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A familiar face: Student-teacher rematches and student achievement

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ABSTRACT

We use administrative data from Indiana to test whether student-teacher rematches in consecutive years affect student achievement. Using models that control for student and teacher fixed effects, we show that student-teacher rematches increase test scores in math and English Language Arts. The positive effects of rematching are constant over elementary and middle-school grades and more pronounced for historically underserved students. Our findings directly support strategies that aim to keep students and teachers together for longer periods of time during K-12 education. They are also consistent with the broader hypothesis that students benefit from increased student-teacher familiarity.

"In order to teach you, I must know you"
- Lisa Delpit (2006, p.183). *Other People's Children: Cultural conflict in the classroom.*

1. Introduction

Policies that aim to improve teacher effectiveness can be divided into two categories. One category focuses on recruiting and retaining more effective teachers. The other aims to improve the effectiveness of the existing teacher workforce. An example of a policy in the latter category is one that keeps students and teachers together for longer periods of time. This can improve teaching and learning by allowing teachers to forge stronger relationships with students and their families (Grant et al., 1996; Little & Dacus, 1999; Minkel, 2015; Nichols & Nichols, 2003).

We contribute to the literature on student-teacher familiarity by estimating the impacts of repeated student-teacher matches on achievement using a seven-year administrative data panel from Indiana. Student-teacher rematching is a key component of broader "looping" policies, which assign students to the same teachers and classmates in consecutive years. Looping policies have long been touted for their potential benefits by educators and scholars alike, who place particular emphasis on their value in strengthening student-teacher relationships (Grant et al., 1996; Little & Dacus, 1999; Minkel, 2015).[1]

Our research design isolates the impacts of student-teacher rematching conditional on fixed student and teacher attributes (observed and unobserved), permitting a plausibly causal interpretation of our estimates. A placebo test designed to uncover evidence of selection into matched student-teacher pairs further supports this interpretation. Following on related work by Hill and Jones (2018), we find that student-teacher rematches increase both math and English Language Arts (ELA) achievement, on average. We also test for heterogenous effects of rematches along two dimensions. First, motivated by the hypothesis that stronger student-teacher relationships are more important for historically underserved student populations (e.g., Eccles & Roeser, 2011; Pianta et al., 1995), we test for effect heterogeneity across students who differ by race/ethnicity, free or reduced-price lunch (FRL) status, English language learner (ELL) status, individualized education program (IEP) status, and prior test scores. We find that the positive effects of rematching are consistently more pronounced for racial/ethnic minority students, English language learners, and students with lower test scores. Second, we test for effect heterogeneity across grade levels and show that elementary and middle-school students similarly benefit from repeated student-teacher matches. Our finding that rematching positively affects student achievement in higher grades is notable because policies designed to increase student-teacher rematches are

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[1] Although looping is uncommon in the United States, in many educational programs around the globe—such as Montessori schools in Italy and the Waldorf School in Germany—teachers are assigned to the same groups of students for multiple years (Hitz et al., 2007). Our focus on student-teacher rematching captures what is arguably the most important looping component. It is also more informative for practitioners because rematching is easier to implement than looping, especially in higher grades.

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Table 1
Student characteristics with and without consecutive student-teacher rematches.

	All students (*N*=903,738)	Never rematched (*N*=820,759)	Rematched (*N*=82,979)	Mean difference
Female	0.492	0.493	0.480	0.013***
Black	0.119	0.121	0.102	0.019***
Hispanic	0.109	0.109	0.105	0.004***
White	0.703	0.700	0.725	-0.025***
Other race/ethnicity	0.069	0.069	0.067	0.002
FRL	0.495	0.498	0.473	0.025***
ELL	0.065	0.065	0.067	-0.002*
IEP	0.136	0.130	0.196	-0.066***
ELA achievement	-0.040	-0.041	-0.037	-0.004
Math achievement	-0.031	-0.033	-0.007	-0.025***

Note. The data include students in grades 3-8 between the 2011-12 and 2016-17 school years in Indiana. Rematches are based on ELA teachers. ELL indicates English Language Learners. FRL indicates students who are eligible for free or reduced-price lunch. IEP indicates students with individualized education program status. ELA indicates English Language Arts.

Table 2
Teacher characteristics with and without consecutive student-teacher rematches.

	All Teachers (*N*=31,235)	Never Rematched (*N*=24,299)	Rematched (*N*=6,936)	Mean Difference
Female	0.854	0.845	0.886	-0.041*
Black	0.045	0.047	0.039	0.008**
Hispanic	0.012	0.012	0.013	-0.0005
White	0.934	0.931	0.941	-0.010**
Other race/ethnicity	0.009	0.010	0.007	0.003**
Years of teaching	11.749	11.453	12.783	-1.139***
Master's degree	0.436	0.426	0.473	-0.047***

Note. The data include teachers in grades 3-8 between the 2011–12 and 2016–17 school years in Indiana. Rematches are based on ELA teachers. ELL indicates students who are English Language Learners. FRL indicates students who are eligible for free or reduced-price lunch. ELA indicates English Language Arts.

less-costly to implement—and thus more feasible—in higher grades.

2. Background

2.1. Student-teacher familiarity

Increased student-teacher familiarity offers several potential benefits for student development (Gehlbach et al., 2016). Teachers with better knowledge of students' academic histories and abilities can assess their students' needs more accurately and personalize instruction more effectively (McCown & Sherman, 2002). Increased familiarity also allows teachers to make stronger connections with students' families and promote parental involvement, which can improve student performance (Hitz et al., 2007; Nichols & Nichols, 2003). For students, being familiar with teachers enables them to navigate teaching styles and encourages participation in classroom activities (Burke, 1996). When students have stronger relationships with teachers, they exhibit enhanced motivation and are more likely to feel safe (Meyer & Turner, 2002; Rasmussen, 1998).

There are reasons to expect the effects of student-teacher familiarity may be especially important for historically underserved students, for whom positive relationships may help offset inequities (Decker et al., 2007; Eccles & Roeser, 2011; Pianta et al., 1995). Similarly, English language learners may also benefit disproportionately from strong relationships with their teachers, which may help to reduce linguistic and cultural barriers they face in the classroom (Lucas et al., 2008; Master et al., 2016; Yoon, 2008).

Another dimension of potential heterogeneity is student age. Students have weaker relationships with adults and stronger relationships with their peers as they age (Lynch & Cicchetti 1997), which suggests student-teacher familiarity may be more important in earlier grades. However, research also suggests older students could benefit from stronger student-teacher relationships (Duong et al., 2019; Longobardi et al., 2016); in the face of generally weakening relationships with adults, older students may stand to gain more at the margin from being closer with their teachers. From a policy perspective, understanding any effect heterogeneity of rematching as students age is useful because school staffing strategies differ across schooling levels. Given that subject-area teacher specialization in secondary schools is prevalent, efforts to rematch students to the same teachers across years (and

Table 3
Student-teacher rematches and student achievement.

	Math				ELA			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rematched	0.038	0.020***	0.014***	0.014***	0.027	0.028***	0.015***	0.015***
	(0.020)	(0.005)	(0.004)	(0.004)	(0.021)	(0.004)	(0.003)	(0.003)
Grade and Year FE	X	X	X	X	X	X	X	X
Student FE		X	X	X		X	X	X
Teacher FE			X	X			X	X
Student, Teacher, and School Time-Varying Controls				X				X
N	2416995	2416995	2416995	2416995	2410195	2410195	2410195	2410195

Note. FE=fixed effects. Student FE models also control for time-variant student level variables, including free or reduced-price lunch eligibility, English language learner status, and IEP status. ELA models include rematches with ELA teachers and ELA teacher characteristics, and math models include rematches with math teachers and math teacher characteristics. Standard errors in parantheses are clustered at the school level. * $p < 0.05$
** $p < 0.01$
*** $p < 0.001$

Table 4
Student-teacher rematches and academic achievement across subgroups.

	Math (1)	(2)	(3)	(4)	(5)	(6)	ELA (7)	(8)	(9)	(10)	(11)	(12)
Rematched	0.007	0.010***	0.011***	0.014***	-0.011	-0.011	0.010***	0.016***	0.013***	0.014***	-0.009	-0.007
	(0.004)	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.004)	(0.004)	(0.003)	(0.003)	(0.006)	(0.007)
Rematches * Black, Hispanic, and others (ref. White)	0.025***					0.015	0.019**					0.001
	(0.007)					(0.008)	(0.006)					(0.007)
Rematched * FRL		0.008				-0.008		0.007				-0.014*
		(0.005)				(0.005)		(0.005)				(0.006)
Rematched * ELL			0.043***			0.018			0.045***			0.038**
			(0.010)			(0.012)			(0.010)			(0.012)
Rematched * IEP				-0.002		-0.023*				0.023**		0.007
				(0.008)		(0.009)				(0.007)		(0.009)
Rematched * prior achievement (bottom tercile)					0.039***	0.043***					0.043***	0.044***
					(0.008)	(0.008)					(0.008)	(0.009)
Rematched * prior achievement (middle tercile)					0.025***	0.026***					0.024***	0.025***
					(0.007)	(0.007)					(0.007)	(0.007)
Grade and Year FE	X	X	X	X	X	X	X	X	X	X	X	X
Student FE	X	X	X	X	X	X	X	X	X	X	X	X
Teacher FE	X	X	X	X	X	X	X	X	X	X	X	X
Student, Teacher, and School Time-Varying Controls	X	X	X	X	X	X	X	X	X	X	X	X
N	2416995	2416995	2416995	2416995	1801962	1801962	2410195	2410195	2410195	2410195	1795619	1795619

Note. ELA models include rematches with ELA teachers and ELA teacher characteristics, and math models include rematches with math teachers and math teacher characteristics. ELL= English language learners. FRL= free or reduced-price-lunch eligibility. All models include student, teacher, grade, and year fixed effects, as well as student level controls (i.e., free or reduced-price lunch eligibility, English language learner, and IEP status), teacher level controls (teaching experience, graduate degree, class size) and school level controls (i.e., percent Black and Hispanic students, percent of free or reduced-price lunch eligibility, school enrollment, and school level achievement). Standard errors in parantheses are clustered at the school level. * $p < 0.05$
** $p < 0.01$
*** $p < 0.001$.

Table 5
The effects of student-teacher rematches by school level.

	Math	ELA
Rematched	0.016***	0.017***
	(0.005)	(0.004)
Rematched * middle school	-0.003	0.003
	(0.008)	(0.006)
Grade and Year FE	X	X
Student FE	X	X
Teacher FE	X	X
Student, Teacher, and School Time-Varying Controls	X	X
N	2416995	2410195

Note. ELA models include rematches with ELA teachers and ELA teacher characteristics, and math models include rematches with math teachers and math teacher characteristics. All models include student, teacher, grade, and year fixed effects, as well as student level controls (i.e., free or reduced-price lunch eligibility, English language learner, and IEP status), teacher level controls (teaching experience, graduate degree, class size) and school level controls (i.e., percent Black and Hispanic students, percent of free or reduced-price lunch eligibility, school enrollment, and school level achievement). Standard errors in parantheses are clustered at the school level. * $p < 0.05$
** $p < 0.01$
*** $p < 0.001$.

grades) will be less disruptive to staffing policies in secondary schools than in elementary schools.

2.2. Prior research on student-teacher rematching

The empirical literature on the effects of student-teacher rematching is thin. Two observational studies of deliberate looping strategies find that looping is associated with higher test scores, better attendance, and less grade retention (Cistone & Shneyderman, 2004; Franz et al., 2010). However, while suggestive, these studies are based on small samples and do not use causal research designs, raising questions as to whether the associations can be attributed to the repeated student-teacher matching itself or a correlated factor.

Hill & Jones (2018) provide the only plausibly causal estimates of rematching effects in the literature. These authors estimate rematching effects using administrative data from North Carolina covering students in grades 3-5 and find that repeated student-teacher assignments increase student achievement. We follow the same methodology as Hill & Jones (2018) and expand on their study in three ways. First, we replicate their analysis using data from a different state. Second, we conduct a more expansive analysis of rematching effect heterogeneity by student characteristics. Specifically, whereas Hill and Jones (2018) test for effect heterogeneity along the dimension of race/ethnicity alone, we additionally test for effect heterogeneity among student subgroups defined by FRL-eligibility status, ELL status, IEP status, and prior achievement status. Finally, we expand on Hill and Jones's (2018) examination of elementary students by estimating rematching effects through the 8th grade.

3. Data and sample

Our analysis is based on statewide administrative data from the Indiana Department of Education (IDOE) covering students and teachers in grades 3-8 from the 2010–2011 to 2016–2017 school years. The data include student demographics and standardized math and reading scores (i.e., Indiana Statewide Testing for Educational Progress-Plus (ISTEP+)) as well as teacher demographics and qualifications. Student test scores are standardized by subject-grade-year to have a mean of zero and standard deviation of one. Our data allow us to identify matches between students and teachers at the classroom level.

We define a rematch as occurring whenever a student and teacher paired in year t were also paired in t-1 in the same subject (math or ELA). Although our full panel covers the years 2010–11 to 2016–17, it is not possible to identify rematching events during the first year of our data panel in 2010–11 because the prior year is unobserved. As a result, we use 2010–11 data only to identify whether rematches occurred in 2011–12 and conduct our analysis of rematching effects over the six-year period from 2011–12 to 2016–17.[2]

Our dataset includes 903,738 unique students and 31,235 unique math and ELA teachers in grades 3-8. Tables 1 and 2 provide summary statistics for students and teachers, respectively. The samples in each table are further divided by whether students and teachers ever experience a rematch. Rematches between students and teachers are relatively uncommon: the annual rematch rates for students are 4.2% in math and 3.5% in ELA. That said, 9.2% of students and 22.2% of teachers experience at least one rematching event over the full data panel.

Table 1 shows that the likelihood of experiencing a rematch is correlated with most of the observed student characteristics in our dataset. Students are more likely to experience a rematch if they are male, White, ineligible for FRL, and higher-achieving. Students are also more likely to rematch if they have an IEP—this may reflect a concerted effort to rematch IEP students, or it could be a byproduct of teacher availability (e.g., there may be just a small number of teachers with IEP assignments and/or IEP certifications within a school). Table 2 shows that observable teacher characteristics are also associated with rematches. Rematched teachers are more likely to be female, White, more experienced, and to have graduate degrees compared to their peers who do not experience a rematch during the panel period. The descriptive differences in Tables 1 and 2 make clear that simple comparisons of outcomes between students by rematching status are insufficient to assess the causal impacts of rematching.

4. Empirical strategy

Our empirical strategy leverages the Indiana data panel to uncover plausibly causal estimates of the impacts of rematches on student outcomes. The key components of our model are student and teacher fixed effects. With the inclusion of these fixed effects, we identify the effects of rematching using within-student and within-teacher variation in the occurrence of rematching events. Our identification strategy mitigates many of the concerns raised about selection into rematching—from both the student and teacher sides—by the descriptive statistics reported in Tables 1 and 2.

Our preferred specification is as follows:

$$Y_{ijsgt} = Rematch_{ijt}\theta + X_{it}\beta_1 + T_{jt}\beta_2 + S_{st}\beta_3 + \pi_i + \delta_j + \gamma_g + \alpha_t + \varepsilon_{ijsgt} \qquad (1)$$

In Eq. (1), Y_{ijsgt} denotes an outcome (either math and ELA achievement) for student i assigned to teacher j in school s, and in grade g in year t. $Rematch_{ijt}$ is an indicator set to one if the student and teacher are rematched from the previous year—i.e., if it is the second consecutive year that they are paired. X_{it} is a vector of time-varying student controls including FRL status, ELL status, and IEP status; T_{jt} is a vector of time-varying teacher characteristics including experience and an indicator for whether the teacher has a graduate degree; and S_{st} is a vector of time-varying school characteristics including the average achievement level of the school, school enrollment, and the fraction of the student population that is Black, Hispanic, and FRL-eligible. π_i is a student fixed effect, δ_j a teacher fixed effect, γ_g a grade fixed effect, and α_t a year fixed effect. ε_{ijsgt} is the idiosyncratic error term. We cluster our standard errors at the school level throughout to allow for within-school data dependence.[3]

As noted earlier, the key components of the model that permit plausibly causal identification are the student and teacher fixed effects. With these fixed effects, the identifying assumption is that student and teacher rematching events are exogenous conditional on time-invariant student and teacher attributes. This identifying assumption would allow, for example, students from vulnerable populations to be systematically less likely to experience a rematch (as indicated by Table 1), as long as selection is based on time-invariant information. The assumption would be violated if principals assign students or teachers to rematches using dynamic information not captured by the student and teacher fixed effects.

In addition to the student and teacher fixed effects, the model also includes grade and year fixed effects along with the vectors X_{it}, T_{jt}, and S_{st} to capture the influence of any time-varying factors associated with students, teachers, and schools. Noting that most of the important variation along these dimensions should be absorbed by the student and teacher fixed effects, our findings should not be meaningfully sensitive to whether the vectors of control variables are included in the model or not, which below we show to be the case. The parameter of interest in Eq. (1) is θ, which under the identifying assumptions of the model can be interpreted as the causal impact of rematching on student outcomes.

We expand the model to test for effect heterogeneity among students who differ by racial/ethnic minority status, FRL status, ELL status, IEP status, and tercile of lagged student achievement. The heterogeneity tests are facilitated by adding interaction terms to Eq. (1) between the $Rematch_{ijt}$ indicator and these student characteristics. Using the same strategy, we test for effect heterogeneity in elementary versus middle-school grades (grades 3-5 versus grades 6-8).

We also provide supporting evidence of our empirical strategy in the form of a placebo test. In the placebo test, we ask whether the *first year* of what will ultimately become a rematching event affects student achievement. If the rematching effect is attributable to increased student-teacher familiarity, and not other aspects of the student-teacher pairing (such as uncontrolled sorting bias), then the effect should be concentrated in the second year only. To implement the placebo test, we simultaneously estimate the effects of the first and second years of observed rematching events in Eq. (1).[4]

5. Results

Table 3 shows our estimates of the average rematching effect on student achievement in math and ELA using our primary specification in Eq. (1), as well as several sparser variants leading up to the full

[2] We focus on consecutive-year rematches because they are the predominant and most policy-relevant type of rematch. The other possibility is a "gap year" rematch—e.g., a student-teacher pairing in year t-2 and t, with a gap in the pairing during year t-1. With regard to prevalence in observed data, in an analysis omitted for brevity we find that of total consecutive-year and gap-year rematches, the latter account for fewer than one in five rematches (i.e., the rate of gap-year matches is about 0.7% annually). With regard to policy relevance, we view policy adjustments to influence the rate of consecutive-year rematches in schools as more intuitive and less likely to be disruptive to staffing than adjustments to influence the rate of gap-year rematches.

[3] Our findings are very similar if we cluster at the teacher or classroom level instead. We prefer school-level clustering conceptually because the rematching treatment is clustered to some degree by school (i.e., some schools have higher rematching rates than others). In addition, school-level clustering is more conservative than the other options.

[4] That is, in Eq. (1), we replace the "$Rematch_{ijt}$" variable with two variables—one for the first year of the rematch (previously unmodeled) and the other for the second year of the rematch. This placebo test is similar to a related placebo test conducted by Hill and Jones (2018). With student and teacher fixed effects both included in the model, the comparison group is described as follows: it consists of students who are observed rematching to teachers in other years, but in a year when the students do not have a teacher they ever rematched with, *and* who are taught by a teacher in that year who rematches with other students, but not this particular student. In simpler terms, the comparison group includes a subset of single-year student-teacher pairings in the data. The subset is a somewhat narrow group, but due to our large data panel, many such instances are observed and both rematching parameters are identified with sufficient precision (see Appendix Table 1).

specification. Our preferred estimates in columns 4 and 8 indicate that rematching causes increases in achievement in math and ELA of 0.014 and 0.015 standard deviations of student test scores, respectively, on average. These estimates are similar in sign and magnitude to comparable estimates from Hill and Jones (2018).[5] While substantively small, the coefficients in both subjects are estimated precisely and clearly distinguishable from zero.

The pattern of estimates in Table 3 reflects the descriptive evidence above showing that rematched students and teachers are positively selected. Comparing the results from the regressions that include just grade and year fixed effects (columns 1 and 5) with the specifications that include student and teacher fixed effects (columns 3 and 7) confirms the positive selection. That is, consistent with the expectation that the rematching coefficients from the sparse models are positively biased, adding the student and teacher fixed effects reduces the magnitude of the positive coefficients on the rematching variables substantially. Moreover, as anticipated, once the teacher and student fixed effects are included in the model, adding the additional time-varying controls for students, teachers, and schools (in columns 4 and 8) does not affect our findings.[6]

The results from our placebo regressions are reported in Appendix Table 1. When we estimate separate parameters for the first and second years of rematching events, the positive effects of the pairings are concentrated in the second year and the first-year coefficients are statistically indistinguishable from zero. This result indicates that our findings are not biased by selection into rematched student-teacher pairings and is consistent with the interpretation that our main estimates in Table 3 capture the causal impacts of participating in the second year of a rematch.[7]

Next, we turn to our heterogeneity analyses by student characteristics and grade levels. First, Table 4 shows differential rematching effects by student subgroup. We find the positive effect of rematching on math achievement is larger for racial/ethnic minority students relative to White students (by 0.025 standard deviations), English language learners relative to native speakers (by 0.043 standard deviations), and bottom-tercile and middle-tercile achieving students relative to top-tercile achieving students (by 0.039 and 0.025 standard deviations, respectively). We find no statistical evidence that rematching effects in math differ for students by FRL or IEP status (Columns 2 and 4, respectively). Our findings in ELA are generally similar, except we estimate a positive rematching effect for IEP relative to non-IEP students of 0.023 standard deviations.

When we add interactions with all student characteristics simultaneously in columns 6 and 12 of Table 4, we find that the weight loads primarily on the student achievement terciles. This indicates that prior achievement is the key factor driving the effect heterogeneity in the table. This result does not change the meaning of the individual coefficients in columns 1-5 and 7-11—i.e., students with the identified characteristics are still differentially affected by rematching as indicated by the coefficients—it just highlights that the disproportionate benefit for lower-achieving students is the primary pathway through which the effect heterogeneity is operating.

In Table 5, we test for effect heterogeneity across elementary and middle-school grades. We find no evidence of effect heterogeneity—that is, we cannot reject the null hypothesis that the positive effect of rematching is the same in elementary and middle-school grades. As mentioned above, the finding that positive rematching effects exist at least through the 8th grade is important given that the structure of teacher staffing in higher grades makes it easier to rematch students and teachers. Our "business as usual" data bear this out: the annual rematching rate is much higher in middle-school grades (grades 6-8: 5.1% annually) than in elementary grades (grades 3–5: 3.2% annually).[8]

6. Discussion and Conclusion

We use an administrative data panel covering students and teachers in grades 3-8 in Indiana to estimate the effects of student-teacher rematching on student achievement. We find that rematching students to teachers for a second consecutive year increases achievement by 0.014 and 0.015 standard deviations in math and ELA, respectively. Our identification strategy relies on student and teacher fixed effects to control for selection into rematched student-teacher pairs and permits plausibly causal interpretation of our estimates. A placebo test in which we estimate the average effect of the first year of rematched pairs uncovers no evidence of sorting bias.

Our findings corroborate similar findings from Hill and Jones (2018), who show that rematching positively impacts math and ELA achievement for elementary students in North Carolina. We build on their work by replicating their main findings in another state and conducting a deeper analysis of effect heterogeneity. Our heterogeneity analysis shows that students from racial/ethnic minority groups, cultural and linguistic minority groups, and lower-achieving students gain the most from being rematched with their teachers in consecutive years. An implication is that policies that increase rates of student-teacher rematches will be equity-improving. We also estimate effect heterogeneity across grade levels and show that the positive effects of rematching persist through the 8th grade. This finding is useful for policy because it will be less disruptive to schools' existing operating procedures to increase rematching rates in later grades.

The most direct implication of our study is that all else equal, increasing student-teacher rematches will yield improvements in student achievement. That said, it is also important to acknowledge that the effect sizes we estimate, which are similar in magnitude to effect sizes estimated by Hill and Jones (2018), are small on average and the

[5] To be more precise, our estimate in math is slightly smaller than in Hill and Jones (2018), and in ELA it is slightly larger; but taken on the whole and accounting for statistical imprecision in both sets of estimates, our findings align very closely with Hill and Jones (2018).

[6] In results suppressed for brevity we also confirm our results are substantively similar using value-added models, in which we include lagged test scores as control variables and omit student fixed effects. The value-added approach requires a separate data build because the requirements are different (e.g., all students must have a lagged test score), but the estimated rematching effects are consistent with our main results.

[7] Our placebo test provides compelling evidence on the plausibility of the key identifying assumption. Although it does not allow us to rule out all sources of potential bias with certainty, the remaining pathways to bias conditional on our placebo results are complex and seem unlikely. For instance, a biasing threat we cannot rule out with certainty is a time-varying dynamic factor that is not present at the time of the initial match, thus missed by the placebo test, but that emerges during the first year of a student-teacher pairing. But even this is unlikely because our placebo results show that the first year of a rematch has no effect on the end-of-year test. This means the source of hypothetical dynamic sorting bias must be even more subtle—it must be the case that the unobserved dynamic factor increases the value of the subsequent rematch, but in a way that is not detectable on the end-of-year test after the first year. While it is not possible to rule out bias from this type of situation (or any situation) with certainty, we view it as unlikely.

[8] Additionally, in results suppressed for brevity we test for effect heterogeneity by teacher characteristics. We find no evidence of heterogenous rematching effects across teachers who differ by race-ethnicity, gender, education level, or experience. Our null findings are not too surprising because the teacher characteristics available in our data are limited and typically not strongly associated with other dimensions of teaching effectiveness, or teacher-effect heterogeneity, in prior research. Given the paucity of our teacher-characteristic data, we caution against overinterpreting our null results—future researchers with access to richer information about teachers (e.g., measures of teaching practice and teacher motivation level, etc.) may be able to conduct more interesting tests of rematching effect heterogeneity across teachers.

appropriate policy response is not obvious. It is worthwhile to pursue low-cost and non-disruptive policies that facilitate more student-teacher rematching, but whether more costly and disruptive changes should be pursued is less certain because efforts to increase rematching will come with tradeoffs. On the one hand, shifting teachers from being responsible for instruction in a single grade to multiple grades will require more time and effort devoted to preparation for teachers (Little & Dacus, 1999) and could lead to increased attrition, especially among novices (Blazar, 2015). Moreover, research shows that specialized experience (i.e., grade-specific) has higher returns than general experience for teachers (Blazar, 2015; Ost, 2014; Ost & Schiman, 2015), suggesting a cost associated with spreading teachers across grades more thinly. On the other hand, teachers may appreciate the reduced burden of becoming familiar with entirely new groups of students and parents each year. It is also possible that rematching has other benefits outside of the achievement effects we document here (e.g., on non-cognitive outcomes).

We conclude with a brief note on how our analysis informs the larger question of the importance of student-teacher familiarity. The rematching events we study can be viewed as creating observable, extensive-margin increases in student-teacher interactions, which correspondingly increase familiarity. But it is important to recognize that differences in student-teacher familiarity across classrooms in K-12 schools are likely driven by a myriad of factors in addition to repeated student-teacher matches. For instance, differences in class size may affect the ability of students and teachers to forge relationships; and indeed, a lack of familiarity has been postulated as an explanation for why efforts to scale up student exposure to specialized teachers do not generate the expected achievement gains (Fryer, 2018; Hwang & Kisida). Similarly, differences in individual teachers' abilities to develop strong relationships with their students, all else equal, may account for some of the widely-documented unexplained variance in teacher quality (Koedel, Mihaly, & Rockoff, 2015). Our study underscores the importance of student-teacher familiarity in student learning and provides broad support for policies that strengthen the relationships between students and teachers, particularly for students from historically underserved populations.

Author Statement

We have no competing interests to declare.

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Appendix

Appendix Table A1

Table A1
Placebo test results.

	Math		ELA	
	(1)	(2)	(3)	(4)
Rematch, Year-2	0.014***	0.011**	0.015***	0.013***
	(0.004)	(0.004)	(0.003)	(0.003)
Rematch, Year-1		-0.007		-0.006
		(0.004)		(0.004)
N	2416995	2416995	2410195	2410195

Notes: Columns 1 and 3 replicate the findings from our full specification in Table 3 (columns 4 and 8 of that table). Columns 2 and 4 show results from placebo tests using the same model structure, for which the regressions include an indicator for the first year of rematched pairs.

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