

# Research Module papers

Winter 2019

These notes will span the collection of papers focused on human capital, inter-generational mobility and other papers confined to the Research Module. Each paper will include a **Quote, Argument, Question, Reflection** (QAQR) report, a **dense summary** and **notes** on the respective paper. The QAQR report follows the format defined in the research module github page. The dense summary contains what I consider key parts of the paper. Notes contain substantive remarks on the key parts of the paper. In blue are lightning talk papers.

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# 1 Life Cycle Earnings, Education Premiums and Internal Rates of Return<sup>1</sup>

## 1.1 QAQR

To be written.

## 1.2 Dense Summary

- **Question:** Tackles three fundamental questions in economics:
  1. What do the education premiums look like over the life cycle?
  2. What is the impact on schooling on earnings over time?
  3. How does the internal rate of return (IRR) compare with the opportunity cost of funds? And does considering progressive taxes lessen the incentive to invest in education?
- **Data:** Only Males. 1943-1963 birth cohorts. Norwegian Panel Data 1967-2010 merged with 1960 census data. Public domain. Linking child to parent possible. Schooling measured until age 40. three measures: (1) Earnings over the life-cycle (discretized) (2) IRR (3) Lifetime Education Premium (mean lifetime earnings)
- **Methods:** OLS, IV
- **Identification Strategies**
  1. compulsory schooling reform as an instrument for education (it is exogenous, thus extremely useful BUT it just increases **required** schooling from 7 to 9 years)
  2. Controlling of course for ability test scores to account for selection bias ('more able' students will get more schooling).
  3. Within-twin-pair estimation (to control for environment/genetics)
- **Conclusion:** Additional Schooling gives higher lifetime earnings and steeper age-earnings profile. IRR around 10% after considering taxes and earnings related to entitlements.

## 1.3 Notes

- **Mincer Regression:** Assumes *exogenous* variation in schooling, quadratic potential experience in explaining log earnings. Mincer also assumes college students do not have any earnings and post-schooling employment is exogenous. (both relaxed here to compare estimates and make bounds).
- **Education Premium:** Interpreting the Mincer regression as the price to pay for a laborer given a certain education level, we can interpret the schooling's coefficient as the *education premium*. With respect to the paper, this is done at each age. They consider the education premium's mean and present value. The IRR is rate  $\rho$  which satisfies the typical PV problem:

$$\sum_{a=17}^{62} \frac{\beta_a}{(1+\rho)^{a-16}} = 0$$

They compare this rate to the real-interest rate (proxy for opportunity cost). Thus, trying to answer the question: Is it more profitable to invest in education or invest in the market at the inflation-adjusted rate? Furthermore, they use the **ordinary annuity** expression to compute the lifetime education premium. The **average real interest rate of 2.3%** was used for 1967-2010 (should another rate be considered?)

- **First Identification Strategy - IV:** Since schooling is endogenous, we seek to extract only the exogenous portion from the first stage regression. They instrument using a boolean variable for compulsory schooling. Estimation results from equation (7) show a strong first stage with an estimated coefficient on the instrument of 0.213. This means that exposure to the compulsory schooling reform increased years of schooling by about one-fifth of a year. The F-statistic for the instrument is around 93, implying that weak instrument bias is not a concern for our analysis.
- **Second Identification Strategy - Controlling for ability:** The second method controls for ability to mitigate selection bias. **The exam:** The arithmetic test mirrors the test in the Wechsler Adult Intelligence Scale (WAIS); the word test is similar to the vocabulary test in WAIS; and the figures test is comparable to the Raven Progressive Matrix test. They use **test score indicators**. Problem: only those that tried for the military took the exam (for 1950 or later).
- **Third Identification Strategy - Within-twin-pair estimation:** To control for environment and genetics - used as a fixed effect.
- **Unbalanced Panel:** When the number of time periods are *not* the same for all individuals. They wish to study those born in 1943-1963 during the period 1967-2010 for ages 17 to 62. Problem:
  1. The person born in 1963 at most will be 47, thus we don't know what happens between 48-62. A difference exists if the cohort was born after 1949. A robustness check (using a balanced panel) is done to reassure the results.
- **Table 2 and Table 3:** The main results show the following:
  1. IRR robust to the specification. Between 10%-14%
  2. Returns to education negative for 17-24 year olds (since they are giving up earnings compared to those going straight to work). Returns increase (still negative) when considering those in the workforce having to pay taxes on their wages.
  3. For ages 25-44, positive, but again for the IV estimate, really noisy (standard errors 15 times larger than that of the twins estimation). This is typically a sign of weak instruments, but the F-stat, t-stat and the fact that the instrument is exogenous helps.
  4. For ages 45-62, IV returns no difference and again high standard errors.
  5. All estimates range between 2% and 9% of the mean.
  6. **Table 3** shows that the IRR results are robust after considering taxes and pension income considering all ages. Looking at Figure 5, we can see that taxes decrease the negative effect of going to college since those who did not go to college and chose to work, must pay taxes on their earnings. The pension entitlements were estimated based on earnings information.
  7. As part of the sensitivity analysis, they had to make sure that cohort trends in earnings were unrelated to the reform. In other words, that nothing else made this earnings 'jump'. This is proven by creating a specification where they use interaction terms for years and municipality characteristics (plus the fixed effects) to explain the timing of the reform (e.g. poor municipalities would be the first to implement the reform. No evidence of such a relationship was found.
  8. Controlling for pre-reform trends, the IRR stays sizeable.
  9. In considering different ways in treating the missing values, whether by imputing zeros or past earnings, results are robust and considering a balanced panel also rendered consistent results.
  10. Mincer Regression estimates are lower because of the restriction of no earnings while in school and exogenous post schooling employment accounts also for the downward bias.

## 2 Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital<sup>2</sup>

### 2.1 QAQR

To be written.

### 2.2 Dense Summary

- **Question:** What goes into the composition of the variation when trying to explain the relationship between the education levels parents attain compared to their children's education levels? Is it mostly family characteristics and inherited ability (selection theory explanation) or education spillovers (being educated has implications on parenting and leads to your children having more educational outcomes)?
- **Significance:** If it is the case that education spillovers exist, education policy seems to be the recourse. Thus, this paper proposes to provide evidence on the *causation* argument and relieve some questions on the intergenerational transmission of education.
- **Data:** Norwegian data and children data from 2000 aged 20-35. Census data from 1960, 1970, 1980. Educational attainment from Statistics Norway. Cohorts of parents born between 1947 and 1958. Analysis mostly focused on parents with less than 9 years of schooling.
- **Methods:** OLS, IV
- **Identification Strategies:**
  1. They instrument for *parental* education using the compulsory schooling reform (1960s Norway) since it is exogenous to parental ability.
  2. They make two assumptions:
    - (a) That individuals who get 9 years of education after the reform would have gotten less than or equal to 9 before the reform.
    - (b) Those who got less than 9 years of education before the reform, get 9 years of education after the reform.
- **Conclusion:** There is little evidence of a causal relationship between the father's education and the children's education. A mother and son comparison generates a weakly significant result, but no causal relationship between mother's and daughter's educations.

### 2.3 Notes

- They checked for significant determinants with respect to the timing of the reform (as did Paper 1) such as average earnings of the people, taxable income, size of the municipality, unemployment rates, urbanization, labor force composition, and education levels and found no relationship.
- Their estimation uses parental education, children age, sex, and fixed effects for parents' age and parents' municipality. The fixed effect for parents' age is their to control for secular (persisting nearly indefinitely) changes in educational attainment over time that may be completely unrelated to the reform. The municipality effects control for variation in the timing of the reform across municipalities that may not have been exogenous to parents' educational choice like them seeking higher education because their municipality required highly skilled workers. As long as we fix these characteristics, make sure they are not correlated with the timing of the reform nor the schooling of the children of this generation.
- **Small things to be aware of**

1. They assume in cases of one-parent households that the child lived with the mom. This comes up because the mother's parent place of residence can be found in the 1960 census. Thus, they make the 'safe' assumption that the child lived there too (this is of course relative to the epoch).
  2. **Selective Migration:** There could be cases where the parents anticipated a change in another municipality and moved there. This affects the estimates, but not the consistency - thus with large enough sample size, the estimate is fine.
- Looking at the distribution of parents and their education levels plus minus 2 years from the reform shows that the *cumulative* distribution of people getting  $\leq 9$  years of education is constant. Thus, the reform itself does not have spillover effects.
  - Looking at family characteristics, there is no evidence of compositional change.
  - **Results**
    1. OLS - positive and significant results (5% level) for discrete and Boolean education DV.
    2. 2SLS - insignificant, large s.e.'s, but this is due to the focus on all parents and not those in the bottom of the distribution where the reform has 'more bite'. The instrument is weak and the t-stat, although showing variation exists between the reform and parental education, is mostly minor at the higher education levels.
    3. Focusing on the sample of parents who received less than 10 years of education, 2SLS shows mother's education has an impact over on child's education level and son. Standard errors decreased after subsetting, thus the estimate is more precise.
    4. Overall, results indicate that the positive correlation between parent and child and their education levels mostly comes from other positively correlated factors with education like ability, family background, income or other factors. The causal effect is weak.
    5. In the literature, high IV estimates are rationalized by heterogeneous returns to education (a particular group whose behavior is impacted by the instrument i.e. child goes to school longer).
    6. To control for the fact that educated women marry educated men, they control for father's education and thus, both parents, in explaining child's educational attainment. They also only include cases where at least 1 parents attained less than 10 years of education. IV estimates still weak.
  - **Robustness/Specification Checks:** (1) Municipality-specific time trends, county cohort fixed effects (2) In case they were inaccurate in selecting the reform year, they removed data for that year, and the years preceding it and following it. (3) Sample selection from 20-35 to 25-35 years old. The initial reason for 20-35 comes from the fact that less educated parents have less educated children. Thus, since the reform hits the lower distribution, the children were expected to be finished by 20 years of age. To be sure they are finished, they consider 25-35 years. Still, significant, positive result for mother/son, but nothing else. (4) Subsetting to the highly educated rendered nothing. (5) Conditional on human capital, high able women are less likely to have children. Thus, when the reform hits, some women will experience an increase in human capital. These women will not have children and the group of women left hit by the reform having children have **less ability** than those mothers not hit by the reform. To check whether the reform increases one's probability to have children, they run a linear probability model with the DV equal to 1 if parent has child and 0 if not. Not significant; but, could also use probit/logit. (6) Sibling fixed effects. This is supposed to capture some unobserved ability. In the first stage regression, if unobserved ability is positively correlated with parental education, then of course the OLS estimates will decrease with including the covariate. First, they consider the sample of parents who have brothers or sisters and seeing how they impact the same-sex child. The results, as hypothesized, reduce the estimates in the OLS regression; implying the OLS estimates are upward biased by unobserved ability.

- **Exploring the mechanisms for explaining why mother’s education affects child’s education:** (1) Children tend to spend more time with their mother. Naturally, extra schooling for the mothers increases their own human capital, thus their optimal human capital *choice* of children. (2) To see if there exists a relationship between mother’s education and father’s education (testing the hypothesis that higher educated women marry highly educated men), they ran a regression using the reform as an instrument for mother’s education and again, nothing. This implies that the greater education induced by the reform did not have any major effect on the type of father chosen by the mother. (8) To test whether women with more education have less children and invest more in each of them, testing the quality/quantity trade off, they see whether increasing mother’s education has an effect on the number of children they have. There is a lack of evidence on this (except for mother’s on the lower end of the educational attainment distribution)

### 3 Estimating ATE/LATE of Education when Compulsory Schooling Laws Really Matter

#### Summary by Florian Schoner<sup>4</sup>

#### 3.1 QAQR

To be written.

#### 3.2 Dense Summary

- Analyzes the impact of the increase of the minimum school-leaving age from 14 to 15 in the UK and Northern Ireland on subsequent earnings in the labor market.
- **Specialty 1)**
  - Prior to the reform, 60% of pupils left school at 14 and this share decreased to 10% after the reform. Therefore, half the population of 14-year-olds are compliers in the sense that they take up the treatment (an additional year of schooling) because they were exposed to the instrument (the minimum school-leaving age is 15).
  - In contrast to previous studies, the group of compliers therefore is a majority of the whole population, hence the LATE should be closer to the ATE (In fact, the author argues that  $LATE \approx ATE$ ).
- **Specialty 2)**
  - Achieves relatively clean identification by using a (fuzzy) RDD
- Previous studies based on IV-estimates of the causal effect of education suffered from the caveat that identification was driven by a very particular group.  
For instance, proximity to college has been used to estimate the LATE in the US. However, that dummy was equal to 1 for only 10% of the population, of which only a fraction *actually* went to college. The literature was concerned whether these particular groups of compliers may exhibit larger effects than the population at large because those individuals are more credit constrained, have a greater distaste for school, or a greater immediate need to work.
- This belief is driven by the fact that IV estimates usually exceed those from OLS-specifications in the literature. This is surprising since we would usually assume that, due to omitted variable bias, OLS *overestimates* the causal effect of schooling. Therefore one potential explanation was that would-be dropouts must have higher gains from additional year of schooling, thus leading to  $LATE > ATE$

- **Findings**

- Finds increase in earnings of around **15 - 17%** due to the additional year of schooling.
- In a corrigendum published by Oreopoulos/the AEA in 2008, revised estimates are around **7 - 13%**, and some of them were rendered statistically insignificant
- Further caveat: Used the wrong SEs for a RDD with a discrete running variable. Using the correct ones renders even more of the estimated effects statistically insignificant.

- **Method** Uses RDD for identification.

- First Stage: Regress average age of leaving full time education on the year aged 14 (from 1947 on, the reform was in place) and polynomials of it and a dummy for 1947 or later. (T on Z)

$$AGELEAVE_i = \gamma_0 + \tau_{FS} \cdot I(MINAGE_i = 15) + \sum_{j=1}^4 \gamma_j \cdot YEAR14_i^j \quad (1)$$

- Second Stage: Regress  $\ln(EARN_i)$  on polynomials of year aged 14, and instrument the indicator: and indicator whether year aged 14 was greater or equal than 14. (Y on Z)

$$\ln(EARN_i) = \beta_0 + \tau_{SS} \cdot I(MINAGE_i = 15) + \sum_{j=1}^4 \beta_j \cdot YEAR14_i^j \quad (2)$$

- then divide  $\hat{\tau}_{SS}/\hat{\tau}_{FS}$

- **Conclusion**

- Since he finds similar LATEs in the UK, US, and Canada and the latter two were suspected to be higher than the respective ATEs, he concludes that the ATE of education is indeed high and that would-be dropouts do not gain more from additional education than the reference population.

## 4 How does your kindergarten classroom affect your earnings? Evidence from STAR<sup>3</sup>

### 4.1 QAQR

To be written.

### 4.2 Dense Summary

- **Questions:**

1. What are the long-run outcomes from Project STAR?
2. What are the long-term impacts of early childhood education? But also rather, do the environments that raise test scores also translate to better adult outcomes (i.e. earnings, college attendance, marriage, home ownership, savings, etc)?
3. Although the effects of classroom attributes on test scores do not persist through education, why do they persist to adulthood?

- **Data:** Linking Project STAR data (student/teacher achievement ratio) and U.S. Tax data from 1996-2008 (1040 form, W-2, and 1098-T, the latter two being available from 1990), they were able to follow 95% of the STAR participants into adulthood.

- **Methods:** OLS, ANOVA
- **Identification Strategy:** Using school-by-entry-grade fixed effects, peer/teacher characteristics and the **Experimental Design** of STAR.
- **Conclusion:**
  1. Kindergarten test scores are highly correlated with (1) earnings at age 27 (2) college attendance (3) home ownership (4) retirement savings.
  2. *Experimental Impacts:*
    - Students in the small classes are significantly more likely to attend college and exhibit improvements in other outcomes.
    - Class size in general does not have a significant effect on earnings at age 27, but it is imprecisely estimated.
    - Having experienced teachers translates to higher earnings
    - Classroom effects (higher quality versus lower quality defined by student test scores) have higher earnings, college attendance rates, and other outcomes.
    - Classroom effects lessen on test scores in later grades (by grade 8, class quality in kindergarten holds no weight), but gains in non-cognitive measures persist.

### 4.3 Notes

- **Brief intro about STAR:** The Student/Teacher Achievement Ratio (STAR hereforth) experiment randomly assigned 11,571 students to different size classrooms (and different types/quality of classrooms) for grades K-3 for 79 schools in 1985-1989.
  - **Assignment:** Students were assigned to small classes (15 students on average), and large classes (22 students on average). Teachers were also randomly assigned (they may differ in quality, defined by years of experience) and by definition, so were your peers.
  - **Objective:** To see the causal effects of such assignment on test scores.
  - **Critique:**
    1. The schools were predominantly of lower socio-economic status compared to the rest of Tennessee and the rest of the U.S.
    2. Since kindergarten was not required, almost half the sample entered in 1st grade; not really an issue, but something to keep in mind. The paper uses school-by-entry-grade fixed effects to control for unobserved characteristics that may have risen from entering the school in different years.
    3. No record of the *location* of the classrooms, only classroom attributes. Details on the students' peers and teachers are also limited.
  - In cases where deviations occurred (e.g. moving from a small classroom to a large classroom), typical protocol was adopted (this sort of non-random sorting is treated by just granting treatment based on the **initial** random assignment, meaning there was an **intent to treat**.)
  - Score distributions of the students from large classes were made, then the scores from the students from the smaller classes were placed in the appropriate percentiles created by the large class distribution so see how treatment class fared in comparison.
- **Variables:** **Earnings** is in 2009 dollars, adjusting for inflation using the CPI. Earnings capped at 100k since less than 1% of the sample made over that much (done to increase precision) and also the earnings outcome was an **average** between 2005-2007 (also done to increase precision of the estimate). 1098-T forms indicate college enrollment. All universities, colleges and vocational institutions receiving federal aid must submit one per student (one student can have multiple forms



if they switch schools for example). They have payments made by the student (which is interesting because you can translate that to years of college completion based on the cost of the university). This was not done/considered.

- To measure **college quality**, they used an earnings-based approach, where they group students based on university and see what their average earnings are. This correlates .75 with the U.S. news ranking system, thus a decent measure.
- **Married** is an indicator for whether a person was *ever* married by age 27 and indicated if the taxes were filed *jointly*. **401k savings** is reported on the W-2. **Home ownership** can be found from the 1040 form, but mostly from the 1098 (evidence of mortgage payments). Naturally, this excludes people that own a home and make zero mortgage payments; but, the likelihood of owning a home before 27 is very small. **Cross-state mobility** indicated whether a STAR student ever lived *outside* Tennessee between age 19-27. **Neighborhood quality** is based on the percentage of college graduates from that zip-code in 2000. **Parents** are defined as those who claim a STAR student as a dependent from 1996-2008 (the likelihood of being a dependent to a non-biological guardian is small). By 1996, STAR students were all 16-17 years old, so still going to high school - 86% were able to be linked. **Parental Income** average of 1996-1998 adjusted gross income (for all observations, income zeroed if no taxes were filed).
- **Other Variables:** Age, gender, race, whether they receive free or reduced lunch, teacher experience, teacher highest degree, test scores from the *entry grade*, student outcomes, parent characteristics, classroom characteristics
- **Validity of Experimental Design: Do characteristics vary across small vs large class types and do they vary across classrooms within schools?** Causal inferences can be made due to the randomization of students into classrooms and no differences in attrition across classrooms. To *verify*, a **balance check** is done using *pre-determined variables*. For instance, before the experiment, we knew the student's age, race, whether they qualified for free lunch or not, and gender. For an experiment to be random, these variables should not impact the decision of classroom assignment. Furthermore, they used parental income, 401k savings, home ownership, marital status, and mother's age at child's birth as another balance check. The balance check is just to establish *randomization*, that allocation of the students to small or large classes or any other attribute did not arise from having a certain background (e.g. poor people were allocated to small classes) - that it is indeed **random**. This can be done by regressing an indicator for whether a student was in a small class on the set of variables above plus age, gender, whether they received free or reduced lunch, and race. No difference implies characteristics across both groups are no different. *Although* these new pre-determined variables are AFTER the experiment, the researchers believe these variables are unlikely to be impacted by the random assignment. **Table 2** shows the randomization tests and concludes the experiment was *indeed* random. Meaning, pre-determined variables showed no difference *across* classrooms. The last test used the predicted values of Column 1, Table 2 and regressed those values on kindergarten classroom indicators (including school FE). Results showed clustering of earnings (created using pre-determined variables) is non-existent by classroom indicators. Clustering here just means the values do not *shift* or cluster in space conditional on the treatment. Then, to see if those 95% of people linked to the tax data were matched non-randomly (e.g. rich people match more than poor people), they run a regression of being matched on whether you were assigned to a small class with and without a full-set of controls - no differences. Finally, to see if there is some variation across groups with respect to the death rate, they regressed the death rate on whether you were assigned to a small class or not - no differences.
- **How to Assess a Relationship:** When trying to make implications about the relationship between variables (in this case, test score percentiles and adult outcomes), descriptive statistics is a great benchmark assessment. Showing correlations and raw differences (without controls), strengthens your argument and also shows you are not tossing in controls for a significant result - that, the

*significant difference* was there from the beginning. Figure Ia plots the correlations and illustrates a summary index (some composite measure for home ownership, 401k savings, if ever married, ever lived outside Tennessee and neighborhood quality).

- **First Results: Regressing adult outcomes on scores**

- **Dependent Variables:** Earnings, College attendance in 2000, College attendance by 2007, college quality, and the summary index
- **Independent Variables:** Entry-grade percentile score, 8th grade percentile score, parental income percentile, class FE, race, gender, age, free lunch status, mother’s age at child birth, indicator for whether they have a 401k, home ownership and a quartic in parent’s household income interacted with an indicator for whether they were married between 1996 and 2008.
- See Table 4 for complete results. Considering the first 5 columns, wage earnings increase given a unit change in the kindergartener’s (KG) percentile score. When controlling for the 8th grade test score, this new score absorbs the entire effect of the kindergarten test score - still significant. Even more interesting is that parent’s income percentile is more indicative of a child’s earnings than test scores.
- For college and the summary index, a unit change in KG’s score translates to higher probabilities in attending college but also, better school quality. Summarized by the summary index of other adult outcomes, a greater KG score implies a better young adulthood.

- **Second Results: Regressing adult outcomes on class size**

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$$y_{icnw} = \alpha_{nw} + \beta SMALL_{cnw} + X_{icnw}\delta + \varepsilon_{icnw}$$

- We seek to explain earnings  $y$  of student  $i$  in classroom  $c$  of school  $n$ , for wave  $w$  (defined as when they entered the school).
- $\alpha_{nw}$  are the school-by-entry-grade FE
- $X_{icnw}$  is the vector of characteristics (parents’ and child’s).
- As we saw earlier,  $X_{icnw}$  is balanced across classrooms, so it should not impact the class size estimate,  $\beta$ .
- Standard errors are done at the school level, but also done at the classroom level for robustness.
- result should be interpreted as the effects of attending a class that is 33% smaller for 2.14 years (2.27 -.13 = 2.14 which is the difference in time spent in small classes for the small class group and large class group).
- attending a smaller class **increased KG test scores** as founded by Krueger (1999)
- Splitting college attendance (i.e. one indicator for college at 20 years old and another indicator for college before 27) is necessary to estimate effects of class size on college attendance properly. The effect is smaller in the latter group and they explain the mechanism behind as students tending to take up more classes early on than in the third or fourth year (i.e. part time vs full time). Attending a small class **increased the likelihood of attending college**, yet this was a weak result.
- Overall, students in small classes **have a higher probability of attending a better quality college**. Quality is defined as the mean earnings of the attendees. Yet, *conditional on attending college*, students from the small classrooms attend **lower quality schools** (based on their measure) than those in the larger classrooms.
- With respect to earnings, the regression estimate is **negative** (-\$124). They blame the imprecision of the estimate, but using some cleverness: The confidence interval’s upperbound is \$535. We saw earlier that a unit increase in percentile from KG test scores meant an increase of \$90 in earnings. We also saw that those in a small classroom can expect to score 5 percentile points

higher than those otherwise. Thus, being in a small classroom means  $5 \times 90 = \$450$  increase in earnings. **Nevertheless**, the conclusion reached says class size is not powerful enough to detect earnings increases of some magnitude as of age 27.

- The effect of class size is present in the summary index, yet the individual factors in the index are insignificant. Side note: Having a 401k is indicative of having a good job; however, being assigned to a small classroom in KG shows no variation in that.
- Considering **subgroups of the sample** (e.g. males, females, whites, blacks, rich or poor (proxied by free lunch status)), they show consistent positive effects when assigned to a small class. Most results are insignificant, but typically those with greater estimates for test scores indicates large estimates across the other adult outcomes. Yet, the STAR experiment is not powerful enough to detect heterogeneity in the impacts of class size on adult outcomes with precision.

### • Third Results: Teacher and Peer Effects

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$$y_{icnw} = \alpha_{nw} + \beta_1 SMALL_{cnw} + \beta_2 z_{cnw} + X_{icnw}\delta + \varepsilon_{icnw}$$

- $z_{cnw}$  is a vector of teacher and peer characteristics.
- On average, for kindergarteners, **having a more experienced teacher** translates to \$1104 more in earnings. The effect is significantly reduced for grades 1-3. A complete explanation cannot be given on the variation for lack of data. To be clear, **Experience was not randomly assigned**, thus the result is more along the lines of: Placing a child in KG classroom with a more experienced teacher will yield better outcomes. It *does not* mean we should increase the experience of teachers to yield better outcomes. The difference in earnings could come from intrinsic characteristics of experienced teachers. Put more concretely, they have a graph showing a linear relationship between teacher experience and earnings. Papers found that teacher performance plateaus after a few years. Thus, it must be other factors driving this linearity, in the particular in the later years. They therefore conclude that early childhood teaching has a causal impact on long term outcomes but that they cannot isolate the characteristics of teachers responsible for this effect.
- There is odd evidence of large classes with experienced teachers showing significantly positive results.
- **Peer Effects** definitely impact the study environment and can make for conducive settings. Proxying for peer abilities using fraction black, fraction female, fraction eligible for free and reduced lunch, and age; they find nothing conclusive from the data with respect to peer effects on earnings (but predictive of test scores). Estimates are imprecise (i.e. small estimates with large standard errors). The lack of power to measure these observables comes from the design itself; randomization across classrooms means no variation across classrooms for these pre-determined variables.

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