# Integrating Minorities in the Classroom: The Role of Students, Parents, and Teachers*

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**Abstract**

We develop a multi-agent model of the education production function where investments of students, parents, and teachers are linked to the presence of minorities in the classroom. We then test the key implications of this model using rich survey data and a mandate to randomly assign students to classrooms. Consistent with our model, we show that exposure to minority peers decreases student effort, parental investments, and teacher engagement and it results in lower student test scores. Observables correlated with minority status explain less than a third of the reduced-form test score effect while over a third can be descriptively attributed to endogenous responses of the agents.

**JEL:** I23, I26, D13
**Keywords:** Minorities, Indigenous students, Peer effects, Student effort, Parental investments, Teachers

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# 1 Introduction

As societies become more culturally and ethnically diverse, policy makers face the challenge of providing minority communities with equal opportunities. Commonly used policies to increase fairness and correct historical injustices involve access to higher quality schooling (e.g., via school choice) or better neighborhoods (e.g., via housing vouchers) with the overarching idea that these lead to increased human capital and help overcome intergenerational disadvantage (Coleman, 1968, Heckman, 2000, Alba, Sloan and Sperling, 2011).[1] Yet, these policies could create externalities that undermine their goals. For example, school integration efforts can create tensions between minority and majority students, parents, and teachers, and lead to pushback and ultimately to more segregation (e.g., Card and Giuliano, 2013, Geay, McNally and Telhaj, 2013, Cascio and Lewis, 2012, Ohinata and Van Ours, 2013, Bisin et al., 2016).

Despite the importance of this topic, there is actually surprisingly little causal evidence on the consequences of being exposed to minorities in the school system, and we know even less about the role that endogenous responses of students, parents and teachers can play in this process. There are two main obstacles driving this knowledge gap. The first challenge is econometric and concerns the selection of minority and majority children into schools. Because majority and minority children are typically not randomly assigned to study together, their interactions and educational achievement are likely driven by common unobserved factors. This selection issue is further exacerbated by segregation. Since minority children often live in highly segregated neighborhoods, the majority children that remain in those neighborhoods could differ on both observable and unobservable characteristics from the general population. In other words, we need to be concerned not only with the selection of minorities but also with the selection of what majority students we observe (Figlio et al., 2024). The second challenge is data availability. In order to comprehensively understand the role that minorities play in the education production function, we need to observe human capital inputs of students, their parents, and teachers alongside with student achievement. Such comprehensive data are rarely available; thus, most studies have to settle with partial descriptions of the process or equate changes in educational outcomes with changes in inputs.

In this paper, we overcome both of those obstacles and ask three main research questions: How does exposure to minority peers in the classroom affect student achievement? Do minority students affect the investments of other students, their parents, and their teachers? And, finally, to what extent are these achievement effects driven by minority status *per se* versus characteristics

[1] Other ways of compensating for historical mistreatment of minorities involve financial compensations or increased self-governance. For example, in the United States, compensation efforts via financial transfers include reparation payments by the Federal Government in the 1990s in response to the Japanese-American internment (Shoag and Carollo, 2020) or the nation's first reparation law for African-American residents of the city of Evanston in 2019 (Evanston, 2019). When it comes to Indigenous Peoples, the population of interest in this paper, Australia, Canada, U.S. and Taiwan all offer a degree of self-governance for these communities as a recognition of their historical rights.

correlated with being a minority (e.g., lower baseline test scores)? Answering these questions is essential for understanding the role minorities play in the education production function, as well as the expected effects of integration efforts and the exact nature of the frictions they may generate.

To discipline our empirical analyses, we first develop a multi-agent model of the education production function where the investments of students, parents, and teachers are linked to the presence of minority students in the classroom. Our model predicts that students, parents and teachers will all decrease their effort in response to an increase share of minority students in the classroom under two, in our view uncontroversial, assumptions: (1) student, parent, and teacher efforts are $q$-complements (i.e., all complement one another in the production of educational achievement (see e.g., Hicks, 1956), and (2) the endowments of minority students are lower than those of majority students. The former assumption makes intuitive sense from an education perspective as it implies that e.g., each additional hour of student effort is more productive when parents are more invested in their child's education or when teachers are more engaged in class. The latter assumption is easily testable in most datasets and only needs to hold on average. Furthermore, it is enough that it holds *in perception* when subjective expectations are allowed—that is, even if there are no endowment differences between minority and majority students, as long as agents expect or perceive these differences to exist. This important feature of our model allows for mechanisms such as prejudice against minorities to drive behavior.

We test our theoretical predictions using data from the Taiwan Education Panel Survey (TEPS). There are three reasons why the TEPS is ideal for this exercise. First, it collects longitudinal information on test scores, student effort, parental investments and teacher engagement in a representative sample of Taiwanese classrooms. Second, the TEPS also collects information on Indigeneity based on which we group students into the minority and the majority. Indigenous students—who are most often easily identifiable in the classroom—have very different cultural and socioeconomic background than the Han Chinese majority. Third, using the TEPS, we can exploit Taiwan's national mandate to randomly assign students and teachers to classrooms within schools. This allows us to produce causal estimates of the exposure to minority peers free from econometric concerns that plague other settings (Manski, 1993, 2000, Angrist, 2014).

Empirically, we first document that Indigenous students are indeed disadvantaged across several pre-determined dimensions and are perceived as lower ability by their teachers. This is consistent with the key testable assumption in our model. Our main empirical results then confirm that when majority students are exposed to a 10 percentage points (pp) higher share of Indigenous peers in their classroom, their test scores decrease by 4% of a standard deviation (SD). We then show that 29% of this negative effect is due to peer characteristics correlated with Indigenous status —mainly lower ability and socioeconomic status —nonetheless leaving a sizeable share of residual effect. We further document that the same 10pp increase in share of Indigenous peers reduces majority student's study effort by 7.5% SD, decreases investments of majority parents by

7.2% SD, and diminishes teacher effort by 24.7% SD. Since all these outcomes are inputs in test score production function, we expect them to explain at least part of the residual effect. In fact, a descriptive analysis suggests that accounting for these endogenous responses can explain 39% of the negative test score effect.

Even though our classroom assignment is as good as random, we conduct a number of sensitivity analyses. Our conclusions are invariant to the exact definition of who the majority students are and to excluding private schools or schools where most sampled students are Indigenous. Furthermore, the results hold at the extensive margin (i.e., when our treatment is redefined as having *an* Indigenous student, and ignoring of how many) while the statistical significance is unaffected by randomization inference (Young, 2019). Finally, we execute two placebo analyses. Our first placebo analysis uses Hakka students, who are another officially recognized minority group in Taiwan with a distinct language and culture but not disadvantaged or discriminated against. Here, as predicted by our model, we do not find any negative peer effects. Second, we create "synthetic" Indigenous students defined as Han majority students matched with their Indigenous peers on pre-determined characteristics including several markers of disadvantage. Here, likewise, we do not find any negative peer effects. Taken together, these analyses suggest that something idiosyncratic to Indigenous students—such as the prejudice others hold against them —and not shared by other minority or disadvantaged groups is the plausible primary driver of the observed negative effects.

Our paper contributes to several strands of literature. First, we provide a theoretical model of interactions between majority and minority students in the classroom in which students', parents', and teachers' efforts affect students' test scores. Our model simplifies these interactions to their bare essentials, providing useful guidance for other settings in which they might occur (e.g., workplace or neighborhoods). At the same time, the addition of parental and teacher behaviors enriches the standard linear-in-means model of peer interactions and brings it closer to a plausible education production function. Because of its simplicity, the model can be easily adapted to explain multi-agent strategic interactions between any two groups where one is more disadvantaged than the other.

Second, we offer some of the first estimates of the effects of exposure to minorities in the classroom identified using institutionally generated random variation. The two closest studies to ours are Antecol, Eren and Ozbeklik (2016) and Hoxby and Weingarth (2005). Antecol, Eren and Ozbeklik (2016) use a small scale RCT based on Mathematica's Teachers Trained Through Different Routes to Certification program to study ability peer effects. They also investigate if a higher share of Black peers increases test scores of other Black students and fail to find any sizeable or statistically significant effects. Hoxby and Weingarth (2005) examines student reassignment in Wake County, North Carolina which was a result of a switch from a desegregation plan balancing students based on race to a plan which used income instead. This led to student reshuffling and produced "thousands of useful reassignment experiments." Their results highlight the importance of accounting for correlated characteristics; after accounting for peer achievement, they find only limited residual contribution from other peer characteristics such as race or income. In our application, this is clearly not the case as correlated characteristics can explain less than a third of the negative effect.

Other studies primarily used data from the United States due to their ethnic-racial history (Hoxby, 2000, Diette and Oyelere Uwaifo, 2014, Figlio and Özek, 2019, Angrist and Lang, 2004, Bifulco, Fletcher and Ross, 2011, Geay, McNally and Telhaj, 2013, Setren, 2022) and most of these are forced to rely on cohort-by-school variation due to the absence of institutionalized randomization. The cohort-by-school source of variation comes with issues—for instance, Vigdor and Nechyba (2007) show that within-school comparisons are not always valid due to changes in local school markets affecting peer-composition over time. Furthermore, much of the prior literature that attempts to deal with selection of minorities still cannot circumvent the selection of majority students that remain in schools attended by minorities (Figlio et al., 2024). Another complication is that prior work often defines minority status based on very heterogeneous peer groups such as immigrants (e.g., Figlio and Özek, 2019, Figlio et al., 2024, Diette and Oyelere Uwaifo, 2014, 2017) or refugees (e.g., Alan et al., 2021, Imberman, Kugler and Sacerdote, 2012, Green and Iversen, 2022, Morales, 2022). Such heterogeneity within the treatment group of interest makes interpreting the results difficult if one subset of peers could generate positive while another negative effects. Some studies on immigrant populations do consider different groups of immigrants (see e.g., Friesen and Krauth, 2011), which is valuable since it allows for accurate characterization of the treatment group. Another notable exception is Hoxby (2000) who distinguishes between multiple racial groups using data from Texas, and finds negative and statistically significant estimates only for exposure to Black peers. While our data does not allow us to distinguish between different Indigenous groups, in Taiwan virtually all Indigenous groups are severely disadvantaged and subject to prejudice when compared to the Han Chinese majority. This homogenizes our treatment group, harmonizes the mechanisms that could be driving our results, and provides a set of estimates that are unique in that they describe reactions to deeply disadvantaged peers. Our estimates are therefore particularly relevant for informing inclusion policies targeted at the most marginalized groups in society.[2]

[2] More broadly, our results contribute to a growing literature in education economics studying peer effects with random assignment. In this literature, previous studies exploit random assignment to middle schools in South Korea and random assignment to classrooms within schools in a few selected Chinese middle schools, however, this variation has not been used to explore minority peer effects; rather these studies estimate the impact of studying with higher-achieving peers, female peers, children of college-educated parents or teacher-student match effects (see e.g., Kang et al., 2007, Fang and Wan, 2020, Chung, 2020, Lim and Meer, 2017, 2020, Feng and Li, 2016). Our results are thus uniquely positioned to inform the policy debate on school integration efforts. In tertiary education studies that use institutionally generated random assignment, as we do, are more common. These include Carrell, Fullerton and West (2009), Carrell, Sacerdote and West (2013) in the United States, Feld and Zoelitz (2017) in The Netherlands, Garlick (2018) in South Africa, and Brunello, De Paola and Scoppa (2010) in Italy. However, even in this broader literature only Chevalier, Isphording and Lisauskaite (2020) specifically focus on the minority status of peers—in their case, non-native speakers at a British university—and on the issue of language barriers in the classroom. In a narrower setting, Rivera (2022) studies minority peer effects at the Chicago Police Academy.

Third, above and beyond the econometric benefits of randomization of minority students, to the best of our knowledge, this is the first study that examines effects on behaviors and investments of students, parents, and teachers together with effects on test scores. This allows us to directly shine a light on the role that peers play in an education production function. Some prior work considered exogenous and endogenous peer effects simultaneously (De Giorgi, Pellizzari and Redaelli, 2010, Ross and Shi, 2022) while Green, Haaland and Vaag Iversen (2022) point out that Norwegian students have improved English language skills likely due to exposure to English-speaking peers outside the classroom. Other studies documented teachers' responses to class composition. For example, Lavy and Schlosser (2011) show that an increase in the proportion of girls in the classroom leads to improved inter-student and student-teacher relationships as well as lower teacher fatigue.[3] Finally, there are relevant papers using structural models of child skill formation that highlight the importance of accounting for parental investment responses to changes in their child's peer group or peer interactions (Agostinelli, 2018, Agostinelli et al., 2024, Boucher et al., 2023). Our results support the idea that parents' endogenous responses to classroom composition are relevant for modeling the education production while extending prior work by also incorporating student and teacher inputs.

Fourth, our work contributes to the literature on the effects of school racial, ethnic, and linguistic diversity on student achievement. This literature has produced inconsistent empirical evidence and has been primarily focused on the United States. Card and Rothstein (2007) find a strong association between racial segregation in neighborhoods and schools and the Black-White gap in SAT scores. Angrist and Lang (2004) analyze spillovers generated by school integration policies, and find short-lived, small to null effects on test scores. This null effect is confirmed in recent work by Setren (2022). Hanushek, Kain and Rivkin (2009) estimate the effect of studying in racially diverse schools, keeping school quality constant, and find large negative effects for Blacks, especially higher ability Black students, and small negative effects for Whites and other non-White minorities. A related but separate strand of literature focusing on ethnolinguistic diversity in schools has likewise yielded mixed findings (e.g. Cho, 2012, Diette and Oyelere Uwaifo, 2014, 2017, Geay, McNally and Telhaj, 2013, Tonello, 2016, Ohinata and Van Ours, 2013, 2016, Friesen and Krauth, 2011, Gould, Lavy and Paserman, 2009, Jensen and Rasmussen, 2011, Ahn and Jepsen, 2015, Hunt, 2017). Unlike our work, these studies do not rely on random assignment and are limited in their ability to directly observe mechanisms. In that regard, our study is closer to three recent small-scale experiments focused on the effect of exposure to refugees on pupils' attitudes, prejudice and friendship formation (Boucher et al., 2020, Alan et al., 2021) and on teachers' prejudice (Alan et al., 2020).

Fifth, our model and empirical results also highlight the potential role of biased subjective expectations in generating the observed data patterns. In this regard, our study contributes to the

[3] In the literature on ability tracking, Duflo, Dupas and Kremer (2011) show that the effects of ability tracking can be partly explained by teachers targeting instruction. On the other hand, Booij, Leuven and Oosterbeek (2017) find that teachers do not seem to change their behaviors in response to ability tracking in tutorials.

growing literature on the negative consequences of stereotyping, discrimination and prejudice in education. This literature shows that teacher expectations affect student educational outcomes (Papageorge, Gershenson and Kang, 2020), that teacher gender stereotyping could reduce girls' achievement (Rakshit and Sahoo, 2023), that teacher stereotyping of one minority group could negatively impact other minority groups (Shi and Zhu, 2023), and that teacher prejudice could increase peer violence (Alan et al., 2020). Relatedly, Anderberg et al. (2024) show that in-group bias peaks in culturally polarized classrooms where both groups are large and have different religious or language backgrounds.[4]

Finally, we provide some of the first evidence on education of Indigenous children, a group that has largely been omitted from research due to data limitations. Friesen and Krauth (2010) study outcomes of Aboriginal students in Canada from exposure to higher share of Aboriginal peers but they do not consider effects on majority students. Barber and Jones (2021) document robust achievement gaps between Indigenous and non-Indigenous students in Canada while Jones (2023) shows that cuts to post-secondary funding for Indigenous students lead to reductions in their educational attainment and labor supply. Our findings enrich this literature by documenting the consequences of and potential mechanisms behind integrating Indigenous children into schools attended predominantly by non-Indigenous students. This population is of particular policy importance given that Indigenous Peoples account for 5% of the global population but as much as 15% of the world's extremely poor (Hall and Patrinos, 2014).

We view our results as having two main policy implications. First, the negative peer effects we document suggest a potential hidden educational cost of policies attempting to integrate minority students in majority dominated schools. Thus, policy makers might want to pro-actively consider compensatory interventions that could minimize these effects. For example, Morales (2022) suggests that additional funding could be one effective offsetting policy. Second, our findings on endogenous responses of students, parents, and teachers highlight the need for broad policies addressing multiple stakeholders in the education production function. For example, Alesina et al. (2018) show that making teachers aware of their bias could decrease grading bias while Tumen, Vlassopoulos and Wahba (2023) show that teacher training programs on diversity awareness improve attendance and academic achievement of refugee students. Given our results, policy makers might consider providing similar resources to students and parents.

[4] In the broader economic context, Glover, Pallais and Pariente (2017) show that manager bias negatively affects minority job performance, Tabellini (2020) shows that immigration to the U.S. in the early twentieth century led to hostile political reactions among natives while Bazzi et al. (2019) presents evidence that inflows of out-group individuals could lead to intergroup conflict. Our model shows one way in which stereotypes can drive the negative effects of minority peers.

# 2 Theory: Peer Effects with Endogenous Responses

## 2.1 Definitions and baseline peer effects model

There are two types of students in each classroom: majority ($M$) and minority ($m$). The number of students is $n$ so that $n^M + n^m = n$. Denote by $q = n^m/n$, the fraction of minority students in a classroom and thus by $1 - q = n^M/n$, the fraction of majority students in a classroom.

### 2.1.1 Utility function

The utility of a student $i$ belonging to either the minority ($m$) or majority ($M$) group and choosing education effort $y_s^i$ is a function of both a private and a social component.[5] It is given by:

$$U_s^i = b^i y_s^i - \frac{1}{2}\left(y_s^i\right)^2 + \phi y_s^i \overline{y}_s^{-i}, \tag{1}$$

where $\overline{y}_s^{-i}$ is the average student effort in the classroom *leaving out* $i$, $0 < \phi < 1$ is the intensity of the spillover effects, and $b^i$ is student $i$'s marginal private benefit from education effort. We have $b^i = \mathbf{x}^i\gamma + \varepsilon^i$, where $\mathbf{x}^i$ is a $(1 \times k)$ vector of $k$ characteristics and $\gamma$ is a $(k \times 1)$ vector, so that $\mathbf{x}^i\gamma = \sum_{l=1}^{k} x^l \gamma^l$ reflects the *observable characteristics*, such as gender, socioeconomic status, etc. and $\varepsilon^i$, the *unobservable characteristics*, such as talent and motivation, of student $i$, independently from the influence of other students in the classroom.

This utility function has two terms. The first term, $b^i y_s^i - \frac{1}{2}\left(y_s^i\right)^2$, corresponds to the utility of exerting $y_s^i$ units of effort when there is *no interaction* with other children. The second term, $\phi y_s^i \overline{y}_s^{-i}$, captures the *spillover effects* that student $i$ experiences from the average effort of their classmates. The first-order condition with respect to effort for student $i$ is given by:

$$y_s^i = b^i + \phi\, \overline{y}_s^{-i} \tag{2}$$

or equivalently

$$y_s^i = \mathbf{x}^i\gamma + \phi\, \overline{y}_s^{-i} + \varepsilon^i. \tag{3}$$

which is the standard linear-in-means (LIM) model.

### 2.1.2 Equilibrium

In order to gain additional intuition from the model and derive closed-form solutions to the equilibrium of this game, we assume that all majority students have the same characteristics and all minority students have the same characteristics, that is, $b^i = b^M$ for all majority students and

[5] Subscript $s$ stands for "student" while superscripts $m$ and $M$ refer to minority and majority, respectively.

$b^i = b^m$ for all minority students. We later discuss how this assumption is relaxed in the empirical analysis. Under this assumption, there are only two education effort levels: one for each of the $n^M$ majority students, denoted by $y_s^M$, and one for each of the $n^m$ minority students, denoted by $y_s^m$. The average education effort in the classroom is then equal to:

$$\bar{y}_s = q y_s^m + (1-q)\, y_s^M.$$

For student $i$ that is either a minority ($m$) or majority ($M$), equation (2) can now be written as:

$$y_s^i = b^i + \phi \left[ q y_s^m + (1-q)\, y_s^M - \frac{y_s^i}{n} \right]. \tag{4}$$

We obtain:[6]

**Proposition 1** *Consider a peer-effect spillover model with a population of minority and majority students. Then, there is a unique interior equilibrium in which study efforts for each type of students are given by*

$$y_s^{M*} = \frac{(1-\phi q)\, b^M + \phi q b^m}{1-\phi}, \tag{5}$$

$$y_s^{m*} = \frac{[1-\phi(1-q)]\, b^m + \phi(1-q) b^M}{1-\phi}. \tag{6}$$

*Moreover, if $b^m < b^M$, then the higher is the fraction of minority students in a classroom, the lower is the study effort of both majority and minority students in the classroom, that is,*

$$\frac{\partial y_s^{M*}}{\partial q} < 0 \textit{ and } \frac{\partial y_s^{m*}}{\partial q} < 0.$$

Indeed, if students from the minority group have lower $b^i$, that is, they have worse characteristics (e.g., have lower test scores at baseline and/or their parents are less educated or have lower income), then, for a student $i$, the higher is the fraction of minority students in their classroom, the lower is the average "quality" $\bar{y}_s^{-i}$ of students in their classroom, and the lower is $i$'s education effort because she obtains less spillovers from other students.

Clearly, our results will still hold if we relax the assumption that all minority students have the same $b^m$ and all majority students have the same $b^M$ but, instead, we assume that there is a distribution of $b^i$ for each type of students with the average characteristic of minority students being lower than that of majority students. We could also expect behavioral reactions of students to the fraction of minority peers even in the absence of real differences between $b^M$ and $b^m$ as long as there is the perceptions that those differences exist. This highlights the role of subjective expectations and stereotypes as a potential driving mechanisms, which we explore below.

[6] The proofs of all results in the theory section can be found in Appendix A.

## 2.2 Incorporating the responses of parents and teachers

We now incorporate the teachers' and the parents' roles in shaping students' education outcomes. First, the education production function that gives the test scores of student $i$ is given by:

$$S^i = \rho \left(y_s^i\right)^{\alpha_1} \left(y_p^i\right)^{\alpha_2} \left(y_t^i\right)^{\alpha_3}, \tag{7}$$

where the subscripts $p$ and $t$ stand for "parent" and "teacher", respectively. We assume $0 < \alpha_1 < 1$, $0 < \alpha_2 < 1$, $0 < \alpha_3 < 1$, with $\alpha_1 + \alpha_2 + \alpha_3 = 1$ while $\rho > 0$ is the efficiency of the education production function. $S_i$ is student $i$'s test score, $y_p^i$ is the effort of student $i$'s *parent* (that is, how much time and resources the parent spends in educating their offspring $i$) while $y_t^i$ is the *teacher*'s effort in and perception about the classroom where $i$ studies (that is, how much time and effort the teacher spends preparing, managing, teaching, and taking care of their students). Observe that

$$\frac{\partial S^i}{\partial y_s^i \partial y_p^i} = \alpha_1 \alpha_2 \rho \left(y_s^i\right)^{\alpha_1 - 1} \left(y_p^i\right)^{\alpha_2 - 1} \left(y_t^i\right)^{\alpha_3} > 0,$$

$$\frac{\partial S^i}{\partial y_s^i \partial y_t^i} = \alpha_1 \alpha_3 \rho \left(y_s^i\right)^{\alpha_1 - 1} \left(y_p^i\right)^{\alpha_2} \left(y_t^i\right)^{\alpha_3 - 1} > 0.$$

Indeed, the more the parent or the teacher exerts effort, the higher is the impact of the student's effort on their test score. Observe also that

$$\frac{\partial S^i}{\partial y_p^i \partial y_t^i} = \alpha_2 \alpha_3 \rho \left(y_s^i\right)^{\alpha_1} \left(y_p^i\right)^{\alpha_2 - 1} \left(y_t^i\right)^{\alpha_3 - 1} > 0,$$

which means that the higher is the teacher's effort in the classroom, the higher is the impact of parent's effort on their child's test score. In summary, student, parent and teacher efforts are all *q-complements* to one another in their impact on student's test scores.[7]

The *timing* is as follows. In the first stage, both parents and teachers decide how much effort to exert. In the second stage, each student decides how much study effort to make.[8]

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[7] If instead of (7), we had chosen a CES education production function, that is,

$$S^i = \left(\alpha_1 \left(y_s^i\right)^{\rho} + \alpha_2 \left(y_p^i\right)^{\rho} + (1 - \alpha_1 - \alpha_2) \left(y_t^i\right)^{\rho}\right)^{1/\rho},$$

then, this would not always be true. Indeed, in this case, it is easily verified that

$$\rho \gtreqless 1 \Leftrightarrow \frac{\partial S^i}{\partial y_s^i \partial y_p^i} \lesseqgtr 0 \text{ and } \frac{\partial S^i}{\partial y_s^i \partial y_t^i} \lesseqgtr 0 \text{ and } \frac{\partial S^i}{\partial y_p^i \partial y_t^i} \lesseqgtr 0.$$

In other words, if $\rho < 1$ ($\rho > 1$), student's and parent's (or teacher's) efforts will be complements (substitutes) and parent's and teacher's efforts will also be complements (substitutes) in their impact on student's test scores. We note that substitution is inconsistent with our empirical findings, and thus we do not develop this specific model further.

[8] In Section 3 we discuss several institutional features that make this timing natural in our context. For example, we show that already at the baseline teachers have preconceptions about minority students (see Appendix Table C.1) and it is reasonable to assume those preconceptions shape teachers' behavior from the start. Therefore, teachers can easily be thought of as first movers. Parents can also be thought of as first movers since their behavior can be shaped by

As usual, we solve the model backwards. In the previous section, we already solved the second stage. By plugging the values in equations (5) and (6), we obtain:

$$S^M = \rho\,(1-\phi)^{-\alpha_1}\left[(1-\phi q)\,b^M + \phi q b^m\right]^{\alpha_1}\left(y_p^M\right)^{\alpha_2}\left(y_t^i\right)^{\alpha_3}, \tag{8}$$

and

$$S^m = \rho\,(1-\phi)^{-\alpha_1}\left([1-\phi(1-q)]\,b^m + \phi(1-q)b^M\right)^{\alpha_1}\left(y_p^m\right)^{\alpha_2}\left(y_t^i\right)^{\alpha_3}. \tag{9}$$

### 2.2.1 Parent's effort choices

The utility of student's $i$ parent is given by:

$$U_p^i = S^i - C(y_p^i),$$

where $C(y_p^i) > 0$ is the cost of effort of student's $i$ parent. Indeed, each parent cares about the test score of their offspring. In principle, one could assume that the marginal cost of parental effort is different between the majority and the minority groups, but this will just complicate our analysis without changing the main conclusions. For tractability, we assume that $C(y_p^i) = y_p^i$, so that

$$U_p^i = S^i - y_p^i. \tag{10}$$

Denote

$$\Delta^M(q) := (1-\phi q)\,b^M + \phi q b^m \text{ and } \Delta^m(q) := [1-\phi(1-q)]\,b^m + \phi(1-q)b^M. \tag{11}$$

Using the optimal efforts of students in equations (5) and (6) and using (11), we obtain:

$$U_p^M = \rho\,(1-\phi)^{-\alpha_1}\left(\Delta^M(q)\right)^{\alpha_1}\left(y_p^M\right)^{\alpha_2}\left(y_t^i\right)^{\alpha_3} - y_p^M. \tag{12}$$

and

$$U_p^m = \rho\,(1-\phi)^{-\alpha_1}\left(\Delta^m(q)\right)^{\alpha_1}\left(y_p^m\right)^{\alpha_2}\left(y_t^i\right)^{\alpha_3} - y_p^m. \tag{13}$$

By maximizing these utility functions, we obtain:

$$y_p^{M*}(q) = Z_1^{1/\alpha_2}\left(\Delta^M(q)\right)^{\alpha_1/(1-\alpha_2)}\left(y_t^i\right)^{\alpha_3/(1-\alpha_2)}, \tag{14}$$

and

$$y_p^{m*}(q) = Z_1^{1/\alpha_2}\left(\Delta^m(q)\right)^{\alpha_1/(1-\alpha_2)}\left(y_t^i\right)^{\alpha_3/(1-\alpha_2)}, \tag{15}$$

knowledge about the classroom composition from the very beginning of junior high school (see Section 3.1). Students in our setting are more easily thought of as second movers since their study effort is measured in classroom and therefore after classroom interactions have developed.

where

$$Z_1 := (\alpha_2\rho)^{\alpha_2/(1-\alpha_2)} (1-\phi)^{-\alpha_1\alpha_2/(1-\alpha_2)}. \tag{16}$$

### 2.2.2 Teacher's effort choices

The teacher of student $i$ maximizes the test score of *all* students in their classroom. In the classroom there are $n = n^M + n^m$ students. The utility of teacher $i$ (meaning the teacher in classroom where $i$ belongs to) is given by:

$$U_t^i = \sum_{i=1}^{n} S^i - y_t^i \tag{17}$$

Using equations (8) and (9), we obtain:

$$\begin{aligned} U_t^i &= n^M S^M + n^m S^m - y_t^i \\ &= n^M \rho (1-\phi)^{-\alpha_1} \left(\Delta^M(q)\right)^{\alpha_1} \left(y_p^M\right)^{\alpha_2} \left(y_t^i\right)^{\alpha_3} \\ &\quad + n^m \rho (1-\phi)^{-\alpha_1} \left(\Delta^m(q)\right)^{\alpha_1} \left(y_p^m\right)^{\alpha_2} \left(y_t^i\right)^{\alpha_3} - y_t^i. \end{aligned}$$

By maximizing this utility function, we obtain:

$$y_t^{i*}(q) = \frac{(n\alpha_3\rho Z_1)^{(1-\alpha_2)/\alpha_1}}{(1-\phi)^{(1-\alpha_2)}} \left[(1-q)\left(\Delta^M(q)\right)^{\alpha_1/(1-\alpha_2)} + q\left(\Delta^m(q)\right)^{\alpha_1/(1-\alpha_2)}\right]^{(1-\alpha_2)/\alpha_1}. \tag{18}$$

### 2.2.3 Parents' and teacher's effort choices: Equilibrium

We have the following result:

**Proposition 2** *Consider a two-stage model where, in the first stage, parents and teachers decide upon their effort while, in the second stage, students decide how much study effort to exert. There is a unique equilibrium in which the teacher's effort is given by* (18) *and parents' efforts by* (14) *and* (15). *Moreover, based on Proposition 1, if $b^m < b^M$, then the higher is the fraction of minority students in a classroom, the lower is the study effort and the test scores of both majority and minority students in the classroom, that is,*

$$\frac{\partial y_s^{M*}}{\partial q} < 0,\ \frac{\partial y_s^{m*}}{\partial q} < 0,\ \frac{\partial S^{M*}}{\partial q} < 0,\ \text{and}\ \frac{\partial S^{m*}}{\partial q} < 0.$$

*Moreover, if $b^m < b^M$, then the higher is the fraction of minority students in a classroom, the lower is the parental effort of both majority and minority students as well as the teacher's effort, that is,*

$$\frac{\partial y_p^{M*}(q)}{\partial q} < 0,\ \frac{\partial y_p^{m*}(q)}{\partial q} < 0\ \text{and}\ \frac{\partial y_t^{i*}(q)}{\partial q} < 0.$$

*Finally, if $b^m = b^M$, then q (the fraction of minority students in the classroom) has no impact on the test scores of students as well as on the efforts of students, parents and teachers.*

The intuition of these results is as follows. First, observing the classroom composition, parents and teachers, who maximize students' test scores, decide on the effort/inputs in their interactions with the children. Second, students decide their study effort based on peer interactions and thus on the spillovers they obtain from each other (Proposition 1). Note that students can also respond to parental and teacher inputs which in turn can lead to additional changes in behaviors of parents and teachers in a repeated game. Since the minority students are disadvantaged in terms of observable characteristics compared to the majority students, when $q$, the fraction of minority students in a classroom increases, the average study effort in the classroom goes down, including that of their own child. It also lowers parents' effort. Similarly, the teacher who maximizes the sum of the test scores of all students in her classroom, observes first the composition of minority and majority students in her classroom. When $q$ increases, because effort is costly, teachers find it optimal to reduce their effort (their engagement with students) because students make less effort (i.e., study less). Finally, since students' test scores are determined by their own study effort as well as investments of their parents and teachers and since all these inputs complement each other, an increase in $q$ reduces students' test scores (Proposition 2).

Although our model is general, it hinges on two critical assumptions that (i) $b^i$ is observable and (ii) that $b^m < b^M$ statistically (e.g., higher poverty rates of minority versus majority students) or in expectations (via e.g., prejudice or discrimination). Therefore, it is most naturally applicable to demographic characteristics used in the extant literature (e.g., race, gender, immigration, language) but not necessarily to ability or disruption (other factors often considered in peer effects research) which are harder to gauge in the short-run. Nonetheless, through repeated interactions and learning the model can be adapted to the latter set of treatments in a medium-run.

# 3 Institutional Setting

## 3.1 Education in Taiwan

Compulsory education in Taiwan starts with primary school at age 6 and ends with junior high school around age 15, however, approximately 95 percent of students continue their education with either General or Vocational Senior High School or Junior College. Appendix Figure C.1 provides a simplified schematic of the Taiwanese education system. The educational curriculum is developed centrally by the Taiwanese Ministry of Education and has no tracking during the compulsory stage of education. It is centered around sciences and mathematics which is often credited as the reason why Taiwanese pupils are consistently placed at the top on international educational rankings (e.g., $4^{th}$ out of 72 countries in PISA 2015 in mathematics and science). Since the 1990s, public junior high schools have been managed at the municipal level and most students are assigned to and attend their local-catchment area school. Although currently there is a degree of school choice in admissions, this was much less common for the cohort we consider in this paper.

Crucial for our identification strategy, students are randomly assigned to classrooms within junior high schools. This rule is mandated by the central government. Details of this mandate can be found in the *Implementation Guideline for Class Assignment of Junior High School Students*, which was the relevant legislation for classroom assignment at the time.[9] The regulations describe in detail the procedure for forming classrooms in junior high schools and the invigilation arrangements made by municipal and county governments ensuring that the mandate is followed. The assignment of students to classrooms within schools is often referred to as "mixed-ability class grouping" or grouping for short. Junior high schools are required to implement and maintain this grouping in all grades. The grouping is conducted by municipality, city, or county government, or by each school if given approval to handle the process itself. There are three approved algorithms for assigning students to classrooms: (1) by "giving the students a test and then forming each class using an S-sequence listing of all the test scores"; (2) by a public drawing of lots; or (3) by using computer random number generation.[10] Within seven days of student assignment, the school allocates homeroom teachers ("Dao Shi") by public drawing of lots in a procedure invigilated by representatives of the teacher and the parents associations.[11] Implementation of the guidelines is high stakes as it is a major item monitored when conducting school evaluations as well as performance assessment and selection of principals. Education in schools with a significant presence of Indigenous students is further regulated by the *Education Act for Indigenous Peoples*, though importantly this act does not allow for exceptions to the random assignment of students to classrooms in junior high schools.[12]

[9] This regulation was later superseded by Article 12 of the *Primary and Junior High School Act* in 2004.

[10] The S-sequence listing assignment refers to sorting all students in a school by test performance and then sequentially assigning the best-performing student to Classroom 1, the second-best performing student to Classroom 2, and so on, returning to Classroom 1 after a student has been assigned to each available classroom. There is strict invigilation of these procedures, including the formation of "classroom grouping promotion committees". Schools that have been approved to handle the grouping themselves are subject to the same regulation, need to publicly announce the grouping process date and time, invite parents as observers, and have the process supervised by government personnel. Incoming or transfer students are assigned to classrooms based on drawing of lots or randomly generated numbers. If the number of classrooms is changed over time, the new classrooms should also be generated by the same procedure.

[11] Dao Shi are highly involved homeroom teachers that teach, guide and mentor their students throughout junior high school (see Cobb-Clark, Salamanca and Zhu, 2019). They are also expected to get to know each student personally, especially those experiencing disadvantage, and to use this relationship to support their students' academic performance. Finally, homeroom teachers are also parents' first port of call for discussing their children's academic performance e.g., they are available for parental phone calls outside of school hours and may visit students at home. One of our measures of teacher engagement indeed pertains to Dao Shi's assessment on how hard it is to manage their specific classroom.

[12] The *Education Act for Indigenous Peoples* defines Indigenous and Ethnic education, differentiating it from general education, defines Indigenous schools and classrooms across primary and secondary education, and sets the rules and exceptions that apply to them. The act also regulates a host of elements, including the minimum required funding for Indigenous education, the formation of preferential scholarships for overseas studies, special requirements for the appointment of teachers of Indigenous classrooms and the language they teach, and the governmental bodies in charge of curriculum development and policy implementation in Indigenous schools. The act does allow senior high

Exceptions from following the institutionalized randomization exist, however, and some schools may open arts, music, sports/physical education or gifted classrooms with authorization from the relevant municipality, city, or county government. In those schools some students can be assigned to these special classrooms and the remainder of students are randomly assigned to the regular classrooms. Nonetheless, given the selective random assignment and the fact that we do not know which classes are exempt, this would invalidate our balancing tests and estimation. In Section 6.2 we discuss how we deal with this institutional issue. Classroom assignment is permanent for almost all students in the sense that, net of extraordinary circumstances like moving to a different municipality or (physical) conflict within a classroom, students typically remain with their initially assigned classmates throughout all three years of junior high school. In Section 6.2 we discuss evidence that this is the case.

At the end of compulsory education, students take the National Basic Competence Test and its results determine competitive admissions to the next educational level. Consequently, students, parents and teachers spend considerable time, money and effort preparing for the exam. For example, schools regularly organize practice exams and other forms of preparation while parents are known to hire private tutors in mathematics, English and sciences. The latter is often facilitated through "cram schools" which are private extra-curricular institutions preparing specifically for high stakes centralized examinations. In our data we do not observe these exams, yet it is worth mentioning that, as part of affirmative action, Indigenous students are eligible for preferential treatment in admissions to senior high schools via bonus points added to their exam.

## 3.2 Minorities in Taiwan

The population of Taiwan is approximately 23 million, mostly descendants of Han Chinese. According to the 2010 Census, 95% of the population was Han Taiwanese, 2.5% was Taiwanese Indigenous Peoples, and 2.5% of individuals were of other origins (mostly immigrants from other Asian countries). At the same time, Han Taiwanese do not constitute a homogeneous group. While the vast majority of those are Hoklo descendants from Fujian (approximately 70%), there is a significant minority of Hakka descendants mostly from eastern Guangdong (approximately 15%). The remaining Han Taiwanese are Waishengren, which defines people who migrated from mainland China to Taiwan between 1945 and 1949 during the relocation of the Republic of China government.

Indigenous Peoples of Taiwan are the minority group of interest in this paper. They are the native inhabitants of the island of Taiwan, and descend from those who lived on the island as far as 6,000 years ago. Although there are many distinct tribes of Indigenous Peoples, with their own histories and customs, the Taiwanese government officially recognizes 16 distinct groups within this broader Indigenous umbrella. The share of Indigenous students among school-age children

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schools with a minimum share of Indigenous students to exceptionally implement a single-track system, but makes no differential provisions for junior high schools, where there is no tracking.

is about 4%, however, they are not distributed evenly across the country. Appendix Figure C.2 presents the fraction of 10 to 14 year old children who are Indigenous by county of residence. The vast majority of those children live in the eastern parts of Taiwan, where their concentration is as high as 45%, but there is also a non-trivial share of Indigenous children living in counties surrounding Taipei —the capital. The vast majority of Indigenous Peoples are fluent in Mandarin and many of them speak at least one Indigenous language. Yet they are culturally and physically very distinct, and extremely disadvantaged compared to other groups in Taiwan, a point that we discuss further below.

Hoklo are primarily descendants of people from southern Fujian who migrated to the island before the start of the Japanese occupation in 1895, with early migrations starting at the beginning of the $17^{th}$ century. They currently account for about 70% of the Taiwanese population. They have the strongest sense of Taiwanese identity among the three Han Chinese groups. For example, in 1999, 75% of Hoklo identified as exclusively Taiwanese, while this share was 58% and 32% for Hakka and Waishengren, respectively (Tsai, 2007). Many Hoklo people are bilingual, speaking both Mandarin (the most commonly spoken language in Taiwan) and Taiwanese Hokkien (also called Taiwanese Hoklo).

Hakka are primarily descendants of people who migrated from the Guandong province of China in the mid-$17^{th}$ century, at the end of the Ming dynasty and the beginning of the Qing dynasty. They currently account for about 15% of the Taiwanese population. Historically, the Hakka faced discrimination from other Chinese ethnic groups that sometimes led to violence, both in mainland China and in Taiwan. These frictions also led to, for example, mass-killings in Hakka villages. The Hakka likewise speak a distinct language. Furthermore, Hakka and Hoklo have similar demographic and socioeconomic characteristics and, despite clear cultural differences, the two groups are ethnically close to one another. These cultural differences resonate especially in food, architecture, arts and crafts but also in social behaviors and hierarchies. For example, Hakka historically put much more emphasis on education making them "the perfect bureaucrats" and they are the only Han ethnicity where women never bound their feet.

Waishengren are predominantly Han Chinese descendants of Chiang Kai-shek's retreating army, who arrived from mainland China between the end of World War II in 1945 and the end of Chinese Civil War in 1949. For this reason they are sometimes refereed to as "mainlanders" and we use the two terms interchangeably. They are the smallest of the three Han Chinese groups comprising about 10% of the population of Taiwan. They likewise have similar characteristics to Hoklo and Hakka.

In our main analyses, we consider all three Han Chinese groups as the majority. We also exclude children of individuals from other countries. In a separate placebo analysis, we use Hakka as a separate minority group that is culturally distinct but observationally similar to the other Han Chinese groups.

# 4 Data

We use data from the Taiwanese Education Panel Survey (TEPS), a project jointly funded by the Taiwanese Ministry of Education, the National Science Council, and the Academia Sinica. The TEPS is a nationally representative longitudinal survey of the education system in junior high school, senior high school, vocational senior high school, and junior college. It is a multi-respondent survey, collecting linked information on students, parents, teachers, and school administrators.

We focus on the junior high school sample of the TEPS because it allows us to measure student ability and educational inputs paired with random assignment to classrooms in Grade 7. The TEPS junior high school sample includes information on more than 20,000 students, their parents, their teachers, and their school administrators over two waves. The first wave was collected in early September 2001 at the very beginning of students' first year of junior high school, and right after their random assignment to classrooms within their school. The second wave was collected in 2003, at the beginning of the students' last year of junior high school.

There are three key features of the TEPS that make it the ideal dataset for our study. First, its sampling framework allows us to observe a random sample of classmates in each junior high school classroom included in the survey. TEPS followed a stratified nested sampling procedure where first 333 randomly selected junior high schools were sampled (45 percent of all junior high schools in the country at the time), with sampling strata for urban and rural areas as well as public and private schools. In each of these schools an average of three classrooms of first-year junior high school students were then randomly sampled. In each of these classrooms, around 15 students were then randomly sampled. Since the mandated maximum class size at the time was 35 students per class, this generally amounts to observing a randomly chosen half of the classroom.[13,14] The sampling and random selection of students was done by the research team and neither teachers nor administrators took part in this process.

Second, students in the TEPS take a standardized test in waves 1 and 2 called the Comprehensive Analytical Ability test. It measures students' cognitive ability and analytical reasoning, and was specifically designed to capture gradual learning over time. The test contains 75 multiple-choice questions, covering general reasoning, mathematics, Mandarin and English. These were

[13] This sampling framework is similar to that of the National Longitudinal Study of Adolescent to Adult Health (Add Health), a panel study of middle and high school pupils in the United States. Add Health is unique in collecting friendship ties and in observing multiple cohorts of students in each school, which makes it particularly appealing for peer effect and networks research (see e.g., Calvo-Armengol, Patacchini and Zenou, 2009, Bifulco, Fletcher and Ross, 2011, Card and Giuliano, 2013, Elsner and Isphording, 2017, Patacchini, Rainone and Zenou, 2017, Agostinelli, 2018).

[14] During the initial data collection researchers noticed an uneven distribution of Indigenous students in certain schools, particularly in high-density Indigenous areas. To ameliorate this problem without compromising the sampling scheme, entire classrooms were surveyed in a subset of schools. This should not pose a problem for our identification strategy given the random assignment to classrooms within schools.

taken from an extensive set of questions, some of which were adapted from other international standardized tests, and some others which were developed by education and field experts in Taiwan. This is a low-stakes test constructed for the purpose of the survey, which has no bearing on student's subsequent academic careers. Importantly, however, the tests were administered by the research team, they were externally graded and the scores were not disclosed to either students, parents, teachers or school administrators. The main goal of the test was to accurately measure children's academic achievement. The anonymity and low-stakes characteristics of the test reduce concerns that students might be differently affected by test-taking stress itself. We standardize these test scores to have a mean of 0 and a standard deviation of 1 in each wave of the test.[15]

Third, the TEPS provides a wealth of questions measuring student behavior, attitudes and beliefs in and outside the school environment, parent-child interactions and parental investments, as well as detailed information on teachers and school administrators. We use this information to construct aggregate indices of student, parent, and teacher effort and engagement. To do so, we first construct scales that capture educational inputs from each of these agents across several dimensions. Each scale combines several questions measuring these behaviors (e.g., the scale capturing student's study hours combines measures of study time in school, out of school, during summer breaks, etc.). To the extent that each of these questions is subject to classical measurement error, combining them into scales will reduce the impact of this measurement error on the precision of our estimates. We then aggregate these scales into indices that combine all relevant educational inputs from each agent, as follows:

- Student effort: study hours
- Parental investments: private tutoring, time spent with child, emotional support
- Teacher engagement: teacher effort, ease to manage the class

Our student effort index therefore uses just a single scale of study hours (which itself combines several measures of study hours), while for parental investments and teacher engagement our indices combine multi-item scales across more dimensions. To ensure that each index only captures investments conducive to test scores (and therefore to bring our empirical analysis closer to our model) we estimate how each scale in the index relates to test scores using wave 1 data

[15] It is reasonable to ask if the low-stakes tests we use as outcomes are predictive of any meaningful outcomes. On one hand, to the extent that they are inconsequential for students and teachers, we might expect lesser effort compared to high-stakes tests. This could be viewed either positively since it reduces incentives for manipulation or negatively if students do not exert effort reflecting their true cognitive ability. We are less concerned about the latter as Gneezy et al. (2019) show that Chinese students, unlike their US counterpart, tend to exert effort even on low-stakes tests. Although we use Taiwanese data, we note that Taiwan like Shanghai (and unlike the US) was ranked very high in the 2018 PISA test. Furthermore, in Appendix Table C.2 we show that wave 2 test scores are strong predictors of future academic and labor market success. For example, one standard deviation increase in test scores is associated with a 26.3 percentage points increase (53%) in the likelihood of being enrolled or having finished university by 2009 and a 11.2 percentage point increase (24%) in the likelihood of earning above-median income in 2013 for those employed.

and then apply those estimates to wave 2 scales. Appendix B provides detailed description of the items we use and the process behind the scale and aggregate index construction.

The data also includes rich demographic information on students and their parents which allows us the assign students to our minority (at least one parent is Indigenous) and majority (the remainder of the sample excluding children of foreign born parents or those who themselves are foreign born) groups.[16]

## 5 Connecting Institutions and Data with the Model

Using Propositions 1 and 2, we can now establish the impact of $q$, the fraction of minority students in a classroom, on $S^i$, the test score of student $i$ who can be either Han Chinese ($M$) or Indigenous ($m$). For that, recall that in Section 2, we derived the equilibrium values of $y_s^{i*}(q)$ (given by equations (5) and (6)), $y_p^{i*}(q)$ (given by equations (14) and (15)), and $y_t^{i*}(q)$ (given by equation (18)). Recall also that plugging these into equation (7), we obtain

$$S^{i*}(q) = \rho \left(y_s^{i*}(q)\right)^{\alpha_1} \left(y_p^{i*}(q)\right)^{\alpha_2} \left(y_t^{i*}(q)\right)^{\alpha_3}. \tag{19}$$

Finally, by assuming that $b^m < b^M$ and taking the partial derivative of $S^{i*}(q)$ with respect to $q$ we obtain

$$\frac{\partial S^{i*}(q)}{\partial q} < 0, \tag{20}$$

which just restates one of the key results of Proposition 2. In words, our model predicts that due to the negative effects of exposure to minority students on study effort of students, investment of parents and engagement teachers, an increase in the share of minority students in the classroom decreases the test scores of both majority and minority students.

Before delving into the econometric analysis, let us explain how this model matches the Taiwanese institutional setting. First, in the model, the majority group (type $M$) corresponds to the Han Chinese while the minority group (type $m$) refers to the Indigenous Peoples of Taiwan (see Section 3). Second, the key assumption to obtain the results in Proposition 1 and 2 is that $b^m < b^M$, that is, minority students have worse observable characteristics than majority students, which is verifiable with the data. Indeed, Appendix Table C.3 shows that the Indigenous students

[16] There are two limitations of the TEPS. First, although some of the junior high school students are longitudinally followed well into early adulthood, the full panel is only observed at the beginning of the first and the last year of junior high school. This is due to the survey design where only a selected subsample of junior high school students are followed in subsequent waves, limiting their usefulness. Second, we cannot observe initial admission rosters into junior high schools, as these data were not collected in the TEPS. However, since there is strict invigilation, public information and enforcement of classroom grouping (see Section 3.1) the differences between the originally rostered classrooms and those classrooms as sampled in the TEPS are likely minimal. In any case, early selectivity related to the assignment of minority students to classrooms within schools should be detected in our balancing tests, as presented in Section 6.2.

are much more disadvantaged than the majority students: they have a 0.72 Standard Deviation (SD) lower baseline scores, 11 percentage points lower university aspirations, and 8 percentage points lower subjective expectations about their ability to go to college. In addition, 30% of them come from families in the lowest income bracket (compared to 11% for the majority), 80% of them have parents with low level of education (compared to 71% for the majority), 60% of them have parents who had financial difficulty in the past 10 years (compared to 28% for the majority), and there is even a 28 percentage points gap in the likelihood that their parents report being in good health. Although attenuated, these differences hold when we include school fixed effects—effectively using only the quasi-experimental variation that identifies our causal estimates.

For our main analyses, we consider all other Han Chinese as "the majority". We take this approach even though, as explained in Section 3, in Taiwan there are two types of minorities officially recognized by the government: the Hakka and the Indigenous Peoples of Taiwan. However, as shown in Appendix Table C.4, Hakka and Hoklo have similar demographic and socioeconomic characteristics; i.e., the two groups are observationally close to one another and therefore the assumption that $b^m < b^M$ largely does not hold for the difference between Hoklo and Hakka students.[17]

One useful but inconsequential simplification in our model is that $b^m < b^M$ needs to be observed by the students, parents, and teachers. In principle, one could imagine that even teachers cannot know the true ability or socioeconomic status of their assigned pupils early on during the first year of junior high school. Our theory, however, carries forward when these differences hold in expectations, even in the absence of actual differences in student endowments. That is, our models' predictions will hold if minority students are expected, based on taste based or statistical discrimination, to be worse than majority students, even if their actual characteristics are similar.[18]

We argue that systematic differences in how Indigenous students are perceived by others are one plausible mechanism behind our results. Although in our data we do not observe perceptions of students or parents, we do have information that allows us to test whether teachers hold systematic (and unwarranted) differences in perceptions between Indigenous and non-Indigenous students.

[17] Appendix Table C.4 also shows the sharp insularity of the different groups in terms of their spoken languages. Most students in all groups are fluent in Mandarin, though Indigenous students less so than others (88% compared to 92% for Hakka and Hokklo and 95% for Washingren). Over 40% of the Hokklo students and about a quarter of the Hakka and Washingren students are also fluent in Hokkien, compared to only 10% of Indigenous students. Furthermore, 17% of Hakka are fluent in their native language while 29% of Indigenous students are fluent in their native languages. Importantly, both of these language are very rarely spoken by children outside of these groups. Namely, only 1% of Hakka, Washingren, and Hoklo speak Indigenous language and similarly low percentages of non-Hakka students can speak Hakka.

[18] Many Indigenous Peoples of Taiwan have different physical appearance than Han Chinese although the degree of contrast will differ depending on a specific Indigenous group as well as associated non-physical attributes of daily life such as clothing or rituals. Similarly, in the US context, individuals who identify as African-Americans, Asian or Hispanic are most often physically different from White Americans. These differences would allow students, parents and teachers to ascribe "group-specific expectations" to minority students even if they observe little else about them.

In particular, homeroom teachers are asked to rate each student's problem-solving abilities at the beginning of the school year, before having the opportunity to meaningfully interact with them. The survey asks homeroom teachers to evaluate the students' abstract and logical thinking ability as "Excellent", "Above average", "Average", "Below average", 'Poor", and "I don't know". We combine the first two categories to create a discrete measure of whether the homeroom teacher believes a student has an above-average abstract and logical thinking ability. We then use this indicator variable to compare teachers' subjective expectations about minority and majority students.[19]

We present those results in Appendix Table C.1. Column 1 shows that, on average, teachers rate Indigenous students 12 percentage points lower than the majority students. This gap only increases when we control for school fixed effects (column 2) and thus leverage the random allocation of students to teachers within a school. Given that in Appendix Table C.3 we have documented large "objective" differences in ability between these two groups of students, it may be that differential perceptions are explained by true achievement differences. Recall, however, that teachers (or students or parents or administrators) were never informed about the performance of the pupils on the baseline (or subsequent) cognitive ability test. Column 3 verifies this and indeed the Indigenous penalty declines by about two-thirds. Nonetheless, the gap at 5 percentage points (or over 16%) remains statistically significant at conventional levels. At the same time, test scores are, as expected, positively correlated with teacher perceptions which suggests that (even early on) teachers can to some degree identify which students will or will not perform well academically. Finally, in column 4, when we include an interaction term between baseline test scores and the Indigenous dummy we find that it is negative and statistically significant and additionally the coefficient on the Indigenous dummy variable becomes more negative. This means that high-achieving Indigenous students suffer an increasing penalty when it comes to teacher expectations. These findings are consistent with teacher's negative perceptions about Indigenous student's ability and support the fact that $b^m < b^M$ also in expectations.

One concern could be that teachers discriminate against all minority students (not necessarily only Indigenous) based on e.g., cultural differences, or that we simply have a flawed perceptions measure. To address this concern, in Panel A of Appendix Table C.5 we show that similar bias does not exist for Hakka students. In fact, teachers systematically have more positive perceptions about the problem-solving abilities of Hakka students, whether we ignore school fixed-effects (Column 1) or absorb them (Column 2). Controlling for student academic aptitude (Column 3) reduces the favorable expectations to an insignificant 1 percentage point, and additionally controlling for the interaction between Hakka indicator and baseline academic achievement

[19] We code "I don't know" responses as missing values in these analyses. An alternative would have been to include these as zeros, which would effectively impute teacher uncertainty as considering the student to be of below-average ability. None of our results hinge on this choice, however; in the data 2% of students are rated as "I don't know" and this fraction is only slightly higher for Indigenous (3.2%) than for Han Chinese (1.92%) students. This suggests that, while teachers might be more uncertain in their perceptions about the former group, these differences are not substantial.

(Column 4) does not alter the conclusions. These results indicate that, unlike for Indigenous students, teachers hold accurate perceptions about Hakka students.

Another concern could be that teachers cannot accurately assess the potential ability of low-achieving students because of other correlated markers of disadvantage affecting even early behavior in class. Panel B of Appendix Table C.5 shows that teachers do not hold systematically biased perceptions against majority students with a similar levels of disadvantage as Indigenous students. After controlling for school fixed effects (column 2) the penalty in teacher-assessed problem-solving ability is only about half of the penalty reported in Appendix Table C.1 and it is statistically insignificant at conventional levels. It further declines when we control for student test scores and the interaction term (columns 3 and 4). For example, in column 4, both estimates on the Indigenous dummy and the interaction term are about one-third of equivalent coefficients in Appendix Table C.1, and neither is statistically significant at conventional levels. Overall we view these results as supporting the notion that teachers in our sample have biased expectations about the academic potential of Indigenous students which is consistent with the proposed theory.

Given the above, we have the following four predictions that we bring to the data:

(1) The higher is the fraction of Indigenous students in the classroom, the lower are the test scores of both the majority and the minority *students* (Propositions 1 and 2).

(2) The higher is the fraction of Indigenous students in the classroom, the lower are the *parents*' efforts for both majority and minority students (Proposition 2).

(3) The higher is the fraction of Indigenous students in the classroom, the lower is the *teachers*' engagement (Proposition 2).

(4) The fraction of Hakka students in the classroom has *no* impact on the test scores of either the majority or the minority *students* (Propositions 1 and 2).

# 6 Empirical Strategy

## 6.1 Estimating Equation

In order to evaluate the effects of exposure to minority students in the classroom, we bring our theoretical model to the data, which expressed in a linear form can be written as:

$$\begin{aligned} Score_{iscw_2} = \alpha_1 &+ \alpha_2 \overline{\%Indigenous_{iscw_1}^{-i}} \\ &+ \alpha_3 Indigenous_i + \alpha_4 \overline{\%Indigenous_{iscw_1}^{-i}} \times Indigenous_i \\ &+ \delta \mathbf{X}_{iscw_1} + \gamma_{sw_1} + \varepsilon_{iscw_2}, \end{aligned} \tag{21}$$

where $i$ stands for student, $s$ for school, $c$ for classroom, $w_1$ for the start of grade 7 (our baseline) and $w_2$ for the start of grade 9 (when we measure our outcomes). All our models include wave 1 school fixed effects ($\gamma_{sw_1}$), which restricts our identifying variation to across-classrooms and within-schools—that is, the level of randomization that the policy imposes. In our main specifications, we also include pre-assignment control variables, $\mathbf{X}_{iscw_1}$, which include students' test scores at the baseline as well as number of standard observable characteristics of students and parents. Thus, our preferred results have a value-added interpretation: compared to the achievement in grade 7, what is the effect of being exposed to more Indigenous students on test scores in grade 9? Since, in the main specifications, we run these regressions on a full sample of students, rather than just for the majority students, we also include student's own Indigenous status dummy and its interaction with the share of Indigenous students. We cluster standard errors at the level of the classroom in wave 1, because this is the level at which randomization occurs.[20] Our main coefficient of interest in Equation (21) is $\alpha_2$, which represents the causal effect of exposure to Indigenous students in the first two grades of junior high school on grade 9 test scores of majority students.[21] Note that $Score_{iscw_2}$ corresponds to $S^i$ in our theoretical model, $\overline{\%Indigenous^{-i}_{iscw_1}}$ to $q$ and $\mathbf{X}_{iscw_1}$ to $b^m$ and $b^M$. Thus, our model predicts that $\alpha_2$ should be negative and statistically significant, as per Proposition 2. Furthermore, $\alpha_4$ should be close to zero and statistically insignificant if Indigenous and non-Indigenous students are affected in similar way by an increase in $q$.

### 6.2 Identifying Assumption: Random Assignment to Classrooms

Our identification strategy exploits the random assignment of students to classrooms in Taiwanese junior high schools (see Section 3.1), which implies a randomly allocated exposure of majority students to Indigenous students. This means that the variable $\overline{\%Indigenous^{-i}_{iscw_1}}$ will be conditionally (on school fixed effects) independent of the error term $\varepsilon_{iscw_2}$ (implying $E[\varepsilon_{iscw_2}|\overline{\%Indigenous^{-i}_{iscw_1}}, \gamma_{sw_1}] = 0$). To support the assumption of random assignment, in Table 1 we show that our treatment of interest—classroom leave-out-share of Indigenous students—is uncorrelated with characteristics of students and parents at the baseline. Each coefficient comes from a separate regression based on modified Equation (21) where we regress the pre-assignment variables on $\overline{\%Indigenous^{-i}_{iscw_1}}$ and school fixed effects, and we exclude all

[20] Our conclusions remain unchanged if we instead cluster at the school-level, only marginally increasing the standard error of our treatment effect (column 3 of Table 2) from 0.139 to 0.157. Clustering standard errors at the school level allows for correlated errors across classrooms. We reach the same conclusions if we consider empirical p-values based on randomization inference (see Appendix Figure C.3).

[21] We focus on linear-in-means model in this paper but one could also consider alternative models such as bad apple, shining light, invidious comparison, boutique, focus, rainbow, single crossing, or subculture (see Hoxby and Weingarth (2005) for details). Many of these models are more suitable to studying ability peer effects rather than minority peer effects which is the focus of our work; especially given that most variation in our sample comes from the extensive margin. Almost 60% of our classrooms have no Indigenous students, 27% of classrooms have only one Indigenous student, and only about 13% of classrooms have two or more Indigenous students.

other controls.[22]

**Table 1:** Balancing Checks: Correlations between Pre-Assignment Characteristics and Exposure to Indigenous Students in the Classroom

| **Treatment:** | | **Share of Indigenous peers** | | |
|---|---|---|---|---|
| | Mean | Coef. | Std. err. | Obs. |
| **Pre-assignment Outcomes:** | | | | |
| Student test scores | Std | 0.076 | (0.220) | 8,517 |
| Female student | 0.49 | -0.022 | (0.097) | 8,517 |
| Student born before 1989 | 0.36 | 0.075 | (0.104) | 8,486 |
| Two-parent household | 0.90 | -0.015 | (0.063) | 8,505 |
| Household income > NT$50k/mo. | 0.42 | -0.113 | (0.103) | 8,472 |
| Stable income for 1+ household member | 0.89 | -0.021 | (0.070) | 8,499 |
| Parent(s) education is high school or less | 0.71 | -0.153 | (0.102) | 8,517 |
| Employed father | 0.88 | 0.134* | (0.073) | 8,210 |
| Parent(s) in good health | 0.58 | -0.008 | (0.107) | 8,514 |
| Prioritized studies since primary school | 0.27 | 0.107 | (0.093) | 8,467 |
| Reviews lessons since primary school | 0.17 | 0.038 | (0.085) | 8,460 |
| Likes new things since primary school | 0.41 | -0.013 | (0.116) | 8,447 |
| Was truant in primary school | 0.35 | 0.081 | (0.088) | 8,495 |
| Student had mental health issues in primary school | 0.47 | -0.032 | (0.087) | 8,491 |
| Had private tutoring before junior high | 0.65 | 0.077 | (0.127) | 8,428 |
| Family help with homework before junior high | 0.84 | 0.037 | (0.073) | 8,125 |
| Student quarreled with parents in primary school | 0.66 | -0.053 | (0.079) | 8,503 |
| Guryan et al. (2009) sorting t-statistic | | 1.1 | | |
| Jochmans (2020) sorting t-statistic | | 1.0 | | |
| LHS test of joint balancing (p-value) | | 0.586 | | |

Note: This table reports estimates from regressing student pre-assignment characteristics on the classroom share of Indigenous peers in wave 1. All regressions include wave 1 school fixed effects. Guryan, Kroft and Notowidigdo (2009) and Jochmans (2023) sorting statistics test whether minority students are more likely to be assigned to classrooms with other Indigenous students within schools, and their critical values are those of the *t*-distribution. LHS test of joint balancing (p-value) refers to the p-value of the F-test from regressing share of Indigenous peers on all pre-assignment covariates and school fixed effects, as suggested by Pei, Pischke and Schwandt (2019). Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

If we run these tests using the entire TEPS data, we do find evidence that Indigenous students are systematically assigned to classrooms with lower-ability peers, with other Indigenous students, and generally with students who have somewhat worse observable characteristics. This is, to some extent, not surprising given the institutional detailes described in Section 3.1. To address this issue, we adapt the algorithm by de Gendre and Salamanca (2020) to detect, characterize and isolate non-complier schools. This procedure identifies 19 small schools, which we exclude from

[22] We can augment these tests to allow for differential effects for Indigenous and non-Indigenous students by regressing baseline characteristics on own Indigenous status, $\overline{\%Indigenous_{iscw_1}^{-i}}$, the interactions of these two variables, and school fixed effects. In these regressions, among 17 level coefficients and 17 interaction coefficients only one estimate is statistically significant at conventional levels.

the analysis, where Indigenous students are disproportionately likely to be assigned to peers with low ability and low socioeconomic status. We then confirm that in these schools students are 1.4 times more likely to report being in a gifted class for science and 1.5 times more likely to report being in a gifted class for arts. These gifted classrooms are a marker of schools where the random assignment mandate is unlikely to be followed (see Section 3.1). The data do not include information on whether these schools have the official approval from the Taiwanese government for the formation of gifted classrooms, or if they are just using these *de facto*. In any case, we exclude these observations from all analyses going forward.[23]

As documented in Table 1, after excluding these schools our balancing tests strongly support the hypothesis that Indigenous students are randomly assigned to classrooms within schools. Out of 17 tests only one yields a coefficient statistically significant at the 10% level, which is less than what we would expect due to pure chance.[24] Furthermore, the estimates are quantitatively small, never exceeding 4.0% effect size relative to the dependent variable mean for a 10pp increase in the fractions of Indigenous students. We also implement two additional tests to ensure the validity of our identifying assumption. First, we use diagnostics developed by Guryan, Kroft and Notowidigdo (2009) and Jochmans (2023) to detect whether Indigenous students are being assigned to specific classrooms with other Indigenous students in violation of random assignment. Neither test can reject the null hypothesis of no systematic sorting. Second, we also implement the "left hand side" test of Pei, Pischke and Schwandt (2019) by regressing $\overline{\%Indigenous_{iscw_1}^{-i}}$ on all pre-assignment characteristics and testing the joint significance of all regressors. This test also fails to reject the null of balanced sample with p-value equal to 0.586. Taken together, these results strongly indicate compliance with the mandate of random assignment of students to classrooms within schools in our estimation sample, and thus support the identifying assumption.[25]

[23] These 19 schools, out of 333 in the full TEPS sample, serve only 1,083 students out of the 20,055 in the data, or a little over 5%. For comparison, we also exclude 152 schools with 7,745 student observations, representing almost 39% of the sample, because these schools do not have any Indigenous students and thus do not contribute identifying variation to our key regressor.

[24] Sample sizes differ across outcomes due to missing values. Conclusions are unchanged if we force the sample to be stable across outcomes.

[25] Another potential concern could be selective attrition (i.e., if exposure to minority peers would affect the likelihood of being observed in the second wave of TEPS). The national mandate to keep classroom grouping throughout junior high school somewhat limits the scope of this concern. Nevertheless, we verify that in our sample 98% of students remain in the same school and, conditional on remaining in the same school, 96% of students also remain in the same classroom two years after the beginning of junior high school. This means that in our sample at most 6% of students change classrooms between survey waves. Indigenous students are no more likely than majority students to change either schools or classrooms. Furthermore, the share of Indigenous peers is uncorrelated with the likelihood that students remain in their assigned classroom given that they remain in the same school. We do find a positive association between the share of Indigenous peers and the likelihood that a student changes schools, yet this estimate is of trivial magnitude (less than one percentage point for a ten-percentage-point increase in Indigenous peers). We view this small shift as a quantitatively unimportant part of the causal effect of Indigenous peers, and note that our effects should be interpreted as intent-to-treat estimates (though most likely empirically indistinguishable from the corresponding treatment effects on the treated we would have obtained without attrition).

# 7 Results

## 7.1 Effects of classroom exposure to Indigenous students on test scores

We first estimate the effects of exposure to Indigenous students on *test scores*. This is akin to a standard linear-in-means models augmented with random assignment of minorities to classrooms. The results are presented in Table 2 and we want to highlight three main findings from this table.

**Table 2:** Main results: The Effects of Exposure to Indigenous Students in the Classroom on Test Scores

| **Outcome [std]:** | **Student test scores** | | | |
|---|---|---|---|---|
| Share of Indigenous peers | -0.299 | -0.392*** | -0.398*** | -0.402*** |
| | (0.215) | (0.140) | (0.139) | (0.145) |
| Indigenous peers × Indigenous | | | | 0.020 |
| | | | | (0.090) |
| Own test scores | | ✓ | ✓ | ✓ |
| Pre-assignment controls | | | ✓ | ✓ |
| $R^2$ | 0.15 | 0.62 | 0.63 | 0.63 |
| Schools | 155 | 155 | 155 | 155 |
| Classes | 588 | 588 | 588 | 588 |
| Students | 8,517 | 8,517 | 8,517 | 8,517 |

Note: This table reports estimates from regressing standardized student test scores in wave 2 on the classroom share of Indigenous peers in wave 1 (and its interaction with an Indigenous student dummy in the rightmost column). All regressions include wave 1 school fixed effects. Own test scores refer to students' test scores in wave 1. Pre-assignment controls include all variables tested for balancing in Table 1. All controls are interacted with an Indigenous student dummy. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

First, conditional on own test scores measured at the baseline, adding pre-assignment controls does not substantively affect the point estimate of interest (column 2 versus column 3). This is expected given the balance documented in Table 1. There is a modest difference in effect sizes between column (1), where we do not control for own test scores, and the subsequent columns where we do. This is likely due to a very strong correlation between test scores in waves 1 and 2 and almost no correlation between share of minorities and wave 1 test scores (as documented in Table 1). Since the omitted variables bias formula multiples these two correlations, even a small imbalance in baseline test scores (e.g., due to sampling variation) would be magnified by a strong correlation between wave 1 and wave 2 test scores. Overall, we view the point estimates of -0.299 (with p-value of 0.163) and -0.392 (with p-value $< 0.001$) as qualitatively similar. Importantly, including baseline test scores meaningfully shrinks the standard errors on our coefficient of interest.

Second, we find statistically significant negative effects from exposure to Indigenous students on test scores (Equation (20)). A 10 percentage point increase in the share of Indigenous students in a classroom reduces test scores of other students by 4.0% of a standard deviation.[26] In our data 60% of classrooms have no Indigenous children, 27% have just one Indigenous child while the remaining 13% have more than one Indigenous child. Given the maximum mandated class size of 35, adding one Indigenous child would increase the share of Indigenous peers by at least 2.9%, which translates into a negative effect of 1.2% of a standard deviation. Although appearing small, this average effect applies to all the remaining students in the classroom (at most 34), and thus, its aggregate effects should not be understated.[27,28]

Third, the interaction term in the last column of the table shows no evidence that increased share of Indigenous peers in the classroom helps Indigenous students themselves. The coefficient is positive but only about one-twentieth of the negative effect for the majority students and not statistically different from zero. This is consistent with Propositions 1 and 2 in the model which imply that test scores should decline for both the majority and minority students if students, parents, and teachers adjust their behaviors. We note that although one could expect positive concordance effects from one's own group of peers (like, for example, Hoxby (2000), who finds stronger intra- compared to inter-race peer effects in achievement) this is not predicted by our model and indeed we do not find empirical support for this hypothesis.[29]

To give economic context to our estimates, it is worth highlighting that prior results in the literature have been both limited and mixed, and with the exception of Antecol, Eren and Ozbeklik (2016) who do not find any statistically significant effects of exposure to minority peers, no prior study was able to exploit lottery-style random assignment of minorities to either schools or classrooms. Similarly, Hoxby and Weingarth (2005) find very limited effects of minority peers once they account for peer achievement—a point which we come back to below. Considering other papers, Angrist and Lang (2004) and Figlio and Özek (2019) do not find negative effects on cognitive ability of native/majority students exposed to racial minorities or immigrants, respectively. Setren (2022) re-examines the METCO program first evaluated by

[26] We define an Indigenous student if either of their parents is Indigenous. Defining students as Indigenous only if both parents are Indigenous reduces the point estimate to -2.8% of a standard deviation but it remains statistically significant at conventional levels.

[27] Appendix Figure C.4 shows a somewhat u-shaped pattern of effects of the number of Indigenous students in a classroom. Negative estimates are similar for exactly one and three or more Indigenous students but they are larger when there are two Indigenous students in the classroom. At the same time, we acknowledge the lack of precision in the "three or more" estimate which is driven by the fact that we only observe 47 classrooms with that many Indigenous students. This limited variation likewise precludes testing many peer effects models alternative to the linear-in-means model (Hoxby and Weingarth, 2005).

[28] We also considered other dimensions of heterogeneity including gender, baseline test scores, and geography. Appendix Table C.6 presents these results. We find somewhat larger effects for boys and significantly larger effects for higher ability students and those residing in Western Taiwan. The gender findings is consistent with differential sensitivity of boys and girls to family (Autor et al., 2019) and school (Autor et al., 2016) inputs. The latter two results are consistent with greater distance between $b^M$ and $b^m$ in these groups.

[29] It could be that in other contexts concordance induces more student effort through e.g., group studying or help from the peers. For example, in college setting, Chevalier, Isphording and Lisauskaite (2020) show that higher share of non-native speakers decreases interactions with native speakers and increases interactions with other non-native speakers in the classroom. Another explanation could be that concordance effects require certain critical mass of similar peers to materialize. In our setting very few Indigenous students have one or more Indigenous peers.

Angrist and Lang (2004) and likewise finds no negative peer effects on academic performance, classroom behavior, and attendance of suburban students. In contrast to our findings, Figlio et al. (2024) find that increasing immigrants in schools from the $10^{th}$ to the $90^{th}$ percentile increases native student's test scores in mathematics and reading by 2.8 and 1.7 percent of a standard deviation, respectively.[30] Morales (2022) also finds positive peer effects from exposure to refugees, for mathematics but not for reading, although she acknowledges that these effects are plausibly driven by compensatory funding and in-kind support available to schools serving refugee students; something that is not available in our setting. Yet, in line with our findings, Hoxby (2000) documents that a 10 percentage point increase in the share of Black students in a classroom, decreases third grade reading test scores by 0.25, 0.10, and 0.06 points for Black, Hispanics, and Anglo students, respectively.[31] Our Indigenous effect sizes are comparable to Hoxby's estimates for Anglo students, and are much smaller than her estimates for Black and Hispanic students. Similarly, Diette and Oyelere Uwaifo (2014), shows that a 10 percentage point increase in students with limited English proficiency lowers mathematics and reading scores of native students by about 0.7 percent of a standard deviation. Thus our results are closest to those reported by Hoxby (2000) and Diette and Oyelere Uwaifo (2014), and they are in contrast with most prior literature; much of which suffers from lack of experimental variation.

We can further compare our estimates to other compulsory school peer effect studies which considered student characteristics such as exposure to domestic violence (Carrell and Hoekstra, 2010), special needs (Balestra, Eugster and Liebert, 2022), disability (Huang, Lu and Zhu, 2021), or gender (Lavy and Schlosser, 2011). These attributes are plausibly observable to others in the educational environment thus satisfying our requirements for the vector $b^i$ as well as Propositions 1 and 2 in the theoretical model. Many of those studies express their magnitudes as an increase of one child in a class of 20. Following this scaling, our estimates would translate to a decline in test scores of 0.020 SD per additional Indigenous student. Carrell and Hoekstra (2010) find effect size of 0.025 SD, Balestra, Eugster and Liebert (2022) likewise find effect size of 0.025 SD, Huang, Lu and Zhu (2021) find effect size of at least 0.051 SD, and finally Lavy and Schlosser (2011) find effect sizes of 0.010-0.013 SD. Carrell and Hoekstra (2010) is particularly relevant for us since the average share of students affected by domestic violence in their sample is 4.6% which is very close to the share of Indigenous students in Taiwan. Our effect sizes are thus very comparable to those generated by exposure to children experiencing domestic violence or

[30] Some of the positive results in this paper are actually consistent with our theory. Figlio et al. (2024) find positive effects for Black students (but not White students), yet the average math score of an immigrant student (their treatment) is -0.093 SD while the average math score of a Black student (their outcome) is -0.495 SD. Likewise, they find positive effects for students in poverty (but actually some negative effects for more affluent students) but average math score of these students is -0.303 SD. In both cases immigrant students appear to outperform their peers i.e., $b^m > b^M$. Rather, in our application, $b^m < b^M$. We view this contrast as a major contribution of our theoretical model which clarifies why some researchers find positive minority peer effects while others find negative ones. It further highlights the need of a deep institutional context knowledge when studying peer effects.

[31] Standard deviation of this test score varies between 2.1 and 2.6 depending on the exact year (Table 2 in Hoxby (2000)), implying re-scaled effect sizes between 2.3 and 11.9 percent of a standard deviation depending on the race and which standard deviation we use.

requiring special needs. On the other hand, they are much larger than the gendered peer effects.[32]

## 7.2 Alternative explanation: Characteristics correlated with Indigeneity

An element we have not yet included in our empirical analysis is the fact that Indigeneity itself is correlated with socioeconomic and educational characteristics. In other words, even in a setting with institutionalized random assignment, majority students are not only assigned an Indigenous person but also a person with lower academic achievement and socioeconomic students (Appendix Table C.3). To the extent that these other characteristics have causal effects on the majority students' achievement, our reduced-form effect captures not only exposure to an Indigenous student but also to their correlated observable and unobservable characteristics, such as lower academic achievement. This is exactly the point that (Hoxby and Weingarth, 2005) make. Given the literature on ability peer effects (e.g., Lavy, Silva and Weinhardt (2012), and in the Taiwanese context de Gendre and Salamanca (2020)), the importance of language skills in schooling (e.g., Boucher et al., 2020), and the effects of disruptiveness in generating peer effects (e.g., Carrell, Hoekstra and Kuka, 2018), we thus need to ensure that the negative effects documented above are indeed due to the fact that these students are Indigenous rather than due to any other (observable) correlated peer characteristics studied in the extant literature. Note that, in this exercise, we are decomposing the effect of Indigenous students based on other baseline characteristics. Since in our setting students are as good as randomly assigned, this exercise can be viewed as a form of causal decomposition (i.e., identifying the part of the causal effect of exposure to Indigenous students that comes through causal effects of other characteristics correlated with Indigeneity). Note, however, that the effect of exposure to Indigenous peers *controlling for other peer characteristics* is actually a different estimand from the one we are most interested in this paper. This estimand is conceptually interesting in that it tries to isolate the part of the effect that is idiosyncratic to the Indigeneity of peers (rather than their socioeconomic circumstances or their ability), but is less policy relevant since it is difficult to think of e.g., reassigning Indigenous students *without their other characteristics* to different classrooms.

To answer this question, we use the decomposition method proposed in Gelbach (2016), adjusted to utilize only within-school variation, which we need since our randomization is performed at the classroom-level within schools. Given the findings in Table 2, we simplify our empirical model by removing the interaction of the share of Indigenous students with the Indigenous student dummy. This means that we are decomposing the average effect of exposure to Indigenous

[32] Due to much smaller sample sizes we are unable to obtain precise peer effects estimates on longer-run outcomes using subsequent waves of the TEPS. Their sign and magnitude do suggest, however, that Indigenous peers decrease medium-run test scores, university enrollment and attainment, and labor market outcomes. Using the estimates in Appendix Table C.2 to map wave 2 test scores to long-run outcomes, we estimate that our peer effect estimates on test scores would imply that one additional Indigenous student in a 35-child classroom translates into average reductions in university attendance and earnings of 0.5% ($\frac{0.166}{0.38} \times 0.012 \times 100$ ) and 0.3% ($\frac{0.112}{0.46} \times 0.012 \times 100$) for the remaining children in the classroom, respectively. Although small at individual level, these losses could meaningfully add up given that they apply to most students.

students on both Indigenous and majority student test scores.

**Table 3:** Decomposing the Effect of Exposure to Indigenous Peers by Correlated Observable Characteristics

| **Outcome [std]:** | **Student test scores** | **Share explained** |
|---|---|---|
| Share of Indigenous peers | -0.393*** | 100% |
| | (0.138) | |
| Effect explained by other peer characteristics | -0.114 | 29% |
| | (0.074) | |
| → by peer test scores | -0.058** | 15% |
| | (0.026) | |
| → by peer socioeconomic status | -0.059 | 15% |
| | (0.047) | |
| → by peer language skills | -0.014 | 4% |
| | (0.054) | |
| → by peer parental investments | 0.005 | -1% |
| | (0.015) | |
| → by peer disruptiveness | 0.013 | -3% |
| | (0.024) | |
| Students | 8,517 | |

Note: This table reports the total effect of the classroom share of Indigenous peers in wave 1 on students test scores in wave 2 and its mediated effect by other pre-assignment peer characteristics in wave 1. Regression includes wave 1 school fixed effects and control for students' own test scores in wave 1 and pre-assignment characteristics, and all controls are interacted with an Indigenous student dummy. It does not include the interaction between share of Indigenous students and being Indigenous and thus is akin to specification from column 3 of Table 2. These estimates are based on a Gelbach (2016) decomposition and produced using the `b1x2` Stata package on a within-school transformation of the data. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table 3 presents these results and shows that disruptiveness of Indigenous students, their language skills and investments of Indigenous parents do not contribute meaningfully to explaining the negative effect on test scores.[33] This is interesting given the findings that Indigenous students are more disruptive along many dimensions, they have lower skills in Mandarin, and the investments of their parents are likewise lower (see Appendix Table C.3, though we note that in our data the markers of disruptive behaviors are weakly correlated with test scores). On the other hand, lower test scores and socioeconomic status of Indigenous students explain slightly less than one-third of the uncovered negative effect. Together, due to these correlated characteristics the negative estimate declines by 0.114 (p-value of 0.127). We conclude that although characteristics correlated with Indigeneity need to be accounted for, they appear to matter somewhat less than the changes in behavior documented in Section 7.3. This is in contrast to findings in (Hoxby and Weingarth, 2005) where peers' ability dominated other correlated demographic characteristics rendering them mostly statistically insignificant.

[33] The coefficient of -0.393 in Table 3 differs slightly from the one in column 3 of Table 2 because it is estimated using Gelbach (2016) command (b1x2 in Stata) rather than the standard OLS.

Given the results on correlated observables, one can also ask if higher ability Indigenous students likewise generate negative peer effects. To the extent that part of the effect is due to discriminatory perceptions even in such a case we would expect to find negative coefficients. Appendix Table C.7 presents these results where we divide Indigenous students into those scoring above versus below median on wave 1 test, noting that only 20% of Indigenous student score above the median (which, again, highlights their disadvantage). We find negative effects on test scores from both low- and high-ability Indigenous peers of similar magnitude, although the latter effect is less precisely estimated since we have less identifying variation for high-ability Indigenous peers. This result is consistent with the effect of Indigenous peers not being primarily driven by differences in student ability. We also find negative responses of parents to both low- and high-ability Indigenous peers, a point that we come back to below. Interestingly, the negative effect on teacher engagement appear to be driven by low-ability Indigenous peers.

### 7.3 Effects of classroom exposure to Indigenous students on student effort, parental investments, and teacher engagement

We now turn to one of the key innovations of our paper: understanding the responses of students, parents, and teachers. Recall that Propositions 1 and 2 in our theoretical framework predict that the higher is the fraction of minority students in a classroom, the lower is the effort of students, their parents, and teachers. Although effort is hard to measure, our data contain several good proxies (i.e., predicting student achievement) for the effort/investments of the three groups in question (see Section 4 for their description). First, Appendix Table C.8 (Panel A) shows that test scores in wave 2 are strongly positively correlated with all three indices of student, parent and teacher effort in wave 2, even after accounting for school fixed effects and other pre-assignment characteristics. Furthermore, for a subset of students that we can follow past wave 2, we document robust associations between wave 2 inputs and subsequent test scores (Panels B and C). These results suggest that our indices capture productive skills. Given our theory, we expect negative effects of exposure to minority peers on all three outcomes and we verify this by re-estimating Equation (21) while replacing the dependent variable with student, parental and teacher indices.

Table 4 presents these results. Consistent with our predictions, we find that higher share of Indigenous students in the classroom lowers student's study hours, lowers parental investments, and lowers teacher engagement. A 10 percentage point increase in the share of Indigenous students in a classroom is expected to decrease study hours of the majority students by 7.5% of a SD, decrease investments of majority parents by 7.2% of a SD, and decrease teacher engagement by 24.7% of a SD.[34] To put these effects in perspective, note that the rural-urban gap in these

[34] In our data we do not observe whether Dao Shi or subject matter teachers are Indigenous and thus we cannot credibly examine if negative teacher responses would be smaller or non-existent if Indigenous students interacted with Indigenous, rather than Han Chinese, teachers. At the same time, Indigenous teachers constitute a tiny fraction of all teachers in Taiwan.

effort measures is 23% of a SD for study hours and parental investments, and 30% of a SD for teacher engagement. Therefore, the effect of one additional Indigenous student in a class of 35 (the maximum class size in Taiwan) amounts to a little under a tenth of the rural-urban gap for study hours and parental investment, and about a quarter of the gap for teacher engagement. Our theory further predicts that the minority students and parents will also negatively adjust their effort but for the two inputs considered here, unlike for test scores, we actually find statistically significant interaction effects. In particular, the negative study hours effects are reduced by two-thirds for Indigenous students (rendering it still negative but statistically insignificant), while the negative parental investment effects are actually exacerbated by 40%. Importantly, our theory has nothing to say about the (relative) magnitude of these changes across minority and majority groups but rather focuses on the direction which still holds in our empirical results. Thus, we view the findings as consistent with our model positing that *all* students will be affected by higher share of minorities assuming that observable characteristics of minorities (actual or perceived) are worse compared to those of the majority children.

**Table 4:** Mechanisms: The Effects of Exposure to Indigenous Students in the Classroom on Student, Parents and Teacher Inputs

| **Outcomes [std]:** | **Student behaviors** | **Parental investments** | **Teacher engagement** |
|---|---|---|---|
| Share of Indigenous peers | -0.751*** | -0.715*** | -2.474*** |
| | (0.282) | (0.224) | (0.660) |
| Indigenous peers × Indigenous | 0.517*** | -0.281* | 0.351 |
| | (0.187) | (0.165) | (0.266) |
| $R^2$ | 0.19 | 0.23 | 0.28 |
| Schools | 155 | 155 | 155 |
| Classes | 588 | 588 | 578 |
| Students | 8,461 | 8,416 | 7,935 |

Note: This table reports estimates from regressing standardized aggregate indices of student-related, parent-related, and teacher-related mechanisms in wave 2 on the share of Indigenous peers in the classroom in wave 1. All regressions include wave 1 school fixed effects and control for students' own test scores in wave 1 and pre-assignment characteristics, and all controls are interacted with an Indigenous student dummy. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Above and beyond our theoretical model, it is worth thinking about *why* students, parents, and teachers could lower their investments in response to exposure to a higher share of minority students. Specifically, while our model predicts effects in line with reinforcing investments of parents and teachers and our empirical analyses confirm this predictions, one could still wonder why we do not find evidence of compensatory investments instead—that is, investment behavior that compensates the majority students for the "perceived negative" exposure to minority peers.

We propose two channels that are consistent with our model but not with the compensatory behavior. First, it could be that majority parents perceive these classrooms as weaker and thus decide to spend their resources elsewhere (perhaps on their other children which we do not observe in the data). Examples of such behavior appear in studies on sibling spillovers in education by Qureshi (2018), Nicoletti and Rabe (2019), and Karbownik and Özek (forthcoming). Second, parents could be receiving an inflated signal from the teachers about the relative performance of their child, leading them to believe that they do not need additional time and money investments. When it comes to teachers, we have already documented in Section 5 that they have negative perceptions about the aptitude of Indigenous students, even conditional on their baseline test scores. This fact is likely correlated with teacher motivation and engagement in classrooms with higher shares of Indigenous students as well as with their relative perception about majority students relative standing (which might be unduly inflated). A related explanation could be that more heterogeneous classrooms become harder to manage, either forcing the teacher to spend more time and effort managing the classroom and less time covering content or due to weariness from the additional effort. Consistent with this idea, Karbownik et al. (2024) document that higher share of lower ability and foreign background students leads to more sick leave and mental health problems among Swedish teachers. Furthermore, Lavy and Schlosser (2011) show that increase share of girls in the classroom improves student-teacher relationships and lowers teachers' fatigue, which also fits the prediction since girls generally have better observables in the classroom than boys in this setting. In out data, we can show that a non-trivial part of the reduction in teachers' input index is driven by the fact that Dao Shis tend to view classrooms with Indigenous students as harder to manage.

### 7.4 Can student, parent and teacher endogenous responses account for the effects on test scores?

Our equilibrium result in Equation (19) implies that test scores are affected because student, parental, and teacher responses enter the human capital production function. In other words, it has to be the case that these changes in behaviors have explanatory power for our test score result. To address this, we again use the method of Gelbach (2016) in a *descriptive* analysis to understand to what extent the changes documented in this section could explain the negative results presented in Table 2. This analysis is limited by the fact that we cannot rule out unobserved confounders jointly affecting test scores and the effort of parents, student, and teachers. Such confounders would bias our mediated effect estimates upwards. In that sense, one could see our estimates as an upper bound of the explanatory power of these variables. For this analysis we again simplify our estimates by removing the interaction of the share of Indigenous students with the Indigenous student dummy.

Table 5 presents the results.[35] We find negative mediation of our three input categories, which

[35] For ease of interpretation we report the decomposition based on specification from column 3 of Table 2, i.e., we do not include the interaction between share of Indigenous peers and being Indigenous. Results based on this specification for outcomes in Table 4 can be found in column 1 of Table C.10. Because there are relatively few Indigenous students in our sample, these estimates are similar to level coefficients reported in Table 4. Since student,

means that part of the negative peer effect can be explained by declines in the inputs of students, parents and teachers. Our results suggest that almost 40% of the negative test score effect documented in Table 2 could be due to changes in the behavior of students, parents, and teachers—factors that have to date been for the most part ignored in the extant literature on peer effects in education. Although descriptive, these results are, again, consistent with the key role of student, parent and teacher effort in shaping student test scores in our model.

**Table 5:** Mediation Analysis

| **Outcome [std]:** | **Student test scores** | **Share mediated** |
|---|---|---|
| Share of Indigenous peers | -0.389*** | 100% |
| | (0.136) | |
| Mediated effect | -0.151*** | 39% |
| | (0.047) | |
| → by student-related mediators | -0.063** | 16% |
| | (0.029) | |
| → by parent-related mediators | -0.029** | 7% |
| | (0.013) | |
| → by teacher-related mediators | -0.059* | 15% |
| | (0.033) | |
| Students | 8,517 | |

Note: This table reports the total effect of the classroom share of Indigenous peers in wave 1 on students test scores in wave 2 and its mediated effect by student-related, parent-related and teacher-related inputs in wave 2. Regression includes wave 1 school fixed effects and control for students' own test scores in wave 1 and pre-assignment characteristics, and all controls are interacted with an Indigenous student dummy. It does not include the interaction between share of Indigenous students and being Indigenous and thus is akin to specification from column 3 of Table 2. These estimates are based on a Gelbach (2016) decomposition and produced using the `b1x2` Stata package on a within-school transformation of the data. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level

### 7.5 Combining correlated characteristics and endogenous responses

Our final analysis combines the decomposition and mediation results presented in Tables 3 and 5, and asks if we can fully explain away the negative Indigenous peer effect documented in Table 2 by accounting for correlated student characteristics as well as endogenous responses of students,

parental, and teacher inputs are correlated, and the Gelbach (2016) decomposition accounts for this interdependence, the coefficients change somewhat compared to those from Table 2. Furthermore, the method allows us to include the individual input measures rather than forcing us to add the parent and teacher indices themselves i.e., the group-$g$ decomposition (proposed by Gelbach (2016, p. 522)) takes care of aggregating the separate measures for the purposes of the analysis. This is advantageous, since the indices forcibly lose some information when aggregating input measures. Adding all the scales separately rather than the three aggregate indices makes this mediation result robust to all possible constructions of the input indices. Finally, unlike other decomposition methods, this one is not sensitive to ordering of the explanatory variables and takes into account correlations between them.

parents and teachers. As in Section 7.4, this is a descriptive exercise and Appendix Table C.9 presents these results. Recall that the results presented in Table 5 in Section 7.4 suggested an explanatory power of 39% while results presented in Table 3 in Section 7.2 suggested explanatory power of 29%. Therefore, ex ante, we expect that we should be able to explain at most 68% of the reduced form effect from Table 2. Including only correlated peer characteristics (column 2) explains 28% of the estimated negative effect, yet the remaining Indigenous peer effect is still statistically significant at 10% level. Conversely, including only student, parent, and teacher endogenous responses (column 3) explains 39% of the estimated negative effect, yet again the coefficient remains statistically significant at 10% level. Finally, conditioning on both correlated peer characteristics and endogenous inputs (column 4) explains away 70% of the negative coefficient which is no longer statistically significant at conventional levels—matching almost exactly our prediction above. One reason for this is that due to random assignment to classrooms channels explored in Sections 7.4 and 7.2 are likely highly independent.

Even though this is a descriptive exercise, we conclude that much of the negative peer effect documented in Section 7.1 is plausibly due to endogenous responses of parents and teachers (somewhat more important) as well as characteristics correlated with peers' Indigenous status (somewhat less important). This insight is valuable for two reasons. First, much of prior literature does not account for correlated observable characteristics when examining minority peer effects which could lead to distorted conclusions. Second, the fact that over one-third of the effect could be attributed to endogenous responses of students, parents, and teachers leaves hope that resource or information interventions could correct these behaviors such that any negative spillovers occurring due to classroom integration efforts are minimized. The remaining, unexplained, 32% of the effect could be due to the fact that even with our rich observables we cannot fully account for all the correlates of Indigeneity or behavioral responses (e.g., we do not observe friendship networks and endogenous responses of friends outside of classroom). Alternatively, there could be a direct interaction component in the reduced-form effect that we are unable to measure.

# 8 Robustness checks

Our main findings, presented in Tables 2 and 4, are robust to a number of alternative specifications.[36] We start our analysis by exploring the stability of our preferred estimates over alternative sample choices. Appendix Table C.10 presents these results. Column 1 replicates our results based on specification from column 3 of Table 2.[37] Column 2 drops schools where Indigenous

[36] For ease of interpretation we report robustness checks for specification based on column 3 of Table 2, i.e., we do not include the interaction between share of Indigenous peers and being Indigenous. Our results are substantively unchanged if we instead focus on specification from column 4 of Table 2 and on Table 4.

[37] This table further documents the average effects of exposure to Indigenous students on students', parents' and teachers' inputs rather than separating them into effects on the Han Chinese majority and the Indigenous minority as in Table 4. Note that these estimates are very similar to the effects for the Han Chinese majority reported in Table 4 despite the fact that for two out of three inputs we also find statistically significant interaction effects. This is because

students are in the majority and therefore where higher classroom shares of these students do not actually represent increased exposure to minorities. In column 3 we exclude both schools where Indigenous or Hakka students form a majority. Finally, in column 4 we drop all private schools, which we worry might be less likely to follow the governmental rules on random assignment to classrooms, or be more likely to cater to a more select set of students and parents. Irrespective of the sample we choose, we find very consistent results.

In another exercise, instead of dropping specific types of schools, we also consider excluding specific Han Chinese students, who are not from the Hoklo majority. Appendix Table C.11 presents these results. Column 1 again replicates our baseline results, column 2 excludes Hakka students, column 3 excludes Waishengren students, and finally column 4 excludes both sets of students. In this case, again, our conclusions remain unchanged.[38] Based on these exercises we conclude that our main results are robust to reasonable alternative specifications of the estimation samples.

As documented in Table C.3 our empirical sample is modestly negatively selected; e.g., we are more likely to see lower-performing students with lower-educated and financially stressed parents. Therefore, our results might not generalize to all Taiwanese students in these cohorts—an important external validity concern. We address this issue by re-weighting our estimates to match the characteristics of the full TEPS sample. Appendix Table C.12 presents these results which are more negative when it comes to student test scores and effort, and less negative when it comes to parental investments and teacher effort. This suggests that our main results, if anything, might underestimate the average treatment effect of having Indigenous peers in the broader population represented in the TEPS.

Given the relatively small share of Indigenous students in population, it is perhaps more policy relevant to ask if having exposure to any Indigenous peer–at the extensive margin–would also generate negative effects. When we use a dummy variable for any Indigenous peer in the classroom we find a negative and statistically significant point estimate of 4.8% of a standard deviation.[39]

in the full sample we observe relatively few Indigenous students (only 7%) that contribute to the identification of the interaction effect, and thus the average effect is similar to the effect for Han Chinese majority.

[38] Interestingly, in a sample where we exclude Waishengren students (columns 3 and 4), the coefficient declines by up to one-third, thereby suggesting that negative peer effects could be particularly severe for this group of students.

[39] An additional concern here is selection into identification (Miller, Shenhav and Grosz, 2022). Even if students are randomly allocated to classrooms, in schools with one or more Indigenous students in each classroom there would be no within-school variation in the treatment. Therefore, these schools would not contribute to the identification of the coefficient on the presence of Indigenous students in the classroom (whereas they do contribute to identifying our main estimates of the effect of the share of Indigenous peers via their within-school variation in the intensive margin). If these schools are systematically different from those that do contribute to identification, and in the presence of heterogeneous treatment effects, this can bias our estimates away from Average Treatment Effect of interest. Out of the 155 schools in the sample with at least one Indigenous student, 18 have two or more Indigenous students in every sampled classroom. These 18 schools serve areas with a high number of Indigenous people and are consequently more disadvantaged and lower-scoring. Therefore, the extensive margin results should be interpreted keeping in mind that these particular schools are not contributing to the identifying variation.

We also check that our inference is not affected by departures from random assignment to classrooms, or by quantitatively important spillovers across classrooms. To assess this, we perform a series of placebo estimates where we randomly re-assign students to placebo classrooms while keeping school structures intact, and then recalculate "placebo" Indigenous exposure effects. We perform this random re-assignment 10,000 times and obtain normal-looking distributions centered around zero (Appendix Figure C.3). These findings are reassuring since, in the presence of unobserved confounders correlated with Indigeneity or quantitatively-important spillovers across classrooms, we would expect skewed distributions for the placebo effects. An additional benefit from this exercise is that by comparing actual and placebo effects we can construct exact tests, with a distribution that is independent of sample size or the distribution of the error term (Young, 2019). Indeed, the aforementioned comparisons of placebo and actual effects reaffirms the statistically significance of the negative effect of exposure to Indigenous students in a classroom (with an empirical p-values of at most 0.003).

Our final robustness exercise involves two sets of placebo tests. Recall that our theoretical model hinges upon the fact that compared to the majority students their Indigenous peers have less favorable observable characteristics ($b^m < b^M$). This means that we should not observe negative peer effects for groups where $b^m \approx b^M$, even if these children come from minority population. Our first placebo test re-estimates our results from Tables 2 and 4 for Hakka students. As mentioned in Section 3.2, Hakka people are likewise an officially recognized by the Taiwanese government minority group, however, as documented in Appendix Table C.4 these students have similar observable characteristics to the Hoklo majority. Furthermore, Panel A of Table C.5 shows that teachers do not appear to hold prejudiced views against Hakka students. Therefore, we do not expect Hakka peers to affect student outcomes. Panel A of Appendix Table C.13 shows that Hakka peers do not appear to generate any economically meaningful peer effects, either for the majority students (the level coefficient) or for the other Hakka students (the interaction term). We further note that the negative statistically insignificant at conventional levels coefficient in column 1 is less than one-tenth of the coefficient in column 4 of Table 2 reported for Indigenous peers. Likewise, we do not find any endogenous responses of the majority students, their parents, or teachers in classrooms with higher share of Hakka students. Two out of three of the coefficients have opposite sign than those reported in Table 4 and they are all much smaller.

For our second placebo exercise, we construct a group of "synthetic" Indigenous students: majority students that are nearly observationally equivalent to Indigenous students. Specifically, for each Indigenous student in each school, using a propensity score function, we define a synthetic Indigenous student that is a non-Indigenous student most similar to them in terms of baseline test scores and all other pre-assignment characteristics. Appendix Figure C.5 shows that there is sufficient common support in propensity scores. We then use these students to construct synthetic Indigenous peers in our placebo regressions. The rationale behind this exercise is to test biased subjective expectations ascribed via Indigeneity as the plausible mechanism behind our results. While synthetic Indigenous students are as disadvantaged in terms of observables as actual Indigenous students, others would not have formed expectations about them based on their identity (consistent with Panel B of Appendix Table C.5)). Therefore, the effect of synthetic Indigenous peers carries with it the treatment of observable disadvantage but without the prejudice—which we argue might be a key mechanisms behind our findings (especially if it interacts with and exacerbates the disadvantage).

Panel B of Appendix Table C.13 shows that we indeed find no effects of synthetic Indigenous peers on student test scores. On the other hand, we do find some positive parental responses when we consider this treatment for both the observably better off students (the level coefficient) and the synthetic students' parents as well (the interaction term). This suggests that absent prejudice parents might actually attempt to compensate for exposure to students with adverse observable characteristics. When it comes to teacher engagement the level coefficient is about one-quarter of the corresponding estimate in Table 4 and it is not statistically significant at conventional level (the positive and marginally statistically significant interaction term suggest zero effects on teacher engagement for disadvantaged students in classrooms with more disadvantaged peers perhaps because of an increased classroom homogeneity). Overall, we view these results as strongly suggesting that Indigenous students generate the negative peer effects due to not only their lower observable characteristics ($b^m < b^M$) but also because of prejudice.

# 9 Conclusions

This paper shows that exposure to Indigenous peers can have negative effects on the educational achievement of students. A 10 percentage points increase in classroom exposure to Indigenous peers lowers test scores of the majority students by 4.0% of a standard deviation. These negative externalities are partially driven by lower observable characteristics of this minority group as well as prejudice against them. Adverse characteristics correlated with Indigeneity account for little less than a third of this negative effect. Exposure to Indigenous peers also decreases student, parent, and teacher effort. Over a third of the negative effect on test scores can be descriptively explained by these endogenous responses.

Taken together, our results have important implications for both modelling peer effects in schools as well as classroom integration efforts. First, when estimating peer effects, it appears important to consider them in the context of multi-agent production function that involves not only direct interactions between students but also endogenous responses of the students, their parents, and teachers in these classrooms. Ignoring those actors could paint a distorted picture on what exactly is driving the reduced form estimates. Second, ignoring contextual interactions —that is, exogenous peer characteristics correlated with the minority status (Manski, 2000)—could lead to biased estimates. Finally, when designing classroom integration policies, it appears important to consider the endogenous actions of students, parents, and teachers; ignoring these responses could lead to misguided policy recommendations.

Our findings are further relevant to a broader policy context, outside of schooling, where peer effects could arise due to endogenous responses of multiple agents. For example, it is plausible that some of the workplace peer effects are generated by the decisions of managers (Cornelissen, Dustmann and Schönber, 2017, Lindquist, Sauermann and Zenou, 2022) or that part of the peer effects found in prisons is due to behaviors of guards or prisoners' visitors (Bayer, Hjalmarsson and Pozen, 2009, Piil Damm and Gorinas, 2020). Understanding *who* is driving the reduced-form effects estimated in the extant literature appears crucial for the design of effective and efficient policies.

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# Appendix A Theory

## A.1 Size of population $n$ is fixed

Assume that the population $n$ is fixed. The first-order condition (2) can be written as:

$$y^i = b^i + \phi \left[ qy^m + (1-q)\, y^M - \frac{y^i}{n} \right]$$

This implies that

$$y^M = \frac{b^M + \phi\, qy^m}{1 - \phi\,(1-q) + \frac{\phi}{n}}$$

$$y^m = \frac{b^m + \phi(1-q)y^M}{1 - \phi q + \frac{\phi}{n}}$$

By solving these equations, we obtain:

$$y^{M*} = \frac{\left(1 - \phi q + \frac{\phi}{n}\right) b^M + \phi q b^m}{\left(1 - \phi + \frac{\phi}{n}\right)\left(1 + \frac{\phi}{n}\right)}$$

$$y^{m*} = \frac{\left[1 - \phi\,(1-q) + \frac{\phi}{n}\right] b^m + \phi(1-q)b^M}{\left(1 - \phi + \frac{\phi}{n}\right)\left(1 + \frac{\phi}{n}\right)}$$

We have

$$\frac{\partial y^{M*}}{\partial q} = \frac{\phi\left(b^m - b^M\right)}{\left(1 - \phi + \frac{\phi}{n}\right)\left(1 + \frac{\phi}{n}\right)}$$

$$\frac{\partial y^{m*}}{\partial q} = \frac{\phi\left(b^m - b^M\right)}{\left(1 - \phi + \frac{\phi}{n}\right)\left(1 + \frac{\phi}{n}\right)}$$

Clearly, if $b^m < b^M$, then $\frac{\partial y^{M*}}{\partial q} < 0$ and $\frac{\partial y^{m*}}{\partial q} < 0$.

## A.2 Proof of Proposition 1

When $n$ is large, $\frac{y^i_s}{n} \to 0$, the first-order condition (2) can be written as:[40]

$$y^i_s = b^i + \phi\, \left[ qy^m_s + (1-q)\, y^M_s \right]. \tag{A.1}$$

[40] Assuming a fixed size of $n$ does not change the results; it just makes the analysis more complicated. See Section A.1 in this Appendix.

This implies that:

$$y_s^{M*} = \frac{b^M + \phi\, q y_s^m}{1-\phi(1-q)} \tag{A.2}$$

$$y_s^{m*} = \frac{b^m + \phi(1-q) y_s^{M*}}{1-\phi q} \tag{A.3}$$

By solving these equations, we obtain:

$$y_s^{M*} = \frac{(1-\phi q)\, b^M + \phi q b^m}{1-\phi} \tag{A.4}$$

$$y_s^{m*} = \frac{[1-\phi(1-q)]\, b^m + \phi(1-q) b^M}{1-\phi} \tag{A.5}$$

We have:

$$\frac{\partial y_s^{M*}}{\partial q} = \frac{\phi\left(b^m - b^M\right)}{1-\phi}, \tag{A.6}$$

$$\frac{\partial y_s^{m*}}{\partial q} = \frac{\phi\left(b^m - b^M\right)}{1-\phi}. \tag{A.7}$$

Thus, if $b^m < b^M$, then $\frac{\partial y^{M*}}{\partial q} < 0$ and $\frac{\partial y^{m*}}{\partial q} < 0$.

## A.3 Proof of Proposition 2

### A.3.1 Parents' effort choices

Let us solve first the parents' effort choice.

Consider the *majority* parent's utility function (12). The first-order condition is equal to:

$$\frac{\partial U_p^M}{\partial y_p^M} = \alpha_2 \rho \,(1-\phi)^{-\alpha_1} \left(\Delta^M(q)\right)^{\alpha_1} \left(y_p^M\right)^{\alpha_2 - 1} \left(y_t^i\right)^{\alpha_3} - 1 = 0$$

Solving this equation leads to:

$$y_p^{M*} = (\alpha_2 \rho)^{1/(1-\alpha_2)} (1-\phi)^{-\alpha_1/(1-\alpha_2)} \left(\Delta^M(q)\right)^{\alpha_1/(1-\alpha_2)} \left(y_t^i\right)^{\alpha_3/(1-\alpha_2)}$$

Thus

$$\left(y_p^{M*}\right)^{\alpha_2} = (\alpha_2 \rho)^{\alpha_2/(1-\alpha_2)} (1-\phi)^{-\alpha_1\alpha_2/(1-\alpha_2)} \left(\Delta^M(q)\right)^{\alpha_1\alpha_2/(1-\alpha_2)} \left(y_t^i\right)^{\alpha_3\alpha_2/(1-\alpha_2)}$$

Denote

$$Z_1 := (\alpha_2 \rho)^{\alpha_2/(1-\alpha_2)} (1-\phi)^{-\alpha_1 \alpha_2/(1-\alpha_2)}$$

We have

$$\left(y_p^M\right)^{\alpha_2} = Z_1 \left(\Delta^M(q)\right)^{\alpha_1 \alpha_2/(1-\alpha_2)} \left(y_t^i\right)^{\alpha_3 \alpha_2/(1-\alpha_2)}$$

and

$$y_p^{M*} = Z_1^{1/\alpha_2} \left(\Delta^M(q)\right)^{\alpha_1/(1-\alpha_2)} \left(y_t^i\right)^{\alpha_3/(1-\alpha_2)}$$

Consider now the *minority* parent's utility function (13). Proceeding as above, we easily obtain:

$$\left(y_p^m\right)^{\alpha_2} = Z_1 \left(\Delta^m(q)\right)^{\alpha_1 \alpha_2/(1-\alpha_2)} \left(y_t^i\right)^{\alpha_3 \alpha_2/(1-\alpha_2)}$$

and

$$y_p^{m*} = Z_1^{1/\alpha_2} \left(\Delta^m(q)\right)^{\alpha_1/(1-\alpha_2)} \left(y_t^i\right)^{\alpha_3/(1-\alpha_2)}$$

#### A.3.2 Teachers' effort choices

The first-order condition is:

$$\alpha_3 n^M \rho (1-\phi)^{-\alpha_1} \left(\Delta^M(q)\right)^{\alpha_1} \left(y_p^M\right)^{\alpha_2} \left(y_t^i\right)^{\alpha_3 - 1}$$
$$+ \alpha_3 n^m \rho (1-\phi)^{-\alpha_1} \left(\Delta^m(q)\right)^{\alpha_1} \left(y_p^m\right)^{\alpha_2} \left(y_t^i\right)^{\alpha_3 - 1} = 1$$

which is equivalent to:

$$\frac{(1-\phi)^{\alpha_1}}{n\alpha_3 \rho} \left(y_t^i\right)^{1-\alpha_3} = (1-q) \left(\Delta^M(q)\right)^{\alpha_1} \left(y_p^M\right)^{\alpha_2} + q \left(\Delta^m(q)\right)^{\alpha_1} \left(y_p^m\right)^{\alpha_2}.$$

Using the equations above, that is

$$\left(y_p^M\right)^{\alpha_2} = Z_1 \left(\Delta^M(q)\right)^{\alpha_1 \alpha_2/(1-\alpha_2)} \left(y_t^i\right)^{\alpha_3 \alpha_2/(1-\alpha_2)}$$

$$\left(y_p^m\right)^{\alpha_2} = Z_1 \left(\Delta^m(q)\right)^{\alpha_1 \alpha_2/(1-\alpha_2)} \left(y_t^i\right)^{\alpha_3 \alpha_2/(1-\alpha_2)}$$

we have

$$\begin{aligned} \frac{(1-\phi)^{\alpha_1}}{n\alpha_3 \rho} \left(y_t^i\right)^{1-\alpha_3} &= (1-q) \left(\Delta^M(q)\right)^{\alpha_1} Z_1 \left(\Delta^M(q)\right)^{\alpha_1 \alpha_2/(1-\alpha_2)} \left(y_t^i\right)^{\alpha_3 \alpha_2/(1-\alpha_2)} \\ &\quad + q \left(\Delta^m(q)\right)^{\alpha_1} Z_1 \left(\Delta^m(q)\right)^{\alpha_1 \alpha_2/(1-\alpha_2)} \left(y_t^i\right)^{\alpha_3 \alpha_2/(1-\alpha_2)} \end{aligned}$$

This is equivalent to:

$$\frac{(1-\phi)^{\alpha_1}}{n\alpha_3 \rho Z_1} \left(y_t^i\right)^{\alpha_1/(1-\alpha_2)} = (1-q) \Delta^M(q)^{\alpha_1/(1-\alpha_2)} + q \left(\Delta^m(q)\right)^{\alpha_1/(1-\alpha_2)}$$

By solving this equation, we obtain:

$$y_t^{i*}(q) = \frac{(n\alpha_3\rho Z_1)^{(1-\alpha_2)/\alpha_1}}{(1-\phi)^{(1-\alpha_2)}}\left[(1-q)\left(\Delta^M(q)\right)^{\alpha_1/(1-\alpha_2)} + q\left(\Delta^m(q)\right)^{\alpha_1/(1-\alpha_2)}\right]^{(1-\alpha_2)/\alpha_1},$$

where

$$Z_1 := (\alpha_2\rho)^{\alpha_2/(1-\alpha_2)}(1-\phi)^{-\alpha_1\alpha_2/(1-\alpha_2)}.$$

Thus,

$$\frac{\partial y_t^{i*}(q)}{\partial q} = \frac{(n\alpha_3\rho Z_1)^{(1-\alpha_2)/\alpha_1}}{(1-\phi)^{(1-\alpha_2)}}\frac{(1-\alpha_2)}{\alpha_1}\left[(1-q)\left(\Delta^M(q)\right)^{\alpha_1/(1-\alpha_2)} + q\left(\Delta^m(q)\right)^{\alpha_1/(1-\alpha_2)}\right]^{(1-\alpha_1-\alpha_2)/\alpha_1}\Psi(q),$$

where

$$\Psi(q) := \left(\Delta^m(q)\right)^{\alpha_1/(1-\alpha_2)} - \left(\Delta^M(q)\right)^{\alpha_1/(1-\alpha_2)}$$
$$+\left(b^m - b^M\right)\frac{\alpha_1\phi}{(1-\alpha_2)}\left[(1-q)\left(\Delta^M(q)\right)^{(\alpha_1+\alpha_2-1)/(1-\alpha_2)} + q\left(\Delta^m(q)\right)^{(\alpha_1+\alpha_2-1)/(1-\alpha_2)}\right].$$

Thus, $sign\left[\frac{\partial y_t^{i*}(q)}{\partial q}\right] = sign\Psi(q)$. Let us determine the sign of $\Psi(q)$. We want $\Psi(q) < 0$. Since $b^m < b^M$, we need to show that:

$$\left(\Delta^m(q)\right)^{\alpha_1/(1-\alpha_2)} < \left(\Delta^M(q)\right)^{\alpha_1/(1-\alpha_2)}.$$

This is equivalent to

$$\Delta^m(q) < \Delta^M(q).$$

That is,

$$(1-\phi(1-q))\,b^m + \phi(1-q)\,b^M < (1-\phi q)\,b^M + \phi q b^m,$$

which is equivalent to $b^m < b^M$. Thus, if $b^m < b^M$, we have:

$$\frac{\partial y_t^{i*}(q)}{\partial q} < 0.$$

### A.3.3 Parents' and teachers' effort choices in education production function

We have:

$$\begin{aligned} Z_1^{-1/\alpha_2}\frac{\partial y_p^M}{\partial q} &= \frac{\alpha_1}{(1-\alpha_2)}\left(\Delta^M(q)\right)^{-\alpha_3/(1-\alpha_2)}\phi\left(b^m - b^M\right)\left(y_t^{i*}(q)\right)^{\alpha_3/(1-\alpha_2)} \\ &\quad + \left(\Delta^M(q)\right)^{\alpha_1/(1-\alpha_2)}\frac{\alpha_3}{(1-\alpha_2)}\left(y_t^{i*}(q)\right)^{-\alpha_1/(1-\alpha_2)}\frac{\partial y_t^{i*}}{\partial q}\end{aligned}$$

If $b^m < b^M$, we clearly obtain:

$$\frac{\partial y_p^M}{\partial q} < 0$$

Similarly

$$\begin{aligned} Z_1^{-1/\alpha_2}\frac{\partial y_p^m}{\partial q} &= \frac{\alpha_1}{(1-\alpha_2)}\left(\Delta^m(q)\right)^{-\alpha_3/(1-\alpha_2)}\phi\left(b^m - b^M\right)\left(y_t^{i*}(q)\right)^{\alpha_3/(1-\alpha_2)} \\ &\quad + \left(\Delta^m(q)\right)^{\alpha_1/(1-\alpha_2)}\frac{\alpha_3}{(1-\alpha_2)}\left(y_t^{i*}(q)\right)^{-\alpha_1/(1-\alpha_2)}\frac{\partial y_t^{i*}(q)}{\partial q} \end{aligned}$$

If $b^m < b^M$, we also obtain:

$$\frac{\partial y_p^m}{\partial q} < 0$$

Consider now students' test scores, that is,

$$S^{M*} = \rho\,(1-\phi)^{-\alpha_1}\left(\Delta^M(q)\right)^{\alpha_1}\left(y_p^{M*}(q)\right)^{\alpha_2}\left(y_t^{i*}(q)\right)^{\alpha_3}$$

and

$$S^{m*} = \rho\,(1-\phi)^{-\alpha_1}\left(\Delta^m(q)\right)^{\alpha_1}\left(y_p^{m*}(q)\right)^{\alpha_2}\left(y_t^{i*}(q)\right)^{\alpha_3}$$

Thus

$$\frac{S^{M*}}{S^{m*}} = \left(\frac{\Delta^M(q)}{\Delta^m(q)}\right)^{\alpha_1}$$

Furthermore

$$\rho\,(1-\phi)^{\alpha_1}\frac{\partial S^{M*}}{\partial q} =$$

$$\begin{aligned} &\alpha_1\left(\Delta^M(q)\right)^{\alpha_1-1}\phi\left(b^m - b^M\right)\left[\left(y_p^{M*}(q)\right)^{\alpha_2}\left(y_t^{i*}(q)\right)^{\alpha_3}\right] \\ &+ \left(\Delta^M(q)\right)^{\alpha_1}\left[\alpha_2\left(y_p^{M*}(q)\right)^{\alpha_2-1}\frac{\partial y_p^{M*}(q)}{\partial q}\left(y_t^{i*}(q)\right)^{\alpha_3}\right] \\ &+ \left(\Delta^M(q)\right)^{\alpha_1}\alpha_3\left(y_p^{M*}(q)\right)^{\alpha_2}\left(y_t^{i*}(q)\right)^{\alpha_3-1}\frac{\partial y_t^{i*}(q)}{\partial q} \end{aligned}$$

Thus

$$\frac{\partial S^{M*}}{\partial q} < 0$$

With a similar calculation, we obtain

$$\frac{\partial S^{m*}}{\partial q} < 0$$

## Appendix B The construction of summary indices of student effort, parental investments and teacher engagement

Our model predicts changes in student effort, parental investments, and teacher engagement in response to exposure to minority peers in the classroom. We construct three indices capturing these inputs into student's education production function.

Since most educational inputs in the TEPS are measured using multiple questions, we first identify blocks of items in the questionnaires that measure related constructs, e.g. students' study hours. We then eliminate badly performing measures of the underlying construct aiming to maximize the informational content and reduce noise. Through Spearman correlations between all items in a block, Cronbach's alpha assessments, and exploratory factor analyses, we remove questions with low correlations to the rest from the block. Once we have narrowed down the list, have verified the performance of each question in a block, and have confirmed that the first factor loadings on each question are similar in magnitude, we construct a summative scale of all selected questions. This results in a series of scales, some of which measure student-driven inputs into the education production function whereas others measure parent- and teacher-driven inputs.

Our core student input is study effort, as captured by study hours. Our scale of study hours combines information from five different items measuring time spent at school, time spent in school-provided tutoring, study time outside school and school-provided tutoring, time spent on the internet for homework, and time spent tutoring during the summer break. All these measures are, a priori, meaningful contributors to academic achievement and can also be influenced by peers. Study effort is often considered as the main potential mechanism for academic peer effects (see e.g., Feld and Zoelitz, 2017, or Xu, Zhang and Zhou, 2022).

Parental investments are measured through three separate scales capturing private tutoring, time spent with the child, and emotional support. School environment has been shown to affect parental investments and academic achievement (see e.g., Pop-Eleches and Urquiola, 2013, Fredriksson, Öckert and Oosterbeek, 2016), and much of this work hypothesizes peers as a key driver of these effects. Furthermore, parental monetary and time investments are the canonical Beckerian household investments in human capital and can therefore respond as complements or substitutes to school inputs, such as exposure to minorities in classroom. The private tutoring scale combines a measure of the time spent in private tutoring outside school and the overall family expenditures in tutoring. The scale measuring time spent with the child combines measures of the weekly time each parent (when present) spends with their child during dinner time. In addition, parental support belongs to a broader set of parenting styles which can also be modelled as parental investments (Cobb-Clark, Salamanca and Zhu, 2019) and thus react to school inputs for similar reasons. Conceptually parental support is close to warmth and more generally measures parental engagement. The parental support scale combines three measures of whether one or both parents listen carefully to the child's thoughts and concerns, provide assistance and assurance when facing difficulties, and accept the child for who they are.

We measure teacher engagement through two separate scales that capture time and effort invested in preparing and delivering material by subject matter teachers, and a measure of whether the Dao Shi (or homeroom teacher) considers the classroom to be hard to manage. Both measures proxy for teacher inputs that make the classroom productive for learning, and can be affected by the social and cultural diversity. For example, lower-achieving peers such as Indigenous students might increase classroom disruption, making classroom management harder and decreasing teacher effort (Duflo, Dupas and Kremer, 2011). Teachers' effort might also be more productive if they feel more motivated and less tired of teaching when working with a more homogeneous classrooms with fewer minority students. The effort scale for subject matter teachers combines 12 measures capturing frequency of exams and quizzes, of homework assignments, of review and discussion sessions, and of extra individual tutorial to some students, all asked to teachers of Math, Chinese and English (the core subject matters in middle school). The scale for whether the classroom is hard to manage as perceived by the Dao Shi is a single dummy variable capturing this perception, which is directly related to the cost of effort for classroom management.

Finally, we further aggregate this still numerous set of scales into student-driven, parent-driven, and teacher-driven summary indices (corresponding to the $y_s^i$, $y_p^i$ and $y_t^i$ in our model). To ensure we capture the set of inputs most productive in the education production function for each of these indices, we perform three regression-based mappings of indices onto academic achievement: one for student effort, one for parental investments, and one for teacher effort. In our main analyses we then use these indices to explore the predictions of our model (Propositions 1 and 2).[41]

For student effort and parental investments, we construct these indices by regressing student test scores in wave 1 on student effort and parental investments in wave 1 (and school fixed effects). The coefficients of these regressions describe the contemporaneous "returns" of each input in the test score production function. Constructing these returns using wave 1 data ensures they are not endogenous to peer assignment. We then multiply these coefficients by the corresponding scales in wave 2 to generate the "forecasted scores" of student effort and parental investments that are productive for test scores.

For teacher engagement we cannot use the same procedure since teachers are randomly assigned to students at the beginning of junior high school, which means that teacher characteristics in

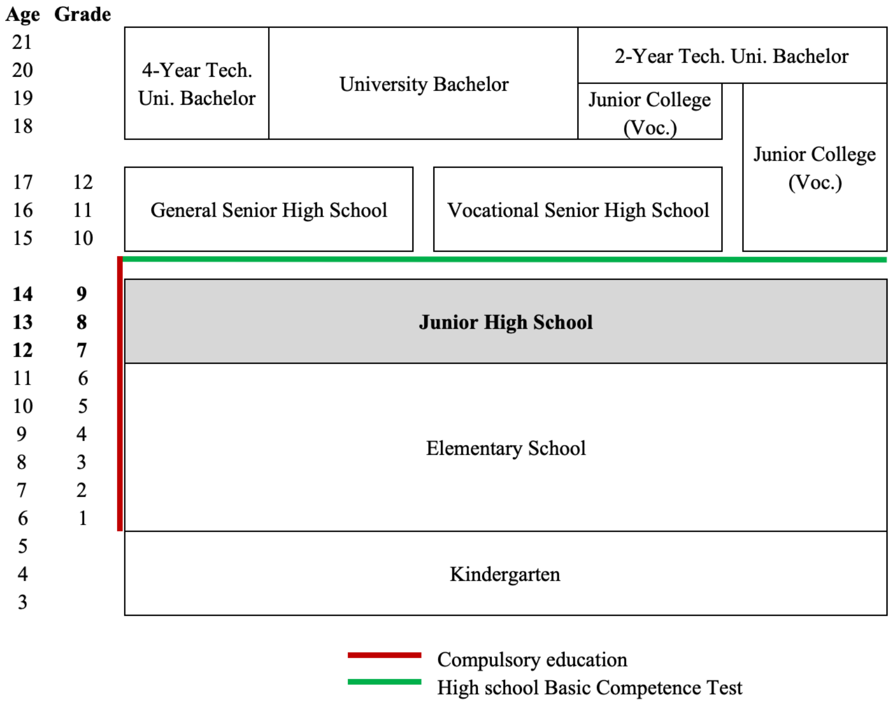
[41] Other alternative ways to construct aggregate indices of these inputs in an omnibus way, such as those proposed by Anderson (2008), Kling, Liebman and Katz (2007) or even simpler summative indices of all individual items, are not suitable for our purposes. The reason is that these methods implicitly assume that all items measure the same underlying construct. However, by construction, our items measure distinct constructs and so imposing a single-construct assumption when aggregating them is incorrect. Moreover, we are interested in separately identifying the role of students, parents and teachers, which means imposing additional informational structure on the data to capture *who* is making the investments we measure.

wave 1 are not correlated with student test scores in wave 1. Instead, we construct the index via a measure akin to predicted teacher value added to produce a mapping of teacher-related inputs to test scores. We do this by regressing wave 2 test scores on wave 1 teacher effort, and control for school-by-baseline-test-score fixed effects to account for baseline class composition.[42] The coefficients from this regression identify the "returns" to teacher-related inputs, which we then multiply by the corresponding teacher effort measure in wave 2 to produce predicted scores.

[42] Our test score measure–a count of the number of correct test answers–has 64 actual points of support. The school-by-baseline-test-score fixed effects means that there is a fixed effect for each student score and school (e.g., everyone who got 37 questions right in school 3 has a fixed effect, everyone who got 38 questions right in the same school gets a different fixed effect, etc.). Effectively, we absorb 4,572 school-by-baseline-test-score fixed effects with this approach, which is a very flexible non-parametric way of controlling for baseline test scores when producing the "predicted value added" for our teacher-related inputs.

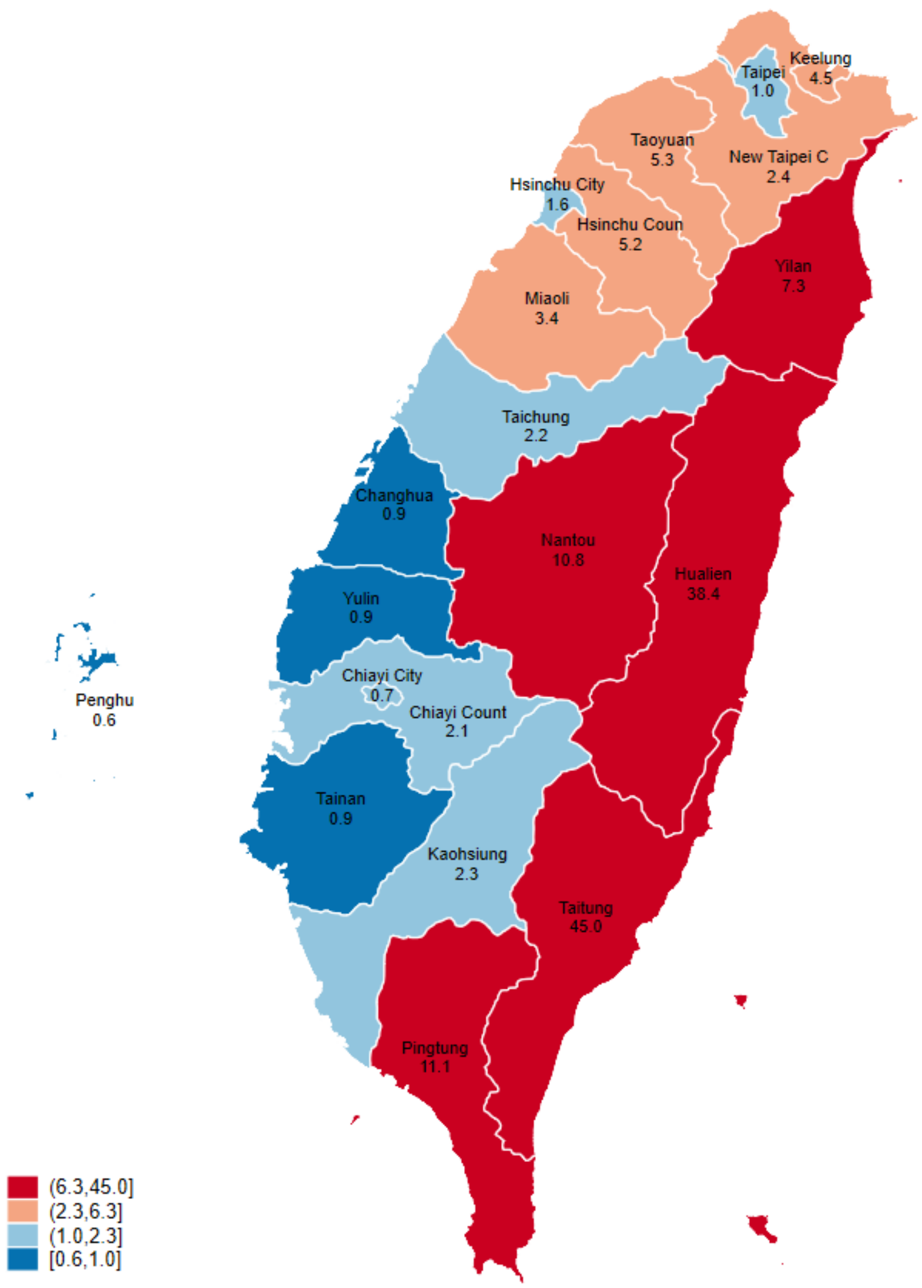
# Appendix C Additional figures and tables

**Figure C.1:** Schematic of the Taiwanese Education System



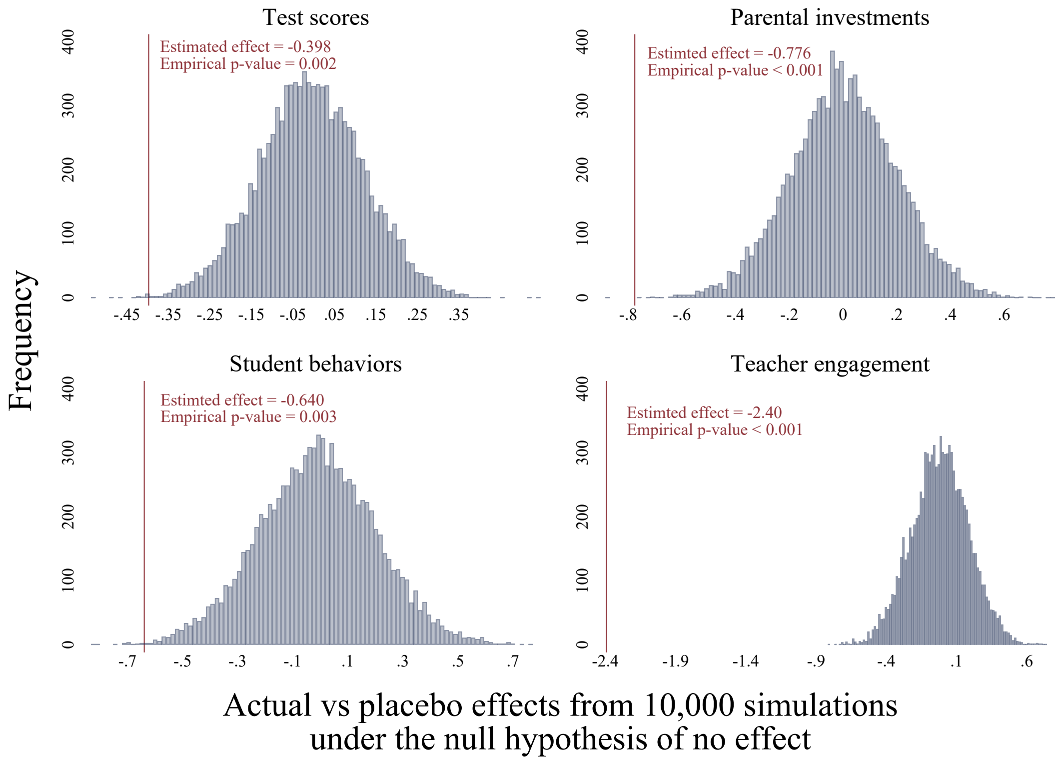

Note: This figure shows a simplified schematic of the Taiwanese education system in 2001. Elementary education covers Chinese, Mathematics, Science, Social Studies, Arts & Music, and Physical Education. Junior high school education covers the same subjects as elementary education in greater depth and complexity. In the non-compulsory senior high school education, students can choose to enroll in either the general track or the vocational track. The general track leads to university and the vocational track leads to vocational training. The figure omits the special education sector for students with special needs (e.g., with physical or mental disabilities). Special education is integrated into the mainstream education system, but also includes specialized schools and facilities for students requiring more specialized instruction.

**Figure C.2:** Fraction of Indigenous Children Among 10 to 14 Year Olds by County



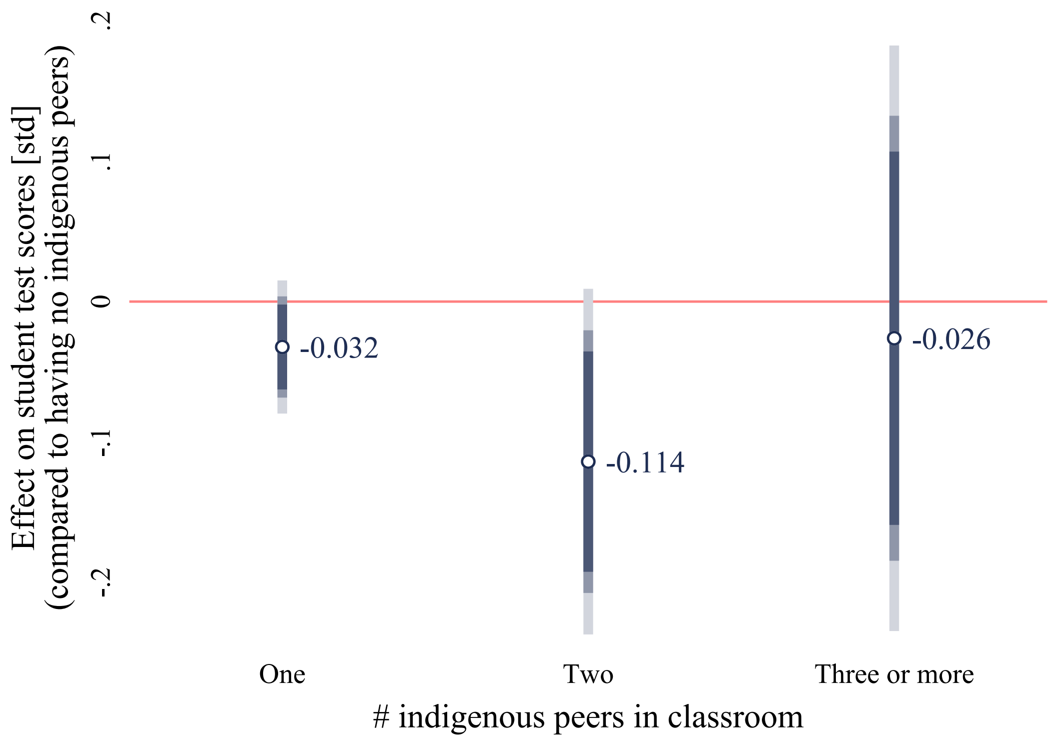

Note: This map represents the share of Indigenous children among 10 to 14 year old children in Taiwan by county of residence. Source: Taiwan Census 2010.

**Figure C.3:** Main Results of Exposure to Indigenous Students: Randomization Inference



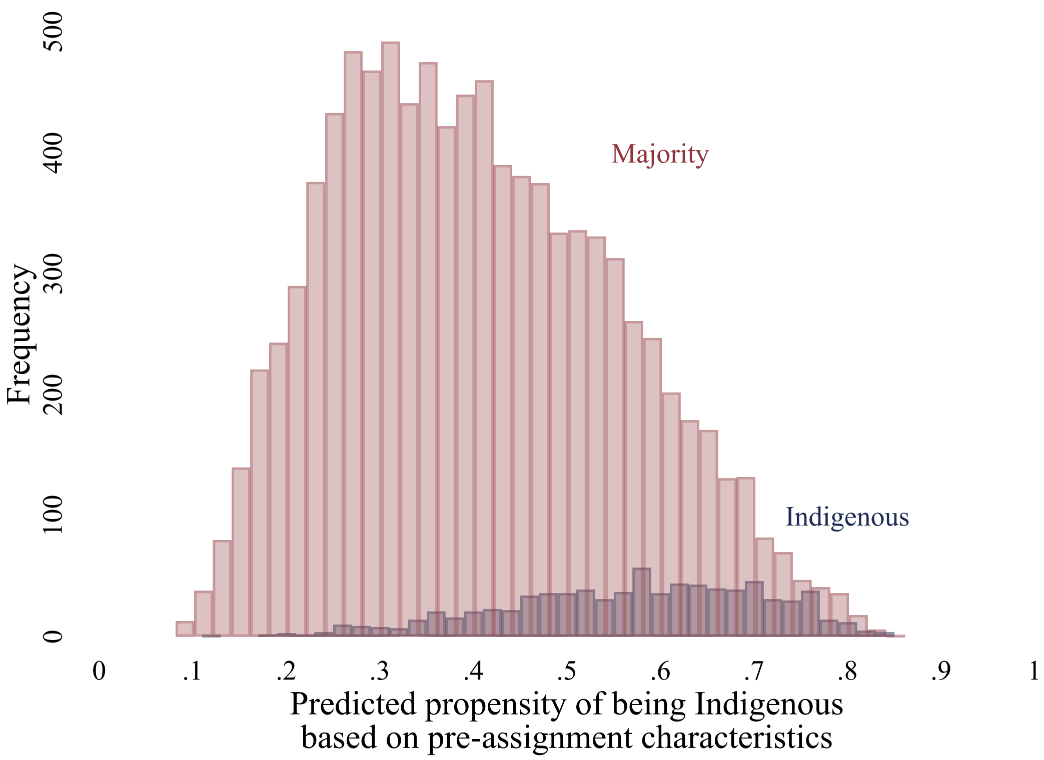

Note: These figures show results of a randomization inference computations where the treatment is the classroom share of Indigenous peers in wave 1. The results are based on estimating modified Equation 21 (dropping the interaction term between share of Indigenous peers and being Indigenous) where the placebo treatments are re-randomized shares of Indigenous students in the classroom in wave 1 within schools. We re-randomize the class structure 10,000 times. Red vertical lines present our point estimates from the preferred specification in column 3 of Table 2 all three columns of Table 4 (modified by removing the interaction term between share of Indigenous peers and being Indigenous). The empirical p-values are calculated as the share of re-randomized effects that are larger than the true effects.

**Figure C.4:** Non-linear Peer Effects on Test Scores




Note: This figure shows the effects on standardized test scores in wave 2 of being exposed to one, two, or three Indigenous peers in the classroom in wave 1. All regressions include wave 1 school fixed effects and control for students' own test scores in wave 1 and pre-assignment characteristics, and all controls are interacted with an Indigenous student dummy. Standard errors are clustered at the classroom level.

**Figure C.5:** Common Support in Propensity-Score Matching of Indigenous and Synthetic Indigenous Students




Note: This figure shows the propensity score that predicts being an Indigenous student, separately for the Indigenous and the majority students. These propensity scores come from predicted probabilities of a probit regression of an Indigenous binary variable on test scores and pre-assignment characteristics. School fixed effects are included in this regression but are not used to produce the fitted values.

**Table C.1:** Negative Teacher Perceptions about Indigenous Students' Ability

| **Outcome:** | **Above-average at problem solving (teacher assessment in W1)** | | | |
|---|---|---|---|---|
| Indigenous student | -0.121*** | -0.153*** | -0.049** | -0.086*** |
| | (0.024) | (0.022) | (0.021) | (0.028) |
| Test scores in W1 [std] | | | 0.234*** | 0.237*** |
| | | | (0.005) | (0.006) |
| Indigenous × Test scores | | | | -0.056*** |
| | | | | (0.021) |
| School FE | | ✓ | ✓ | ✓ |
| Outcome mean | 0.30 | 0.30 | 0.30 | 0.30 |
| $R^2$ | 0.00 | 0.05 | 0.25 | 0.25 |
| Schools | 154 | 154 | 154 | 154 |
| Classes | 576 | 576 | 576 | 576 |
| Students | 7,579 | 7,579 | 7,579 | 7,579 |

Note: This table reports estimates of regressing a measure of teachers' subjective assessment of student problem-solving ability in wave 1 on an Indigenous student dummy, students' test scores in wave 1, and their interaction. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

**Table C.2:** Association between Wave 2 Test Scores and Long-Term Outcomes

| **Treatment:** | | **Test scores in wave 2 [std]** | | | | | |
|---|---|---|---|---|---|---|---|
| | **Mean** | **Coef.** | **Std. err.** | **$R^2$** | **Schools** | **Classes** | **Students** |
| **Outcomes:** | | | | | | | |
| Test scores in 2005 [Std] | -0.46 | 0.816*** | (0.020) | 0.69 | 155 | 535 | 1,753 |
| Test scores in 2007 [Std] | -0.39 | 0.763*** | (0.020) | 0.66 | 155 | 531 | 1,705 |
| Enrolled or finished university in 2009 | 0.50 | 0.263*** | (0.014) | 0.43 | 153 | 507 | 1,342 |
| Enrolled or finished vocational education in 2009 | 0.37 | -0.158*** | (0.016) | 0.26 | 153 | 507 | 1,342 |
| Enrolled or finished university in 2013 | 0.38 | 0.166*** | (0.016) | 0.26 | 153 | 501 | 1,300 |
| Enrolled or finished in postgraduate studies in 2013 | 0.17 | 0.093*** | (0.011) | 0.20 | 153 | 501 | 1,300 |
| Earns above-median income in 2013 | 0.46 | 0.112*** | (0.019) | 0.22 | 148 | 451 | 909 |

Note: This table reports estimates of regressing student outcomes in senior high school or after graduation on test scores in wave 2. These outcomes are measured in subsequent waves of the TEPS and only for a subset of students. Selective attrition is accounted for via inverse probability weights. All regressions include wave 1 school fixed effects. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

**Table C.3:** Descriptive Statistics

| | Mean of characteristics in sample: | | | | Difference | |
|---|---|---|---|---|---|---|
| | TEPS | Estimation | Indigenous | Majority | Raw | School FEs |
| Indigenous parent(s) | 0.04 | 0.07 | 1.00 | 0.00 | 1.00*** | 1.00*** |
| Hakka parent(s) | 0.17 | 0.21 | 0.06 | 0.22 | -0.16*** | -0.23*** |
| Waishengren parent(s) | 0.18 | 0.15 | 0.07 | 0.16 | -0.08*** | -0.06*** |
| Hoklo parent(s) | 0.82 | 0.80 | 0.18 | 0.85 | -0.67*** | -0.55*** |
| Test scores at baseline [std] | 0.00 | -0.14 | -0.80 | -0.09 | -0.72*** | -0.42*** |
| Female student | 0.50 | 0.49 | 0.51 | 0.49 | 0.02 | 0.06** |
| Student aspires to go to university | 0.45 | 0.43 | 0.33 | 0.43 | -0.11*** | -0.04 |
| Student expects to go to university | 0.35 | 0.32 | 0.25 | 0.33 | -0.08*** | -0.04* |
| Fluent in | | | | | | |
| Mandarin | 0.91 | 0.92 | 0.88 | 0.92 | -0.04** | -0.02 |
| Taiwanese | 0.37 | 0.37 | 0.10 | 0.39 | -0.28*** | -0.27*** |
| Hakka | 0.04 | 0.05 | 0.03 | 0.05 | -0.02*** | -0.04*** |
| Indigenous language | 0.02 | 0.03 | 0.29 | 0.01 | 0.28*** | 0.25*** |
| English | 0.15 | 0.13 | 0.05 | 0.14 | -0.09*** | -0.05*** |
| Foreign language | 0.03 | 0.02 | 0.01 | 0.02 | -0.01 | -0.01 |
| Parent(s) education is HS or less | 0.65 | 0.71 | 0.80 | 0.71 | 0.09*** | 0.08*** |
| Household monthly income is | | | | | | |
| NT$20,000 or less | 0.11 | 0.12 | 0.30 | 0.11 | 0.20*** | 0.11*** |
| NT$20,000-NT$50,000 | 0.41 | 0.46 | 0.47 | 0.45 | 0.01 | 0.01 |
| NT$50,000-NT$100,000 | 0.35 | 0.33 | 0.20 | 0.34 | -0.15*** | -0.07*** |
| More than NT$100,000 | 0.14 | 0.09 | 0.03 | 0.09 | -0.06*** | -0.05*** |
| Parent(s) in good health | 0.59 | 0.58 | 0.33 | 0.60 | -0.28*** | -0.24*** |
| Financial difficulties in past 10 yrs | 0.27 | 0.30 | 0.60 | 0.28 | 0.32*** | 0.26*** |
| No. of siblings of student | 1.77 | 1.86 | 2.36 | 1.82 | 0.54*** | 0.27*** |
| Family helps with homework since | | | | | | |
| Before primary school | 0.49 | 0.51 | 0.53 | 0.51 | 0.02 | -0.01 |
| Before junior high school | 0.34 | 0.33 | 0.29 | 0.33 | -0.04* | -0.02 |
| Junior high school | 0.83 | 0.84 | 0.82 | 0.84 | -0.02 | -0.02 |
| Child has private tutor since | | | | | | |
| Before primary school | 0.34 | 0.32 | 0.19 | 0.33 | -0.14*** | -0.09*** |
| Before junior high school | 0.68 | 0.65 | 0.40 | 0.67 | -0.27*** | -0.14*** |
| This semester, the student | | | | | | |
| Cheated on exams | 0.15 | 0.15 | 0.17 | 0.15 | 0.01 | 0.02 |
| Skipped class | 0.04 | 0.04 | 0.10 | 0.03 | 0.06*** | 0.04*** |
| Quarreled with teacher | 0.07 | 0.07 | 0.11 | 0.07 | 0.05*** | 0.02 |
| Watched porn | 0.04 | 0.04 | 0.06 | 0.03 | 0.02* | 0.01 |
| Smoked, drank, or chewed areca | 0.04 | 0.04 | 0.11 | 0.04 | 0.08*** | 0.05*** |
| Ran away from home | 0.03 | 0.02 | 0.05 | 0.02 | 0.02** | 0.01 |
| Stole/destroyed property | 0.04 | 0.04 | 0.06 | 0.04 | 0.03* | 0.02* |
| Observations | 20,055 | 8,517 | 588 | 7,929 | 8,517 | 8,517 |

Note: This table reports means for key student and household variables in the TEPS data. Column 1 reports means for the entire data, column 2 reports means for our estimation sample, columns 3 and 4 divide the estimation sample into the Indigenous and the majority students. Subsequent two columns test whether characteristics of the Indigenous and the majority students are statistically different without (column 5) and with wave 1 school fixed effects (column 6). Raw mean differences (column 5) between the Indigenous and the majority students are calculated using two-sample t-tests assuming unequal variances. Differences conditional on wave 1 school fixed effects (column 6) absorb within-school variation and the standard errors of the mean differences are clustered at the classroom level. In both cases, ***, ** and * mark mean differences between the Indigenous and the majority students statistically different from zero at the 99, 95 and 90 percent confidence level. The bottom row reports the maximum number of observations. For select variables mean estimates might use fewer observations due to missing data points.

**Table C.4:** Descriptive Statistics across Four Student Groups

| | Mean of characteristics for: | | | |
|---|---|---|---|---|
| | **Indigenous** | **Hakka** | **Waishengren** | **Hoklo** |
| Indigenous parent(s) | 1.00 | 0.02 | 0.03 | 0.02 |
| Hakka parent(s) | 0.06 | 1.00 | 0.15 | 0.12 |
| Waishengren parent(s) | 0.07 | 0.11 | 1.00 | 0.11 |
| Hoklo parent(s) | 0.18 | 0.47 | 0.58 | 1.00 |
| Test scores at baseline [std] | -0.80 | -0.12 | 0.11 | -0.09 |
| Female student | 0.51 | 0.49 | 0.50 | 0.49 |
| Student aspires to go to university | 0.33 | 0.43 | 0.49 | 0.43 |
| Student expects to go to university | 0.25 | 0.33 | 0.39 | 0.33 |
| Fluent in | | | | |
| Mandarin | 0.88 | 0.92 | 0.95 | 0.92 |
| Hokkien | 0.10 | 0.24 | 0.22 | 0.43 |
| Hakka | 0.03 | 0.17 | 0.04 | 0.02 |
| Indigenous language | 0.29 | 0.01 | 0.01 | 0.01 |
| English | 0.05 | 0.14 | 0.17 | 0.14 |
| Foreign language | 0.01 | 0.02 | 0.04 | 0.02 |
| Parent(s) education is HS or less | 0.80 | 0.71 | 0.51 | 0.73 |
| Household monthly income is | | | | |
| NT$20,000 or less | 0.30 | 0.12 | 0.07 | 0.11 |
| NT$20,000-NT$50,000 | 0.47 | 0.43 | 0.36 | 0.46 |
| NT$50,000-NT$100,000 | 0.20 | 0.36 | 0.40 | 0.34 |
| More than NT$100,000 | 0.03 | 0.10 | 0.16 | 0.09 |
| Parent(s) in good health | 0.33 | 0.61 | 0.58 | 0.60 |
| Financial difficulties in past 10 yrs | 0.60 | 0.26 | 0.24 | 0.29 |
| No. of siblings of student | 2.36 | 1.89 | 1.54 | 1.83 |
| Family helps with homework since | | | | |
| Before primary school | 0.53 | 0.51 | 0.51 | 0.52 |
| Before junior high school | 0.29 | 0.34 | 0.35 | 0.33 |
| Junior high school | 0.82 | 0.85 | 0.86 | 0.84 |
| Child has private tutor since | | | | |
| Before primary school | 0.19 | 0.31 | 0.36 | 0.33 |
| Before junior high school | 0.40 | 0.65 | 0.66 | 0.68 |
| This semester, the student | | | | |
| Cheated on exams | 0.17 | 0.14 | 0.18 | 0.15 |
| Skipped class | 0.10 | 0.03 | 0.02 | 0.03 |
| Quarreled with teacher | 0.11 | 0.06 | 0.07 | 0.07 |
| Watched porn | 0.06 | 0.04 | 0.03 | 0.03 |
| Smoked, drank, or chewed areca | 0.11 | 0.04 | 0.03 | 0.03 |
| Ran away from home | 0.05 | 0.02 | 0.02 | 0.02 |
| Stole/destroyed property | 0.06 | 0.04 | 0.03 | 0.03 |
| Observations | 588 | 1,755 | 1,276 | 6,846 |

Note: This table reports means for key student and household variables in the TEPS data across the Indigenous, the Hakka, the Waishengren, and the Hoklo gropups. The bottom row reports the maximum number of observations. For select variables mean estimates might use fewer observations due to missing data points.

**Table C.5:** Teacher Perceptions about Hakka and Synthetic Indigenous Students' Ability

| | Panel A: Hakka students | | | |
|---|---|---|---|---|
| **Outcome:** | **Above-average at problem solving (teacher assessment in W1)** | | | |
| Hakka student | 0.035** | 0.035** | 0.013 | 0.013 |
| | (0.016) | (0.016) | (0.014) | (0.015) |
| Test scores in W1 [std] | | | 0.235*** | 0.234*** |
| | | | (0.005) | (0.006) |
| Hakka × Test scores | | | | 0.001 |
| | | | | (0.012) |
| School FE | | ✓ | ✓ | ✓ |
| Outcome mean | 0.30 | 0.30 | 0.30 | 0.30 |
| $R^2$ | <0.01 | 0.04 | 0.25 | 0.25 |
| Schools | 154 | 154 | 154 | 154 |
| Classes | 576 | 576 | 576 | 576 |
| Students | 7,579 | 7,579 | 7,579 | 7,579 |
| | **Panel B: Synthetic Indigenous students** | | | |
| **Outcome:** | **Above-average at problem solving (teacher assessment in W1)** | | | |
| Synthetic Indigenous | 0.063** | -0.073 | -0.017 | -0.029 |
| | (0.026) | (0.065) | (0.020) | (0.032) |
| Test scores in W1 [std] | | | 0.231*** | 0.231*** |
| | | | (0.005) | (0.005) |
| Synthetic Indigenous × Test scores | | | | -0.018 |
| | | | | (0.024) |
| School FE | | ✓ | ✓ | ✓ |
| Outcome mean | 0.30 | 0.30 | 0.30 | 0.30 |
| R2 | <0.01 | 0.04 | 0.25 | 0.25 |
| Schools | 154 | 154 | 154 | 154 |
| Classes | 576 | 576 | 576 | 576 |
| Students | 7,579 | 7,579 | 7,579 | 7,579 |

Note: This table reports estimates of regressing a measure of teachers' subjective assessment of student problem-solving ability in wave 1 on a Hakka student dummy, students' test scores in wave 1, and their interaction in Panel A, and on a Synthetic Indigenous student dummy, students' test scores in wave 1, and their interaction in Panel B. A synthetic Indigenous student is defined as the majority student whose observable characteristics are closest to their Indigenous peer in school. This proximity is operationalized via propensity score matching using test scores and all preassignment characteristics in wave 1. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

**Table C.6:** The Effects of Exposure to Indigenous Students in the Classroom on Test Scores: Heterogeneity

| | Effect of share of Indigenous peers on test scores | | | | | |
|---|---|---|---|---|---|---|
| | Gender | | Student W1 test score | | Location within country | |
| | Boys | Girls | Below median | Above median | West (low Indigenous density) | East (high Indigenous density) |
| Share of Indigenous peers | -0.449*** | -0.336** | -0.264* | -0.674*** | -0.586*** | -0.064 |
| | (0.143) | (0.147) | (0.135) | (0.175) | (0.196) | (0.197) |
| Difference (p-value) | 0.190 | | <0.001 | | 0.062 | |

Note: This table reports average marginal effects from regressions of wave 2 test scores on the share of Indigenous peers in the classroom in wave 1 interacted with indicators for female student, above-median student test score in wave 1, and school located in the eastern part of the country, one at the time. This is akin to specification from column 3 of Table 2 but including interaction terms. Column 1 and 2 present results by gender, column 3 and 4 by baseline test scores, and columns 5 and 6 by geography. All regressions include wave 1 school fixed effects and control for students' own test scores in wave 1 and pre-assignment characteristics, and all controls are interacted with an Indigenous student dummy. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

**Table C.7:** Effects of Exposure to Low and High Ability Indigenous Students on Test Scores and Endogenous Responses of Students, Parents, and Teachers

| **Outcomes [std]:** | **Test scores** | **Student behaviors** | **Parental investments** | **Teacher engagement** |
|---|---|---|---|---|
| Share of low-ability Indigenous peers | -0.385*** | -0.493 | -0.804*** | -3.231*** |
| | (0.143) | (0.314) | (0.246) | (0.710) |
| Share of high-ability Indigenous peers | -0.392 | -1.164** | -0.650* | 0.527 |
| | (0.353) | (0.465) | (0.392) | (1.149) |
| Difference (p-value) | 0.985 | 0.144 | 0.692 | 0.003 |
| R-Squared | 0.63 | 0.19 | 0.23 | 0.29 |
| Students | 8,517 | 8,461 | 8,416 | 7,935 |

Note: This table reports estimates of regressing standardized test scores and aggregate indices of student-related, parent-related, and teacher-related inputs in wave 2 on the share of low and high ability Indigenous peers in the classroom in wave 1. Low and high ability Indigenous peers are defined as Indigenous peers scoring below or above the median of all students on the wave 1 standardized test. All regressions include school fixed effects and control for students' own test scores in wave 1 and pre-assignment characteristics, and all controls are interacted with dummies for high and low ability Indigenous student. This is akin to specification from column 3 of Table 2 but where we consider shares separated by ability. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

**Table C.8:** Correlation between Test Scores and Student, Parent and Teacher Inputs

| | | | | |
|---|---|---|---|---|
| **Panel A: Student test scores in W2** | | | | |
| Student behaviors in W2 [std] | 0.256*** | | | 0.239*** |
| | (0.011) | | | (0.012) |
| Parental investments in W2 [std] | | 0.112*** | | 0.079*** |
| | | (0.012) | | (0.012) |
| Teacher engagement in W2 [std] | | | 0.074*** | 0.061*** |
| | | | (0.017) | (0.015) |
| School FE | ✓ | ✓ | ✓ | ✓ |
| Pre-assignment controls | ✓ | ✓ | ✓ | ✓ |
| R-Squared | 0.31 | 0.25 | 0.25 | 0.32 |
| Students | 8,461 | 8,416 | 7,935 | 7,844 |
| **Panel B: Student test scores in W3 (2005)** | | | | |
| Student behaviors in W2 [std] | 0.232*** | | | 0.224*** |
| | (0.028) | | | (0.030) |
| Parental investments in W2 [std] | | 0.082*** | | 0.071*** |
| | | (0.026) | | (0.026) |
| Teacher engagement in W2 [std] | | | 0.084** | 0.068** |
| | | | (0.034) | (0.030) |
| School FE | ✓ | ✓ | ✓ | ✓ |
| Pre-assignment controls | ✓ | ✓ | ✓ | ✓ |
| R-Squared | 0.33 | 0.29 | 0.30 | 0.34 |
| Students | 1,742 | 1,739 | 1,647 | 1,635 |
| **Panel C: Student test scores in W4 (2007)** | | | | |
| Student behaviors in W2 [std] | 0.222*** | | | 0.199*** |
| | (0.030) | | | (0.031) |
| Parental investments in W2 [std] | | 0.115*** | | 0.098*** |
| | | (0.029) | | (0.029) |
| Teacher engagement in W2 [std] | | | 0.075** | 0.071** |
| | | | (0.034) | (0.032) |
| School FE | ✓ | ✓ | ✓ | ✓ |
| Pre-assignment controls | ✓ | ✓ | ✓ | ✓ |
| R-Squared | 0.37 | 0.34 | 0.34 | 0.38 |
| Students | 1,696 | 1,693 | 1,598 | 1,588 |

Note: This table reports estimates of regressing standardized test scores in waves 2 (Panel A), 3 (Panel B) and 4 (Panel C) on aggregate indices of student-related, parent-related, and teacher-related inputs in wave 2. All regressions include wave 1 school fixed effects and pre-assignment characteristics which are interacted with an Indigenous student dummy. Regressions in Panels B and C use inverse probability weights to correct for potentially selective sample attrition. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

**Table C.9:** Association between the Share of Indigenous Students in the Classroom and Test Scores: Conditioning on Correlated Observables and Endogenous Responses of Students, Parents, and Teachers

| **Outcome [std]:** | **Student test scores** | | | |
|---|---|---|---|---|
| Share of Indigenous peers | -0.398*** | -0.286* | -0.241* | -0.120 |
| | (0.139) | (0.159) | (0.142) | (0.155) |
| Other peer characteristics | | ✓ | | ✓ |
| Mechanisms | | | ✓ | ✓ |
| Fraction explained | N/A | 28% | 39% | 70% |
| $R^2$ | 0.63 | 0.64 | 0.65 | 0.65 |
| Schools | 155 | 155 | 155 | 155 |
| Classes | 588 | 588 | 588 | 588 |
| Students | 8,517 | 8,517 | 8,517 | 8,517 |

Note: This table reports estimates of regressing standardized student test scores in wave 2 on the share of Indigenous peers in the classroom in wave 1. Column 1 replicates results using specification from column 3 of Table 2, column 2 controls for correlated peer characteristics considered in Table 3, column 3 controls for mechanisms considered in Table 4, and finally column 4 considers both of these sets of controls jointly. All regressions include wave 1 school fixed effects and control for students' own test scores in wave 1 and pre-assignment characteristics, and all controls are interacted with an Indigenous student dummy. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

**Table C.10:** Robustness Checks: Alternative Samples —Excluding Schools

| **Excluded schools:** | **None (baseline)** | **Indigenous majority** | **Indig./Hakka majority** | **Private** |
|---|---|---|---|---|
| **Outcome [std]:** | | | **Student test scores** | |
| Share of Indigenous peers | -0.398*** | -0.468*** | -0.432*** | -0.384*** |
| | (0.139) | (0.147) | (0.156) | (0.142) |
| Students | 8,517 | 8,166 | 7,286 | 8,251 |
| **Outcome [std]:** | | | **Student behaviors** | |
| Share of Indigenous peers | -0.639** | -0.592* | -0.503 | -0.571* |
| | (0.303) | (0.318) | (0.334) | (0.306) |
| Students | 8,461 | 8,110 | 7,231 | 8,195 |
| **Outcome [std]:** | | | **Parental investments** | |
| Share of Indigenous peers | -0.776*** | -0.841*** | -0.816*** | -0.780*** |
| | (0.236) | (0.255) | (0.272) | (0.242) |
| Students | 8,416 | 8,074 | 7,200 | 8,150 |
| **Outcome [std]:** | | | **Teacher engagement** | |
| Share of Indigenous peers | -2.393*** | -2.301*** | -2.659*** | -2.499*** |
| | (0.665) | (0.646) | (0.687) | (0.683) |
| Students | 7,935 | 7,590 | 6,743 | 7,686 |

Note: This table reports estimates of regressing standardized outcomes in wave 2 on the share of Indigenous peers in the classroom in wave 1 using specification from column 3 of Table 2. Column 1 replicates results using specification from column 3 of Table 2 for test scores (Table 2) as well as student, parental, and teacher inputs (Table 4). Column 2 drops schools where Indigenous students constitute a statistical majority, column 3 drops schools where either Hakka or Indigenous students constitute a statistical majority, and column 4 drops private schools. All regressions include wave 1 school fixed effects and control for students' own test scores in wave 1 and pre-assignment characteristics, and all controls are interacted with an Indigenous student dummy. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

**Table C.11:** Robustness Checks: Alternative Samples —Excluding Majority Student Subgroups

| **Excluded students:** | **None (baseline)** | **Hakka** | **Waishengren** | **Hakka & Waishengren** |
|---|---|---|---|---|
| **Outcome [std]:** | | **Student test scores** | | |
| Share of Indigenous peers | -0.398*** | -0.375** | -0.277* | -0.265* |
| | (0.139) | (0.146) | (0.143) | (0.149) |
| Students | 8,517 | 6,762 | 7,241 | 5,675 |
| **Outcome [std]:** | | **Student behaviors** | | |
| Share of Indigenous peers | -0.639** | -0.658* | -0.632** | -0.680* |
| | (0.303) | (0.348) | (0.302) | (0.354) |
| Students | 8,461 | 6,713 | 7,192 | 5,633 |
| **Outcome [std]:** | | **Parental investments** | | |
| Share of Indigenous peers | -0.776*** | -0.780*** | -0.870*** | -0.829*** |
| | (0.236) | (0.255) | (0.233) | (0.251) |
| Students | 8,416 | 6,680 | 7,154 | 5,607 |
| **Outcome [std]:** | | **Teacher engagement** | | |
| Share of Indigenous peers | -2.393*** | -2.487*** | -2.394*** | -2.478*** |
| | (0.665) | (0.634) | (0.671) | (0.631) |
| Students | 7,935 | 6,273 | 6,738 | 5,256 |

Note: This table reports estimates of regressing standardized outcomes in wave 2 on the share of Indigenous peers in the classroom in wave 1 using specification from column 3 of Table 2. Column 1 replicates results using specification from column 3 of Table 2 for test scores (Table 2) as well as student, parental, and teacher inputs (Table 4). Column 2 drops Hakka students, column 3 drops Waishengren students, and column 4 drops both Hakka and Waishengren students. All regressions include wave 1 school fixed effects and control for students' own test scores in wave 1 and pre-assignment characteristics, and all controls are interacted with an Indigenous student dummy. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

**Table C.12:** Main Results Scaled by Inverse Probability Weights

| **Weights:** | **None (baseline)** | **IPW for selection into estimation sample** |
|---|---|---|
| **Outcome [std]:** | **Student test scores** | |
| Share of Indigenous peers | -0.398*** | -0.596*** |
| | (0.139) | (0.192) |
| Students | 8,517 | 8,517 |
| **Outcome [std]:** | **Student behaviors** | |
| Share of Indigenous peers | -0.639** | -0.951*** |
| | (0.303) | (0.298) |
| Students | 8,461 | 8,461 |
| **Outcome [std]:** | **Parental investments** | |
| Share of Indigenous peers | -0.776*** | -0.686*** |
| | (0.236) | (0.233) |
| Students | 8,416 | 8,416 |
| **Outcome [std]:** | **Teacher engagement** | |
| Share of Indigenous peers | -2.393*** | -2.137*** |
| | (0.665) | (0.672) |
| Students | 7,935 | 7,935 |

Note: This table reports estimates of regressing standardized outcomes in wave 2 on the share of Indigenous peers in the classroom in wave 1. Column 1 replicates results using specification from column 3 of Table 2 for test scores (Table 2) as well as student, parental, and teacher inputs (Table 4). Column 2 shows estimates using inverse probability weights (IPW) to account for differences between our main estimation sample and the entire TEPS data. Probability weights use pre-assignment student and peer characteristics as predictors, but exclude Indigenous student dummies, Indigenous peer measures, and wave 1 school fixed effects. We restrict weights to their $1^{st}$ and $99^{th}$ percentiles. Main regressions include wave 1 school fixed effects and control for students' own test scores in wave 1 and pre-assignment characteristics, and all controls are interacted with an Indigenous student dummy. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

**Table C.13:** Falsification Test Using Hakka and Synthetic Indigenous Students

| Panel A: Exposure to Hakka peers | | | | |
|---|---|---|---|---|
| **Outcome [std]:** | **Student test scores** | **Student behaviors** | **Parental investments** | **Teacher engagement** |
| Share of Hakka peers | -0.037 | 0.225 | 0.023 | -0.169 |
| | (0.097) | (0.161) | (0.137) | (0.414) |
| Hakka peers × Hakka | 0.024 | 0.157 | 0.075 | 0.143 |
| | (0.074) | (0.137) | (0.101) | (0.139) |
| $R^2$ | 0.63 | 0.19 | 0.22 | 0.26 |
| Schools | 155 | 155 | 155 | 155 |
| Classes | 588 | 588 | 588 | 578 |
| Students | 8,517 | 8,461 | 8,416 | 7,935 |

| Panel B: Exposure to synthetic Indigenous peers | | | | |
|---|---|---|---|---|
| **Outcome [std]:** | **Student test scores** | **Student behaviors** | **Parental investments** | **Teacher engagement** |
| Share of synthetic Indigenous peers | -0.022 | 0.038 | 0.619*** | -0.612 |
| | (0.145) | (0.281) | (0.233) | (0.631) |
| Synthetic peers × Synthetic Indigenous | 0.182 | 0.306 | 0.971** | 0.787* |
| | (0.201) | (0.421) | (0.442) | (0.458) |
| R2 | 0.63 | 0.19 | 0.22 | 0.26 |
| Schools | 155 | 155 | 155 | 155 |
| Classes | 588 | 588 | 588 | 578 |
| Students | 8,517 | 8,461 | 8,416 | 7,935 |

Note: Panel A of this table reports estimates of regressing standardized outcomes in wave 2 on the share of Hakka peers in the classroom in wave 1, and its interaction with a Hakka student dummy. Panel B of this table reports estimates of regressing standardized outcomes in wave 2 on the share of synthetic Indigenous peers in the classroom in wave 1, and its interaction with a synthetic Indigenous student dummy. A synthetic Indigenous student is defined as the majority student whose observable characteristics are closest to their Indigenous peer in school. This proximity is operationalized via propensity score matching using test scores and all preassignment characteristics in wave 1. All regressions include wave 1 school fixed effects and control for students' own test scores in wave 1 and pre-assignment characteristics, and all controls are interacted with a Hakka (Panel A) or synthetic Indigenous (Panel B) student dummy. Standard errors clustered at the classroom level are in parentheses. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.