy is associated with more cosmopolitan attitudes (hence greater outgroup tolerance) given its status as a lingua franca in many multi-cultural and global settings (Hu and Liu, 2020).

I test the possibility of this alternative explanation through two tests. First, I investigate whether there is any substantive difference between English-speaking bilinguals and non-English-speaking bilinguals. Specifically, if the alternative explanation was true, then we might expect that English-speaking bilinguals should display less political discrimination against ethnic outgroups than non-English-speaking bilinguals, given the suggestion that English proficiency is associated with less intergroup prejudice. Focusing on a subset of bilingual respondents, I regress the four outcome measures of ethnic outgroup political discrimination on *English*, a binary variable that equals if the respondent speaks English at home or outside, and zero otherwise. Table SI4f.14 shows that English-speaking bilinguals exhibit no significant difference in outgroup discrimination when compared to non-English-speaking bilinguals.

Next, I conduct a second test by examining the subset of ethnic Malay respondents who

Table SI4f.14: **Effect of Spoken Language on Ethnic Outgroup Political Discrimination (Only Bilingual Respondents)**

DV =	Coethnic MP	Coethnic PM	Ethnic Rights	Ethnic Party
English	−0.015	0.145	−0.010	0.126
	(0.016)	(0.272)	(0.028)	(0.120)
Female	−0.011	0.012	−0.020	−0.003
	(0.012)	(0.016)	(0.017)	(0.017)
Urban School	−0.003	−0.031	−0.071**	−0.014
	(0.013)	(0.017)	(0.018)	(0.018)
Education	−0.011	−0.010	0.008	0.009
	(0.006)	(0.008)	(0.008)	(0.009)
Married	0.025	0.036*	0.020	−0.010
	(0.013)	(0.018)	(0.018)	(0.018)
Employed	0.035	0.028	−0.008	0.030
	(0.025)	(0.033)	(0.034)	(0.032)
Income	−0.004	−0.012**	−0.021**	−0.012**
	(0.003)	(0.004)	(0.004)	(0.004)
Constant	0.605**	0.690*	0.433**	0.241
	(0.042)	(0.278)	(0.061)	(0.133)
Cohort FE	Yes	Yes	Yes	Yes
N	1313	1310	1310	1310

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. The models are applied to a subset of multilingual respondents who speak more than one language at home and/or outside. *English* equals one if the respondent speaks English at home and/or outside, and zero otherwise. **p < 0.01; *p < 0.05.

do not speak English at all. For this test, I regress the outcome measures of outgroup discrimination on *Bilingual*, a binary variable that equals one if the respondent speaks more than one language at home and/or outside, and zero otherwise. According to the theory, we expect that bilingual respondents should indicate less political discrimination against ethnic outgroups. Table SI4f.15 provides some evidence in support of this expectation. Specifically, coefficient estimates for *Bilingual* are negative in three out of the four models, though it is only statistically significant for the *Coethnic MP* outcome measure. One possibility is that the number of non-English-speaking bilinguals is considerably small in the survey. As such, the null findings may stem from the under-powered nature of the tests.

SI4g Generalizability of Findings

I investigate the theoretical link between bilingualism and outgroup tolerance at the cross-national level using data from the Asian Barometer Survey (ABS, 2017). The main predictor in the analyses is *Bilingual*, which equals one if the respondent speaks a mixture of local

Table SI4f.15: **Effect of Bilingualism on Ethnic Outgroup Political Discrimination (Only Non-English-Speaking Respondents)**

DV =	Coethnic MP	Coethnic PM	Ethnic Rights	Ethnic Party
Bilingual	−0.053**	−0.223	0.042	−0.156
	(0.022)	(0.252)	(0.030)	(0.124)
Female	0.049***	0.015	−0.041*	0.008
	(0.016)	(0.020)	(0.024)	(0.024)
Urban School	0.013	−0.024	0.039	0.022
	(0.016)	(0.019)	(0.025)	(0.024)
Education	−0.008	0.006	0.014*	−0.003
	(0.005)	(0.007)	(0.008)	(0.008)
Married	0.008	0.056***	0.019	−0.036
	(0.017)	(0.020)	(0.025)	(0.025)
Employed	0.005	−0.080***	0.063*	0.054
	(0.022)	(0.025)	(0.033)	(0.033)
Income	0.006	0.014***	−0.011*	0.003
	(0.004)	(0.004)	(0.006)	(0.006)
Constant	0.574***	0.848***	0.349***	0.385***
	(0.040)	(0.046)	(0.057)	(0.058)
Cohort FE	Yes	Yes	Yes	Yes
N	666	666	665	666

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors (HC0) are displayed in parentheses. The models are applied to a subset of non-English-speaking respondents who do not speak English at home and/or outside. *Bilingual* equals one if the respondent speaks more than more language at home and/or outside, and zero otherwise. **p < 0.01; *p < 0.05.

and official languages at home, and zero otherwise. The two main outcome variables are (1) *Ethnic Inequality*, a 4-point ordinal variable that measures how strongly a respondent feels toward the treatment of different ethnic communities in their country, and (2) *Anti-Immigrant*, a 4-point ordinal variable that measures respondents' opinions on whether the government should increase or decrease the flow of immigrants into their country. I also include other socioeconomic controls and country fixed effects in the linear models. Finally, I isolate the effect of the English language on outgroup attitudes by excluding countries where a substantial proportion of their residents speak English as their first or second language. These are Hong Kong, Malaysia, Singapore, and Philippines.

The full regression results are displayed in Table SI4g.16. Accordingly, respondents who speak two languages at home are more sensitive toward the plight of other ethnic communities in their country (i.e., positive point estimates for *Ethnic Inequality*) and display less anti-immigrant sentiments (i.e., negative point estimates for *Anti-Immigrant*). In addition, the results remain unchanged even if we exclude respondents who come from countries where a substantial proportion of their populations speak English as a first or second language.

Table SI4g.16: **Bilinguals from ABS Display Less Outgroup Prejudice**

DV =	Ethnic Inequality		Anti-Immigrant	
	All Respondents	Exclude English-Speaking Countries	All Respondents	Exclude English-Speaking Countries
Bilingual	0.049*	0.048*	−0.074**	−0.055†
	(0.022)	(0.023)	(0.026)	(0.031)
Education	0.013*	0.007	−0.019**	−0.020**
	(0.005)	(0.006)	(0.006)	(0.007)
Female	−0.006	−0.004	0.051**	0.046*
	(0.014)	(0.016)	(0.016)	(0.019)
Age	−0.002**	−0.003**	0.004**	0.004**
	(0.001)	(0.001)	(0.001)	(0.001)
Married	0.039*	0.048*	0.031†	0.041†
	(0.017)	(0.020)	(0.018)	(0.022)
Income	0.025**	0.013*	−0.019**	−0.027**
	(0.006)	(0.006)	(0.007)	(0.008)
Urban	0.110**	0.105**	0.006	0.041†
	(0.018)	(0.019)	(0.019)	(0.023)
Religion	−0.008	0.017	0.041	0.041
	(0.026)	(0.030)	(0.030)	(0.036)
Constant	2.220**	2.111**	2.798**	2.227**
	(0.051)	(0.052)	(0.054)	(0.063)
Country FE	Yes	Yes	Yes	Yes
N	12,726	9,616	10,586	7,516

Notes: Table entries are unstandardized coefficient estimates. Robust standard errors are displayed in parentheses. The values for *Ethnic Inequality* and *Anti-Immigrant* range between 1 and 4. The larger the *Ethnic Inequality*, the stronger the respondent feels that different ethnic groups are treated unfairly by the government. In contrast, the larger the *Anti-Immigrant*, the weaker the respondent discriminates against immigrants in their country. English-speaking countries refer to countries where a substantial proportion of their residents speak English as their first or second language. These are Hong Kong, Malaysia, Singapore, and Philippines. $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\dagger}p < 0.1$.

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