Weak Markets, Strong Teachers: Recession At Career Start and Teacher Effectiveness

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Abstract

How do alternative job opportunities affect teacher quality? We provide causal evidence on this question by exploiting business cycle conditions at career start as a source of exogenous variation in the outside options of potential teachers. Unlike prior research, we directly assess teacher quality with value-added measures of impacts on student test scores, using administrative data on over 30,000 teachers in Florida public schools. Consistent with a Roy model of occupational choice, teachers entering the profession during recessions are significantly more effective in raising student test scores. Results are supported by robustness tests and unlikely to be driven by differential attrition.

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1 Introduction

How do alternative job opportunities affect teacher quality? This is a crucial policy question as teachers are a key input in the education production function (Hanushek and Rivkin, 2012) who affect their students' outcomes even in adulthood (Chetty et al., 2014b). Despite their importance, individuals entering the teaching profession in the United States tend to come from the lower part of the cognitive ability distribution of college graduates (Hanushek and Pace, 1995). One frequently cited reason for not being able to recruit higher-skilled individuals as teachers is low salaries compared to other professions (e.g., Dolton and Marcenaro-Gutierrez, 2011; Hanushek et al., forthcoming).

Existing research provides evidence consistent with the argument that outside options matter. A first strand of the literature has used regional variation in relative teacher salaries, finding that pay is positively related to teachers' academic quality (e.g., Figlio, 1997). A second strand has used long-run changes in the labor market – in particular, the expansion of job opportunities for women – finding that the academic quality of new teachers is lower when job market alternatives are better (e.g., Bacolod, 2007). However, both bodies of evidence suffer from key limitations. First, relative pay may be endogenous to teacher quality. Second, measures of academic quality are poor predictors of teacher effectiveness (cf. Jackson et al., 2014). This important policy question therefore remains unresolved.

We exploit business cycle conditions at career start as a source of exogenous variation in the outside labor-market options of potential teachers.[1] Because the business cycle conditions at career start are exogenous to teacher quality, our reduced-form estimates reflect causal effects. In contrast to prior research, we directly measure teacher quality with value-added measures (VAMs) of impacts on student test scores, a well-validated measure of teacher effectiveness (e.g., Kane and Staiger 2008; Chetty et al. 2014a,b; and Jackson et al. 2014 for a review). Combining our novel identification strategy with VAMs for individual elementary school teachers from a large U.S. state, we provide causal evidence on the impact of economic downturns on teacher quality.

[1]To our knowledge, the idea that outside labor-market options at career start matter for teacher quality was first proposed by Murnane and Phillips (1981) in their classic paper on "vintage effects." Zabalza (1979) provides early evidence that starting salaries within teaching influence individual decisions to enter the profession, while Dolton (1990) finds large impacts of teachers' relative earnings and earnings growth.

Our value-added measures are based on individual-level administrative data from the Florida Department of Education on over 30,000 4th- and 5th-grade teachers in Florida's public schools and their students. The data include Florida Comprehensive Assessment Test (FCAT) math and reading scores for every 3rd-, 4th-, and 5th-grade student tested in Florida in the 2000-01 through 2008-09 school years. The data also contain information on teachers' total experience in teaching (including experience in other states and private schools), which is used to compute the year of entry into the profession (which is not directly observed). Following Jackson and Bruegmann (2009), we regress students' math and reading test scores separately on their prior-year test scores, student, classroom, and school characteristics, and grade-by-year fixed effects to estimate each teacher's value-added. We then relate the VAMs in math and reading to several business cycle indicators from the National Bureau of Economic Research (NBER) and the Bureau of Labor Statistics (BLS).

We find that teachers who entered the profession during recessions are substantially more effective in raising students' test scores than teachers who entered the profession during non-recessionary periods. Figure 1 shows this main result of the paper, plotting average VAM in math by teacher entry cohort against our main recession indicator. In our regressions, recession teachers are about 0.1 standard deviations (SD) more effective in raising math test scores. The effect is half as large for reading value-added. Quantile regressions suggest that the difference in math value-added between recession and non-recession entrants is, if anything, more pronounced at the upper end of the teacher effectiveness distribution. Based on figures from Chetty et al. (2014b), the difference in average math effectiveness between recession and non-recession entrants implies a difference in students' discounted life-time earnings of around \$13,000 per classroom taught each year.[2] Under the more realistic assumption that only 10% of recession-cohort teachers are in this cohort *because* of the recession, these teachers are roughly one SD more effective in teaching math than the teachers they push out. Based on the variation in teacher VAMs in our data, being assigned to such a teacher would increase a student's test scores by around 0.20 SD.

[2]Chetty et al. (2014b) estimate that students who are taught by a teacher with a 1 SD higher value-added measure at age 12 earn on average 1.3% more at age 28. Assuming a permanent change in earnings and discounting life-time earnings at 5%, this translates into increases in discounted life-time earnings of \$7,000 per student. We obtain our estimate by multiplying this number by our effect size and average classroom size.

Robustness regressions show that neither business cycle conditions in the years before or after teachers' career starts, nor those at certain critical ages (e.g., when most students enter or complete college), impact teacher effectiveness; only conditions at career start matter. While we cannot investigate attrition in the years *before* our sample period, further analyses suggest that our results are not driven by differential attrition among recession and non-recession cohorts. Although teachers entering during recessions are more likely to exit the profession during our sample period, the observed attrition pattern works against our finding and, if anything, suggests that our results understate the differences in effectiveness between recession and non-recession cohorts at career start. The results are also not driven by any single recession cohort, nor by the use of recessions as our preferred measure of business cycle conditions. Using alternative business cycle measures such as unemployment levels and changes yields very similar results and even predicts value-added fluctuations across cohorts who entered the profession outside recessions. The recession effect is not driven by differences in teacher race, gender, age at career start, school characteristics, or cohort size. Our finding that the effect of recessions on teacher effectiveness is twice as strong in math as in reading is consistent with evidence that wage returns to numeracy skills are twice as large as those to literacy skills in the U.S. labor market (Hanushek et al., 2015). These results are also consistent with the common finding that students' reading scores are more difficult to improve than their math scores (cf. Jackson et al., 2014).

Our analysis is motivated by arguments from a simple Roy model of occupational choice (Roy, 1951). Teaching is a relatively stable occupation over the business cycle since teacher demand depends primarily on student enrollment and is typically unresponsive to short-run changes in macroeconomic conditions (e.g., Berman and Pfleeger, 1997).[3] Therefore, high-skilled individuals may choose teaching over other professions during recessions because of lower (expected) earnings in those alternative occupations. Consistent with this idea, existing studies show that the supply of workers for public sector jobs in the U.S. is higher during economic downturns (e.g., Krueger, 1988; Borjas, 2002). Falch et al. (2009) document the same pattern for the teaching profession in Norway. *Teach For America*, an organization that recruits academically talented college graduates into

[3]Kopelman and Rosen (2016) report higher job security for public sector jobs (including teaching) than for jobs in the private sector. Consistently, newspapers have reported that teaching is recession-proof. During the most recent recession, however, job security for teachers did decline substantially (e.g., New York Times, 2010). This last downturn does not drive our results.

teaching, saw a marked decline in the number of qualified applicants during the recent economic recovery (New York Times, 2015). Meanwhile, several U.S. states have reported sharp declines in enrollment in university-based teacher preparation programs as the job market has improved (National Public Radio, 2015). We cannot disentangle mechanisms convincingly but present suggestive evidence that leads us to interpret these results as supply effects, rather than demand effects or direct impacts of recessions on teacher effectiveness.

If our results are indeed driven by changes in teacher supply, they would have important policy implications. First, they suggest that increasing the economic benefits of becoming a teacher may be an effective strategy to increase the quality of the teaching workforce. In contrast to de Ree et al. (2018), who find that unconditional increases in teacher pay for incumbent teachers do not improve student achievement, our results suggest that *selection into teaching* is affected by changes in economic benefits. This is in line with field-experimental evidence from developing countries: for example, Ashraf et al. (2018) find that selection of individuals under career incentives rather than purely social incentives leads to better outcomes in public service delivery. Second, our results also suggest that recessions may provide a window of opportunity for the public sector to hire more able applicants. Finally, they also suggest that recent improvements in cognitive skills among new teachers in the U.S. documented by Goldhaber and Walch (2013) may be attributable to the 2008-09 financial crisis, rather than an authentic reversal of long-term trends.

We extend previous research that has called attention to the potential importance of outside job options for teacher quality. Most recently, Britton and Propper (2016) exploit centralized wage regulation that generates regional variation in teachers' relative wages in England to document positive effects of relative teacher pay on school productivity.[4] However, their school-level data do not allow them to disentangle selection into the teaching profession from the sorting of teachers into specific schools and potential differences in teacher effort due to efficiency wage effects. Bacolod (2007) documents a decrease in the academic quality (as measured by standardized test scores and undergraduate institution selectivity) of female teachers in the U.S. over time that coincided with improvements

[4] Loeb and Page (2000) similarly relate regional variation in relative teacher wages and unemployment rates to rates of educational attainment but also lack direct measures of teacher quality.

in women's outside options.[5] In comparison with her study, we use a more rigorous identification strategy and direct measures of teachers' performance on the job.

Business cycle fluctuations have previously been exploited as a strategy to identify selection effects in the labor market. Oyer (2008), for example, studies the impact of the business cycle on the likelihood that MBA graduates enter the banking sector. He finds that entering investment banking as a graduate during a bull market increases life time earnings by around $1.5-5 million.[6] Boehm and Watzinger (2015) show that PhD economists graduating during recessions are more productive in academia, a finding best explained by a Roy-style model. Finally, Shu (2012) finds that a one percentage point decrease in the unemployment rate in the year of graduation decreases the future annual patent output of an MIT cohort by around 5% on average. Although the settings differ, our effect is similar to the findings of these papers. But while these studies enhance the plausibility of our findings, they relate to rather small groups in the labor market with highly specialized skills. Teachers, in contrast, make up roughly 3 percent of full-time workers in the U.S. and play a critical role in developing the human capital of future generations. Moreover, little is known about how to improve the quality of the teaching workforce. Thus, extending this identification strategy to teacher quality fills an important gap in the literature.

The paper proceeds as follows. Section 2 briefly presents the conceptual framework and describes the teaching profession in Florida. Section 3 introduces the data, explains our value-added measures, and presents our empirical model. Section 4 reports results on the relationship between business cycle conditions at career start and teacher effectiveness in math and reading and provides robustness checks. Section 5 discusses potential implications for policymakers. Section 6 concludes.

2 Conceptual Framework and Setting

In this section, we provide a short conceptual framework to motivate our analysis. We then document the feasibility of a short-run response in teacher supply to fluctuations in

[5] Corcoran et al. (2004), Hoxby and Leigh (2004), and Lakdawalla (2006) provide additional evidence of the importance of outside job options for the supply of American teachers.

[6] A small literature also documents persistent negative wage effects of completing college during a recession (e.g., Kahn, 2010; Oreopoulos et al., 2012).

economic conditions by providing information on the pool of potential teachers nationally and describing the requirements for entry into the teaching profession in Florida.

2.1 Conceptual Framework

Our analysis is motivated by a simple Roy-style framework of self-selection (Roy, 1951), where individuals choose an occupation to maximize (expected) utility.[7] Individuals choose between working in the teaching sector and working in a different sector, which comprises all outside labor-market options of potential teachers. If ability is valued in both sectors, but teaching has lower returns to ability, the selection of talent into teaching is negative.[8] When a recession hits the economy, the share of individuals seeking employment in the teaching sector increases as teaching becomes more attractive relative to more cyclical outside options.[9] Because of the initially negative selection into teaching, this implies that the average ability of individuals entering the teaching sector during economic downturns is higher. We will test this by using teacher value-added as a measure of effectiveness in the classroom.

While this argument only addresses the supply of teachers, fluctuations in demand could also explain changes in teacher quality over the business cycle. Fluctuations in demand would lead to higher quality of teachers entering during recessions if the following two conditions hold. First, school authorities are able to assess the quality of inexperienced applicants and accordingly hire the more able ones. Second, fewer teachers are hired during recessions than during booms. If either of these two conditions does not hold, fluctuations in demand cannot cause recession teachers to be more effective than non-recession teachers. We discuss the relative merit of explanations of our results based on supply-side and demand-side mechanisms in more detail in Section 4.5.

[7]For a simple model outlining this framework, see the working paper version (Nagler et al., 2015).

[8]Both of these assumptions have empirical support. First, Chingos and West (2012) show that, among 35,000 teachers leaving Florida public schools for other industries, a 1 SD increase in teacher value-added is associated with 6–8 percent higher earnings in non-teaching jobs. Second, wages are more compressed in the government-dominated teaching profession than in the business sector (cf. Hoxby and Leigh, 2004; Dolton, 2006).

[9]Both the employment probability and relative earnings likely change in favor of the teaching profession during recessions, but we cannot discriminate between these two channels in our empirical analysis. Empirically, employment in the teaching sector is less cyclical than employment in other sectors (see Berman and Pfleeger 1997; Simpkins et al. 2012).

2.2 Supply of Potential Teachers in Florida

Nationally, the number of individuals completing teacher education programs each year has been roughly double the number of newly hired teachers since at least 1987, when the earliest comprehensive data are available (Cowan et al., 2016). This implies that, at any point in time, there is a large pool of potential teachers nationally who are eligible to obtain certification immediately, regardless of the rigidity of state certification regimes. It also suggests that, for many potential teachers, the key decision about whether or not to enter the profession occurs when they enter the labor market rather than when they choose a degree program.

Contrary to the national situation, the demand for new teachers in Florida has exceeded the supply of new graduates from in-state preparation programs since at least the 1980s due to growth in the student population and, since 2003, a statewide class-size reduction mandate (Moe, 2006). In response to this pressure, state policymakers have consistently sought to recruit teachers from outside Florida. For example, a 1983 law required the Florida Department of Education to create a teacher referral and recruitment center to pursue strategies such as advertising teaching positions in states with declining enrollments and in major newspapers and establishing a national toll-free number to handle inquiries from prospective teachers (Florida Department of Education, 1986). In the 1980s, the state estimated that as many as 45 percent of new teachers in Florida had completed their preparation program in another state. Similarly, the U.S. Department of Education (2013) indicates that 23 percent of individuals receiving their initial Florida teaching credential in 2009 were prepared out-of-state. In our data, 19% of teachers report having teaching experience in other states. These statistics highlight the extent to which the pool of potential teachers for Florida public schools is national in scope and therefore apt to be influenced by national rather than state-specific economic conditions.

Temporary fluctuations in economic conditions are also more likely to influence selection into teaching when certification regimes permit as many individuals as possible to enter the profession without completing additional training. Traditionally, American states required potential teachers to complete an undergraduate or master's degree teacher preparation program in order to be certified to teach. Although in practice individuals without certification were often granted emergency credentials, these certification requirements likely constrained any short-term supply response. In recent decades, however, shortages of certified teachers in specific subject areas led many states to create alternative entry routes that allow college graduates who have not completed a traditional preparation program to begin teaching immediately while completing the remaining requirements for professional certification. As of 2011, 45 states had approved an alternative certification program and individuals completing these programs comprised roughly 20 percent of all individuals completing teacher preparation programs nationwide (U.S. Department of Education, 2013).

Florida's certification regime is typical of those states that have created alternative entry routes into teaching. The state initially awards professional teaching certificates only to graduates of state-approved teacher preparation programs who have passed tests of general knowledge, professional education, and the subject area in which they will teach.[10] However, college graduates who have not completed a teacher preparation program are eligible for a temporary certificate if they majored or completed a specified set of courses in the relevant subject area. They may also become eligible for a temporary certificate by passing a test of subject-matter knowledge. Individuals with a temporary certificate may then teach for up to three years while completing 15 credit hours of education courses and a school-based competency demonstration program. These arrangements allow any college graduate to enter the teaching profession in Florida (at least temporarily) in response to labor market conditions by passing a single exam.

Florida first authorized alternative certification for teachers in all grades and subject areas in 1997 and, since the 2002-03 school year, has required that each school district in the state offer its own alternative certification program (Moe, 2006). However, the state permitted school districts to hire teachers on temporary certificates for up to two years even before creating a formal alternative route and, until 1988, allowed the same individual to receive a temporary certificate multiple times (Florida Department of Education, 1986). The extent to which certification requirements may have constrained the supply response to labor market conditions among college graduates in the state prior to that period is therefore unclear.

[10]Florida also recognizes professional certificates in comparable subject areas granted by other states and by the National Board of Professional Teaching Standards.

3 Data and Empirical Strategy

In this section, we introduce our data, explain our value-added model, and describe our empirical strategy. We use variation in career start years to analyze the impact of outside labor-market opportunities on the selection into teaching. We estimate the career start year by subtracting total experience in teaching from the year in which we observe the teacher.

3.1 Data

Teacher value-added measures are based on administrative data from the Florida Department of Education's K–20 Education Data Warehouse (EDW). Our EDW data include observations of every student in Florida who took the state test in the 2000–01 through 2008–09 school years, with each student linked to his or her courses (and corresponding teachers). We focus on scores on the Florida Comprehensive Assessment Test (FCAT), the state accountability system's "high-stakes" exam. Beginning in 2001, (only) students in grades 3–10 were tested each year in math and reading. Thus, annual gain scores can be calculated for virtually all students in grades 4–10 starting in 2002. This implies that we can compute value-added measures for teachers observed in the 2001-2002 school year or later. The data include information on the demographic and educational characteristics of each student, including gender, race, free or reduced price lunch eligibility, limited English proficiency status, and special education status.

The EDW data also contain detailed information on individual teachers, including their demographic characteristics and teaching experience. We use only 4th- and 5th-grade teachers because these teachers typically teach all subjects, thus avoiding spillover effects from other teachers. We construct a dataset that connects teachers and their students in each school year through course enrollment data. Our teacher experience variable reflects the total number of years the teacher has spent in the profession, including both public and private schools both in Florida and other states. This experience variable contains a number of inconsistencies, which we try to correct. We treat the latest observed experience value as correct (assuming information updating at the Florida Department of Education) and adjust all other values accordingly. *Year of career start* is defined as the calendar year at the end of the school year a teacher is observed in the data minus total years of teaching experience. We adjust career start dates for gaps in teaching observed after 2002, that is, when we directly observe whether a teacher is working in Florida public schools each year.[11] We are unable to adjust for gaps in teaching prior to this time, potentially introducing additional error into the measurement of the career start date for teachers starting their careers before the 2000-01 school year.[12]

Starting from the baseline dataset that contains all 4th- and 5th-grade students with current and lagged test scores, we apply several restrictions to keep only those teachers who can be confidently associated with students' annual test score gains. We only keep student-teacher pairs if the teacher accounts for at least 80% of the student's total instruction time (deleting 24.5% of students from the baseline dataset). We exclude classrooms that have fewer than seven students with current and lagged scores in the relevant subject and classrooms with more than 50 students (deleting 1.8% of students). We also drop classrooms where more than 50% of students receive special education (deleting 1.5% of students). We further exclude classrooms where more than 10% of students are coded as attending a different school than the majority of students in the classroom (deleting 0.7%) and we drop classrooms for which the teacher's experience is missing (deleting 1.8% of students). Finally, we restrict the sample to the most recent 40 cohorts of teachers, which ensures that we have large enough cohort samples to estimate the business cycle effects (at least 150 teachers per cohort).[13] This yields a dataset of almost 32,600 public school teachers with value-added measures for math and reading.

Our main indicator for the U.S. business cycle is a dummy variable reflecting recessions as defined by the National Bureau of Economic Research (NBER). Recession start and end dates are determined by NBER's Business Cycle Dating Committee based on real GDP, employment, and real income. The NBER does not use a stringent, quantitative definition of a recession, but rather a qualitative one, defining a recession as "a period between a peak and a trough" (see http://www.nber.org/cycles/recessions.html). For example, the NBER dates the economic downturn of the early 1990s to have occurred between July 1990 (peak) and March 1991 (trough). We code our recession indicator

[11] As expected, results are slightly weaker, but similar, when using the original, uncorrected values.

[12] Overall, about one quarter of teachers in our sample whom we observe both in 2000-01 and 2008-09 (i.e., in the first and last school year in our sample) gain less than eight years of experience. This does not vary with teachers' initial experience level.

[13] Note that including all teachers (i.e., ignoring cohort size) or alternatively using the eligibility thresholds for regular retirement benefits of Florida teachers as sample restrictions (i.e., teachers with at most 30 years of experience and below the age of 63; see Chingos and West (2015)) yields very similar results.

variable to be one in 1990 (the beginning of the recession), and zero in 1991. Accordingly, teachers starting their careers in the 1990-91 school year are classified as having entered during a recession.[14] In robustness checks, we use alternative business cycle indicators such as unemployment (in levels and annual changes, for college graduates and overall, nationwide and in Florida, and for specific industries) and GDP, which come from the Bureau of Labor Statistics and the Bureau of Economic Analysis. NBER's recession indicator is highly correlated with unemployment rates (both levels and annual changes) and GDP.

3.2 Empirical Strategy

This section describes the estimation of the teacher value-added measures and our strategy for analyzing the relationship between business cycle conditions at career start and teacher effectiveness.

Estimating Teacher Value-Added

Teacher value-added measures (VAMs) aim to gauge the impact of teachers on their students' test scores. We estimate VAMs for 4th- and 5th-grade teachers based on students' test scores in math and reading from grades 3 to 5.[15] To estimate the value-added for each teacher, we regress students' math and reading test scores separately on their prior-year test scores, student, classroom, and school characteristics as well as grade-by-year fixed effects. Student-level controls include dummy variables for race, gender, free- and reduced-price lunch eligibility, limited English proficiency, and special-education status. Classroom controls include all student-level controls (including prior-year test scores) aggregated to the class level and class size. School-level controls include enrollment, urbanicity, and the school-specific shares of students who are black, white, Hispanic, and free- and reduced-price lunch eligible.

To obtain an estimate of each teacher's value-added, we add a dummy variable, θ_j, for each teacher:

[14] To capture the economic downturn, the unemployment difference measures the change from 1990 to 1991. Similarly, the unemployment level is taken from 1991 because the lack of job openings is reflected in a higher unemployment level in the following year. The unemployment level in the following year is also most strongly correlated with the binary recession indicator.

[15] Note that student testing in Florida starts in grade 3 only.

$$A_{ijgst} = \widehat{\alpha} A_{i,t-1} + \beta X_{it} + \gamma C_{it} + \lambda S_{it} + \pi_{gt} + \theta_j + \epsilon_{ijgst}$$

where A_{ijgst} is the test score of student i with teacher j in grade g in school s in year t (standardized by grade and year to have a mean of zero and standard deviation of one); $A_{i,t-1}$ contains the student's prior-year test score in the same subject; X_{it}, C_{it}, and S_{it} are student-, classroom-, and school-level characteristics; π_{gt} are grade-by-year fixed effects; and ϵ_{ijgst} is a mean-zero error term. After estimating the teacher VAMs, θ_j, we standardize them separately for math and reading to have a mean of zero and a standard deviation of one.[16]

We control for school characteristics rather than include school fixed effects because the latter would eliminate any true variation in teacher effectiveness across schools. However, in Section 4.3 we show that our results are robust to including both school and school-by-year fixed effects.

Since test scores suffer from measurement error, the coefficient on the lagged test score variable, $A_{i,t-1}$, is likely downward biased, which would bias the coefficients on other control variables correlated with lagged test scores. We therefore follow Jackson and Bruegmann (2009) and use $\widehat{\alpha}$, which is the coefficient on the lagged test scores from a two-stage-least-squares model where the second lag of test scores is used as an instrument for the lagged test scores (see the web appendix of Jackson and Bruegmann (2009) for details). Because this procedure requires two lags of test scores, the estimation of $\widehat{\alpha}$ is based on 5th-grade students only (since students were not tested in grade 2). The results from the math and reading IV models are reported in Table A1 in the Appendix. The first-stage results show that the twice lagged test scores strongly predict the lagged test scores both in math and in reading (Columns 1 and 2). The coefficient estimates are about 0.6. When instrumenting the lagged test scores with the twice lagged scores (thus eliminating idiosyncratic measurement error) in the second stage, the coefficients on the test scores, as expected, substantially increase and become close to 1 (Columns 3 and 4). Most importantly, however, our results do not depend on instrumenting the lagged test scores with the twice lagged test scores (see Section 4.3).

[16]To simplify notation, we drop the subscripts j, g, and s for the lagged test score and for the student-, classroom-, and school-level characteristics. We include grade-by-year fixed effects because test scores have been standardized using the full sample of students and because teachers are not observed in all years.

Although widely used by researchers, the reliability of value-added models of teacher effectiveness based on observational data continues to be debated (see, e.g., Jackson et al., 2014; Chetty et al., 2017; Rothstein, 2017). The key issue is whether non-random sorting of students and teachers both across and within schools biases the estimated teacher effectiveness. This would be the case if there were systematic differences in the unobserved characteristics of students assigned to different teachers that are not captured by the available control variables.[17]

Value-added models have survived a variety of validity tests, however. Most importantly, estimates of teacher effectiveness from observational data replicate VAMs obtained from experiments where students within the same school were randomly assigned to teachers (Kane and Staiger, 2008; Kane et al., 2013). Chetty et al. (2014a) and Bacher-Hicks et al. (2014) exploit quasi-random variation from teachers switching schools to provide evidence that VAMs accurately capture differences in the causal impacts of teachers across schools.[18]

Even if our VAMs were biased by non-random sorting of students and teachers, it is unclear whether this would bias our estimates of the relationship between recessions at career start and teacher effectiveness. In Section 4.3, we show that our results are robust to the inclusion of student fixed effects, which eliminates any non-random sorting of students to teachers. However, we refrain from using this specification as our main value-added measure because adding student fixed effects only allows the comparison of teachers who teach the same group of students, while eliminating any (useful) variation in teacher effectiveness across teachers who teach different classrooms or in different schools. In contrast, our main VAMs allow comparing teachers' effectiveness across classrooms and schools (Jackson, 2009).

Finally, some critics argue that value-added measures may reflect teaching to the test rather than true improvements in knowledge. In a seminal study, Chetty et al. (2014b) find that having been assigned to higher value-added teachers increases later earnings and the likelihood of attending college and decreases the likelihood of teenage pregnancy for girls. Of course, there may be other dimensions of teacher quality not captured by VAMs (e.g., Jackson, forthcoming). The weight of the evidence, however,

[17]For a more general discussion on the assumptions behind value-added models, see Todd and Wolpin (2003).

[18]Using a different administrative data set, Rothstein (2017) argues that evidence on school switchers does not rule out the possibility of bias. His arguments are criticized in the reply by Chetty et al. (2017).

indicates that teacher value-added measures do reflect important aspects of teacher quality.

Business Cycle Conditions at Career Start and Teacher Value-Added

To estimate the effect of business cycle conditions at career start on teacher effectiveness, we relate the macroeconomic conditions in the U.S. at career start to a teacher's value-added in math and reading. Specifically, we estimate the following reduced-form model:

$$\widehat{\theta}_j = \alpha + \gamma Rec_{js} + \beta X_j + u_j$$

where $\widehat{\theta}_j$ is the value-added of teacher j (either in math or in reading). Rec_{js} is a binary indicator that equals 1 if teacher j started working in the teaching profession (in year s) in a recessionary period and equals 0 otherwise. The vector X_j includes teacher characteristics. Most importantly, it contains total experience in the teaching profession (yearly experience dummies), which is not accounted for in the VAM computation but has been shown to influence teacher effectiveness (e.g., Papay and Kraft, 2015).[19] As experience differs between recession and non-recession teachers – due in part to the idiosyncratic distance between recessions and the time period covered by our administrative data – experience is a necessary control. Additional teacher characteristics included in some specifications are year of birth, age at career start, educational degree, gender, and race. Controlling linearly for year of birth and age at career start implies that we are implicitly also controlling for a linear time trend in the career start year.[20] Note that these teacher characteristics do not influence the business cycle. The reduced-form estimate γ (controlling only for experience) therefore identifies a causal effect. To the extent that the inclusion of additional controls changes the estimate of γ, they represent mechanisms rather than confounders. In some specifications we also control for teacher cohort size to address the possibility that our effects may be driven by fluctuations in teacher demand over the business cycle.

Because the source of variation we exploit is the business cycle conditions at career start, we always adjust standard errors for clustering at the level of the career start year. This

[19] Previous work has shown that teacher experience affects teacher value-added non-linearly (e.g., Rockoff, 2004). Wiswall (2013) shows that non-parametric specifications yield the most convincing results. Our results are robust to using teachers with above 20 or 25 years of experience as the omitted category.

[20] Because we already include grade $\times$ school-year fixed effects when estimating the VAMs, we do not control for a linear trend in school years in the reduced-form model.

way of computing standard errors follows the literature that also uses national business cycles for identification (e.g., Oyer, 2008; Kahn, 2010; Oreopoulos et al., 2012; Boehm and Watzinger, 2015; Shu, 2012). Because of the limited number of clusters (40 entry cohorts in the main specifications), however, inference may be problematic. Using alternative procedures such as wild-cluster bootstrap (Cameron et al., 2008) yields similar standard errors in most specifications (see Appendix Table A2). However, wild-cluster bootstrap also runs into problems in settings with binary treatment and few treated clusters. This is reflected in the very low standard errors when using this procedure for our main specification (Column 1).

To assess whether our estimated recession effect is statistically significant due to pure chance, we additionally perform a permutation test to assess how likely an effect of our estimated size is when assigning teacher cohorts randomly to a recession or non-recession cohort (at career start).[21] To do so, we use the randomization inference test (*ritest*) developed by Heß (2017). The randomization inference test reveals that our estimated coefficient is statistically significant at the one percent level and larger in magnitude than almost all simulated effect sizes (Figure A1). Note that this permutation test has to be interpreted cautiously, however, since it ignores the time series dimension of our data: because the recession indicator is assigned independently across years in the permutation test, this likely makes it harder to find placebo recession effects, which are rather low-frequency in the actual data but need to be high-frequency to show up in the permutation. Nevertheless, the randomization inference test is helpful in assessing whether our results are likely to be driven purely by chance.

Based on our conceptual framework, we expect to find a positive effect of recessions at career start on teacher effectiveness since recessions negatively shock the outside options of potential teachers. Due to this shock, both the number and the average quality of applicants increases, leading to higher average value-added in recession cohorts. Since we do not observe the intermediate steps (e.g., application rates or earnings), we estimate a reduced-form relationship between business cycle conditions at career start and teacher value-added. Note also that because the effectiveness of all teachers in our sample is estimated during the same period (2001-2009), systematic differences in the effort levels

[21] Randomizing the treatment variable, while keeping the outcomes fixed, has been used in previous studies (e.g., Bertrand et al., 2004; Cattaneo et al., 2017).

of recession and non-recession teachers due to differences in the (policy or economic) environment seem unlikely.

4 Business Cycle Conditions at Career Start and Teacher Effectiveness

We start by documenting differences in math and reading effectiveness between recession and non-recession teachers. Using kernel density plots and quantile regressions, we then show at which parts of the effectiveness distribution recession and non-recession teachers differ. In robustness regressions, we show that teacher effectiveness is not associated with business cycle conditions several years before and after career start or with business cycle conditions at certain critical ages of teachers. We also show that our results are robust to using alternative business cycle indicators or alternative value-added measures and are not driven by any single recession. Finally, we provide suggestive evidence that our results are not driven by differential attrition of recession and non-recession teachers.

4.1 Main Results

We first present summary statistics separately for recession teachers and the much larger group of non-recession teachers (Table 1). The unemployment level of college graduates was higher when recession teachers started their careers. Similarly, unemployment was rising for recession teachers, but slightly falling for non-recession teachers. These differences are statistically significant at the one percent level. The size of the teacher cohorts (i.e., career start years) in our sample is on average very similar for recession and non-recession teachers. The share of male teachers is approximately the same in both samples. Among recession teachers, the share of teachers with a Master's or PhD degree is slightly larger and the share of white teachers is somewhat smaller. Because recession teachers started around three school years earlier than non-recession teachers on average, recession teachers also have more teaching experience. The average age at career start is around 30, while the median age at career start is 28. These ages are also very similar (median 28, mean 31) for the subsample of teachers who started their careers during our sample period. The two groups of teachers teach similar types of students as measured by the share of students who are black and by the share of students eligible for free or reduced-price lunch. Recession teachers have on average 0.08 SD higher math value-added and 0.05 SD higher reading value-added than non-recession teachers (with only the former difference being statistically significant).

After documenting the raw gap in math value-added between recession and non-recession teachers (see also Column 1 in Table 2), we add several teacher characteristics (Table 2). Due to the idiosyncratic distance between recessions and our sample period, experience is a necessary control. We therefore refer to Column 2 as our preferred specification. The value-added gap increases to 0.11 SD and standard errors are almost halved when dummies for teaching experience are included (Column 2).[22] Figure 1 presents this result as a time series of math VAM (adjusted for experience) against year of career start, with NBER recessions shaded in gray: Cohorts starting during recessions tend to have higher mean VAM in math than cohorts starting outside recessions. Adding year of birth and age at career start changes the coefficient on the recession indicator only little (Column 3). Note that the combination of these two variables yields the (linear) career start year, which is therefore implicitly also taken into account.

Further controlling for teacher characteristics, such as whether the teacher holds a Master's or PhD degree, and whether the teacher is male or white, also does not affect our coefficient of interest.[23] Finally, while the size of the teacher cohort in our sample is negatively related to math value-added, controlling for cohort size even slightly increases our estimated coefficient (Column 5). Since demand fluctuations over the business cycle can explain the negative impact of recessions on teacher effectiveness only if schools hire *fewer* (and more able) teachers during recessions (see Section 2.1), this finding suggests that demand fluctuations are unlikely to drive our result. The specification with all control variables indicates that recession teachers are around 0.1 SD more effective in teaching math than non-recession teachers. Since we interpret all control variables, except experience, as potential mechanisms rather than confounders, we omit them in all regressions below.

[22] The coefficient on the recession indicator increases because recession teachers are overrepresented among rookie teachers and the first years of teaching experience improve effectiveness the most.

[23] Differences in the placement of recession and non-recession teachers represent another potential mechanism through which recessions could impact productivity (Oyer, 2006). However, controlling for important student characteristics at the school level, such as the share of black students and the share of students eligible for free or reduced-price lunch, does not explain the value-added difference (results not shown).

The conceptual framework predicts selection effects due to changing outside labor-market options over the business cycle. Because existing research indicates that earnings returns are twice as large for numeracy than for literacy skills in the U.S. labor market (Hanushek et al., 2015), we would expect selection effects over the business cycle to be weaker for reading effectiveness than for math effectiveness. The effects on teachers' reading value-added are indeed similar to, but weaker than in math (Table 3). The bivariate relationship between recession at career start and teacher effectiveness is positive, but statistically insignificant (Column 1). As is the case for math value-added, controlling for teaching experience substantially reduces the standard error, rendering the coefficient estimate statistically significant at the one percent level (Column 2). Adding the other teacher characteristics and teacher cohort size reduces the coefficient of interest only slightly. In terms of magnitude, the recession impact for reading is about half as large compared to math (around 0.05 SD). As selection effects among potential teachers should be stronger with respect to math skills, we focus on teachers' math effectiveness in the remaining analyses.[24]

Heterogeneity of Effects

While Table 2 indicates that recession teachers are *on average* more effective in raising students' math test scores than non-recession teachers, it is unclear whether this effect is driven by fewer ineffective teachers or more highly effective teachers in recession cohorts. To analyze the recession impact across the distribution of math value-added, we estimate kernel density plots and quantile regressions. The kernel density plots of teachers' (experience-adjusted) math value-added reveal a rightward shift in math effectiveness for recession cohorts along the entire distribution except for the least effective teachers (Figure 2).[25] The quantile regressions, which also control for experience, are in line with this finding (Figure 3 and Appendix Table A3): while teachers at the very bottom of the value-added distribution have similar VAMs, recession teachers are significantly more effective than non-recession teachers from the 10th percentile onward. The largest VAM difference is observed among very highly effective teachers, with point estimates of

[24]The results of the following analyses show the same overall pattern for teachers' reading effectiveness, but are less pronounced and more volatile than the results for math. All results are available on request.

[25]A Kolmogorov-Smirnov test shows that the distributions are statistically significantly different at the one percent level.

differences peaking at almost 0.2 SD. Note that most point estimates between the 10th and 95th percentile do *not* differ significantly from each other. However, if anything, recession effects are more pronounced at the upper end of the teacher effectiveness distribution, which would be consistent with a supply-side explanation.

In Table 4, we run our preferred specification on subsamples to assess whether recessions have differential impacts across various groups of teachers. Male teachers seem to be more affected than female teachers (Columns 1 and 2), which may suggest that the career options of men are more strongly influenced by recessions than those of women. The results are, however, also in line with larger measurement error in the career start year for women, who are more likely to have gaps in their careers due to, e.g., child-rearing. In Columns 3 and 4, we find similar recession impacts for teachers with and without a Master's or PhD degree. In line with existing research (Jones and Schmitt, 2014; Hoynes et al., 2012), Columns 5 and 6 provide indirect evidence that minorities are more affected by recessions than whites. Finally, Columns 7 and 8 indicate that teachers starting their teaching careers at a relatively high age (above median) are more affected than those starting at younger ages. This may suggest that the decisions of mid-career entrants to the teaching profession are more strongly influenced by the outside labor market.[26]

Our main results show that teachers entering the profession in recessions are significantly more effective in raising students' test scores. In the remainder of this section, we will show the robustness of this result and provide tests of alternative explanations. First, we show that outside labor market conditions matter only at career start, not at any other point in teachers' lives. Second, we report the estimated impacts separately for each recession. We also show that results are the same when only using teachers whose careers we fully observe and when using only those who do not report teaching experience outside Florida. Third, we show the impact of labor-market conditions at career start using alternative measures of business-cycle conditions such as unemployment rates, unemployment changes, and GDP growth. Fourth, we provide suggestive evidence that differential attrition between recession and non-recession teachers does not seem to drive our results.

[26]Splitting the sample at the quartiles of the age at career start distribution, we find that the impact is weakest (and statistically insignificant) for individuals who become teachers at rather young ages (below age 25). The recession impact is above 0.1 SD, and statistically significant, for the other three sub-groups of teachers who started at older ages (results not shown).

4.2 Business Cycle Conditions in Other Years

So far we have assumed that it is the business cycle condition at the point in time when individuals enter the teaching profession that matters for their effectiveness. To test this hypothesis, we run robustness checks where we include recession indicators for the years before or after career start with lags and leads of up to three years. Adding these recession indicators to the main model does not change our coefficient of interest (Columns 2 and 3 in Table 5). Furthermore, the estimated effects of the business cycle conditions in the years before or after our preferred year are all close to zero and statistically insignificant.[27]

One might worry that our career start year measure captures the effect of macroeconomic conditions at key ages (Giuliano and Spilimbergo, 2014). For example, many individuals may decide to become teachers when entering college (around age 18) or upon completing their undergraduate or graduate studies (between ages 22 to 24). Therefore, we include recession indicators at ages 18-32 (in two-year steps) to confirm that it is the economic conditions at career start that affect teaching quality. With one exception, all coefficients on the indicators of recessions at specific ages are again close to zero and statistically insignificant (Column 4).

These analyses suggest that business cycle conditions *at career start*, but not at other points in time, such as the degree choice stage, influence the effectiveness of teachers.

4.3 Further Robustness Checks

In this subsection, we show that our result is not driven by any one recession and that it is robust to using more continuous measures of business cycle conditions at career start and robust to using alternative value-added measures.

Impact of Individual Recessions

Since the number of recession cohorts is limited, one might worry that our result is driven by a single recession. To investigate this issue, we include a separate binary indicator for each recession (Table 6). Column 1 indicates that teachers in the 1970 recession and all recent recessions have higher math value-added than the average non-recession teacher.

[27]Similarly, using each of these other recession indicators individually instead of our main recession indicator also yields small and mostly statistically insignificant coefficients (results not shown).

In Column 2, we combine the separate recession indicators for the adjacent recession years of 1980, 1981, and 1982 and find that teachers who started during those years are on average as effective as the average non-recession teacher. In Column 3, we only keep two non-recession cohorts immediately before and immediately after each recession, such that the cohorts being compared are more similar. This leads to the same finding: most recessions have positive effects on teacher effectiveness. The recession impact is not driven by a single recession.[28]

To further investigate the results from Table 6, we present estimates of the impact of a recession at career start on teacher value-added separately for recent and distant teacher cohorts (Table 7). In line with Table 6, Columns 2 and 3 show that the impact of recent recessions is stronger than the baseline estimate, while the impact of distant recessions is close to zero and not significant. This finding may, to some extent, also reflect differential patterns of attrition with respect to effectiveness among recession and non-recession teachers, an issue we examine in Section 4.4.

Since we estimate the year of career start (and do not observe gaps in teachers' careers due to fertility, child-rearing, or family mobility before our sample period begins), we assess whether our results are sensitive to this potential measurement error issue by restricting the sample to the entry cohorts for which we can observe the entire career. The recession impact for this group of teachers is more pronounced than the baseline effect (Column 4). However, because this subsample only contains two recession cohorts and very few cohorts in total (8 cohorts), we prefer to use all available teacher cohorts.

Finally, we test whether our estimates reflect selection into the teaching profession or rather selection of teachers with out-of-state experience into Florida public schools. In Column 5, we restrict the sample to those teachers without any teaching experience outside Florida. The coefficient of interest becomes somewhat larger than in the baseline specification.[29]

[28]Below, we show that teacher effectiveness can be predicted by unemployment levels and changes at career start even when the NBER recession cohorts are excluded.

[29]Moreover, there is no statistically significant difference in the incidence of teaching experience outside Florida between recession (25%) and non-recession cohorts (24%). Controlling for any out-of-state experience does not change our coefficient of interest either. This makes an explanation based on migration patterns into Florida unlikely.

Alternative Business Cycle Measures

We also evaluate the robustness of our results to the use of alternative measures of teachers' outside options. Figure 4 allows comparing our preferred binary measure of the business cycle (indicated by green and blue dots, respectively) and the variation of a continuous measure, i.e., one-year unemployment changes. In line with our baseline results, unemployment changes and teacher value-added are positively related.[30] In Table 8, we compare our preferred specification using the NBER recession indicator (Column 1) with alternative business cycle indicators, such as GDP growth (Column 2), unemployment level (Column 3), and one-year unemployment changes (Column 4), respectively. Both unemployment measures are based on the unemployment rates of college graduates, as this is the relevant labor market for potential teachers.[31]

Consistent with our baseline results, GDP growth is negatively related to teacher value-added. The coefficients on the unemployment measures are also in line with our main findings and statistically significant (at least) at the five percent level. The coefficient estimates using the alternative measures imply somewhat weaker, but qualitatively similar recession effects (based on the difference in each business cycle indicator between recession and non-recession cohorts), suggesting that none of the alternative business cycle indicators on its own fully captures the entire effects of a recession on potential teachers' choices. Finally, it is unlikely that the alternative job opportunities of potential teachers are evenly distributed across industries. For example, one would expect few potential teachers to work in agriculture. In Columns 5 and 6, we find that the one-year unemployment change in agriculture at career start is unrelated to teacher quality, while the labor-market conditions in nonagriculture industries do matter. This pattern is consistent with the selection of potential teachers into teaching who alternatively would have chosen industries requiring similar skills.

[30] In line with Figure 1, the time series of math value-added and unemployment differences and levels also move very closely, especially in the subsample of teachers who started their careers after 1990. Results are available upon request.

[31] We use national rather than Florida-specific unemployment rates in this analysis because state-level unemployment rates are not available for college graduates, the national unemployment rates are more reliable, and because Florida recruited teachers heavily from out of state throughout our sample period (see Section 2.2). Thus, using Florida-specific measures of economic conditions is likely to underestimate the true effect. In Appendix Table A4, we show that graduate-specific unemployment rates have a stronger impact on teacher value-added than general unemployment rates and that Florida-specific unemployment rates have a somewhat weaker impact than national unemployment rates.

The results in Table 6 may suggest that the effect of labor-market conditions at career start is (almost) only driven by the last three recessions. However, this is not the case. When excluding all eight NBER recession cohorts – that is, the entry cohorts with the clearest adverse labor market conditions at career start – all unemployment measures are still positively associated with teacher math effectiveness, with four of the six coefficients being statistically significant at conventional levels (Appendix Table A5). Compared to Table A4, which includes all teacher cohorts, the magnitudes of the coefficients in Table A5 are substantially attenuated. This finding is not surprising, given that Table A5 excludes some of the most severe recessionary periods from the sample. Overall, our results are not driven by only few unusual recessionary periods but hold even without these cohorts.

Alternative VAMs

To assess the sensitivity of our results with respect to the value-added measure, we run our preferred specification with alternative VAMs (Appendix Table A6). For comparison, Column 1 replicates the results based on our preferred VA measure (see Column 2 of Table 2). In Column 2, we add school fixed effects when estimating teachers' value-added. The inclusion of school fixed effects eliminates any bias from unobserved school characteristics that influence teacher effectiveness, but also removes variation in true teacher effectiveness to the extent that average teacher quality varies across schools. The gap in effectiveness between recession and non-recession teachers is somewhat attenuated, but the change is small. In Column 3, we add school-by-year fixed effects when estimating value-added. The estimate is further attenuated, but remains significant. In Column 4, we add student fixed effects when estimating value-added, which completely eliminates any non-random sorting of students to teachers. The recession impact is similar to using school-by-year fixed effects, indicating that our results are not driven by non-random sorting of students.[32] In Columns 5 and 6, we account for the fact that the precision of the teacher value-added measures varies across teachers. Our results are similar when weighting teachers in our preferred specification by the number of student-year or

[32]Note that including school or student fixed effects likely removes variation in true teacher effectiveness, for example through common effectiveness components at the school level, but also because of spillovers across teachers (Jackson, 2009). Furthermore, including student fixed effects only allows the comparison of teachers who teach the same group of students. In contrast, our preferred VA measure (without school or student fixed effects) allows comparing teacher effectiveness across classrooms and schools.

teacher-year observations that underlie their value-added measures. Finally, in Column 7, we use a value-added measure where the lagged test scores have not been instrumented with the twice-lagged test scores. This yields the same recession impact than our preferred VA measure. Overall, the exact specification of the VAM does not drive our results.

4.4 Differential Attrition of Teachers

We find that teachers who started their careers during recessions are more effective. On the one hand, effectiveness differences might already exist among entering teachers (*selection*). On the other hand, recession and non-recession teachers might have very similar VAMs at career start, but low-quality recession teachers might be more likely to leave the occupation than low-quality non-recession teachers (*differential attrition*). We use our data to assess which of these two channels is more plausible. Note, however, that we cannot directly address attrition before 2001-02, the first year for which we have VAMs. This is an important caveat since attrition rates are generally high and differential attrition might have happened already before we observe the teachers in our data.

Since our dataset includes teachers in the public school system in Florida, attrition in our sample means that a teacher leaves the Florida public school system. However, if differential attrition of recession and non-recession teachers were driving our results, then one would expect earlier recession cohorts to be much more effective, but more recent recession cohorts to be only slightly more effective, than non-recession teachers. This pattern is not present in Table 7, which shows that recession effects are generally larger for more recent cohorts. We interpret this as first, indirect evidence that differential attrition is unlikely to drive our results.

To provide more direct evidence, Figure 5 shows the number of teachers observed in 2001-02 (the first year for which we have VAMs) that dropped out by 2008-09 (the last year of our sample period) separately for high TVA and low TVA teachers and each career start year (Panel (a)). Attrition seems to be somewhat more common among low TVA teachers than high TVA teachers, especially among the more recent cohorts. However, this pattern does not appear to differ systematically between recession cohorts and non-recession cohorts.[33] Since cohort sizes differ substantially across career start

[33]The increase in attrition for the final cohort (i.e., teachers who started in the school year 2001-02) is largest because most teachers leave teaching during their first years of teaching.

years, Panel (b) shows attrition relative to cohort size, dividing the number of attrited teachers (from Panel (a)) by the size of the respective teacher cohort observed in 2001-02. Attrition rates are generally high, in line with the literature (West and Chingos, 2009). Similar to Panel (a), however, attrition relative to cohort size does not appear to differ systematically between recession and non-recession cohorts. Overall, there is thus no clear pattern during our sample period indicating that our results are likely to be driven by differential attrition. To provide additional evidence that attrition is unlikely to drive our results, we now turn to econometric analyses.

We define attrition as *not* being observed as a teacher during the last school year in our sample period (2008-09). First, we investigate whether starting during a recession is correlated with attrition (Columns 1 and 2 in Table 9).[34] Controlling for teachers' value-added, we find that recession teachers are somewhat more likely to drop out, although this difference is not statistically significant (Column 1). Controlling for recession status at career start, more effective teachers are less likely to drop out.

Among teachers who started teaching during our sample period (about 42% of the full sample), recession teachers are slightly less likely to leave the public school system than non-recession teachers, although this difference is not statistically significant (Column 2). More importantly, in recession cohorts, exiting teachers are significantly more effective compared to exiting non-recession teachers. This pattern works against our result, suggesting that the value-added gap is even larger at career start and decreases over time. This is confirmed in Column 3 when we look directly at value-added, finding a large gap at career start, which decreases with experience. This is in line with the observation that we only find effects for more recent recessions. Taken at face value, this estimate implies that the gap in value-added between recession and non-recession teachers closes after around 25 years. Because the implied time period before the gap closes depends on the functional form we impose on the interaction between starting in a recession and teaching experience, this number needs to be interpreted very cautiously. The same pattern holds, and in fact becomes much more pronounced, when using only teachers who started teaching during our sample period (Column 4).

In sum, with the important limitation that our data does not allow us to convincingly address attrition before the school year 2001-02, these analyses do not suggest that

[34]Because the school year 2008-09 is the attrition target year, these regressions exclude teachers who started teaching in 2008-09.

differential attrition between recession and non-recession teachers explains our finding. If anything, the observed attrition pattern seems to reduce the estimated difference in effectiveness between recession and non-recession teachers over time.

4.5 Discussion

The effect of recessions at career start on teacher effectiveness might be driven by demand or supply fluctuations over the business cycle (or both). As noted in Section 2.1, demand fluctuations can generate our findings only if school authorities (i) hire fewer teachers during recessions (e.g., due to budget cuts) *and* (ii) are able to assess the quality of inexperienced applicants and hire those applicants most likely to be effective.

Our results do not allow us to disentangle these two mechanisms convincingly. We argue, however, that the two conditions required for a demand-side explanation are unlikely to hold in our setting. First, in our data, the cohort size of teachers starting in any year (either measured as absolute numbers or as log cohort size) is not significantly related to the business cycle, as shown in Figure 6 and Table A7.[35] Given the sizeable point estimates and the limited number of only 40 (cohort) observations, these results have to be interpreted cautiously. Yet, recall that directly controlling for teacher cohort size does not affect the recession impact.

Second, it seems unlikely that school authorities are able to identify the best applicants since education credentials, SAT scores, and demographic characteristics – typically the only ability signals of applicants without prior teaching experience – are at best weakly related to teacher effectiveness as measured by VAMs (e.g., Chingos and Peterson, 2011; Jackson et al., 2014). Furthermore, our quantile regression results suggest that the recession effect is not driven by teachers at the bottom of the value-added distribution, which is predicted by demand fluctuations. If anything, the effects are more pronounced for very effective teachers.

Taken together, these results suggest that decreases in the demand for teachers during recessions are unlikely to be the driving force behind these effects.[36] We therefore interpret

[35]This finding is corroborated by official statistics from the BLS, which indicate that employment in the local government education sector typically increases during recessions (with the exception of the recessions in 1980-1982 and the Great Recession; see Figure 1 in the working paper version (Nagler et al., 2015); see also Berman and Pfleeger, 1997).

[36]Recessions may in principle also have a direct effect on teachers' effectiveness. This could happen, for example, if teachers who started their career in a recession were more fearful of losing their jobs and

our results to suggest that supply fluctuations over the business cycle are the more likely mechanism behind the impact of recessions at career start. At the same time, we acknowledge that we cannot disentangle the supply and demand channels in a decisive manner.

Finally, note that we estimate a reduced-form coefficient. To gauge the quality difference between recession-only teachers and those they replace, we have to inflate our reduced-form estimates by the share of recession-cohort teachers who would not have entered teaching under normal labor-market conditions. If all teachers who start during recessions became teachers only because of the recession, the effectiveness difference would be equal to our reduced-form estimate (0.11 SD). However, if only 10% of the recession teachers went into teaching due to the recession, the difference in effectiveness would be 10 times as large, around one SD. This would imply an impact on student math achievement of being assigned to a recession-only teacher of around 0.2 student-level standard deviations.

5 Policy Implications

Irrespective of the underlying mechanism, our findings have important implications for policymakers. If a demand-side explanation was correct, then recession teachers are more effective because fewer teachers are being hired during recessions. This would imply that policymakers have greater ability to identify effective candidates than is often assumed. Improvement strategies that take advantage of that, such as efforts to increase the pool of candidates, and perhaps increasing class size so as to be able to hire fewer, but better teachers, could then constitute effective policies.

However, although we cannot rule out other mechanisms with certainty, the pattern of results suggests to us that the supply-side explanation is more likely. In this case, increasing the economic benefits of becoming a teacher might be an effective strategy of improving the educational outcomes. In a Roy model of occupational choice, worse outside options during recessions are equivalent to higher teacher wages. Thus, our results

thus provided more effort, which raised their effectiveness permanently. However, in this case we would again expect the least effective teachers to be disproportionally better in recession cohorts. If the business cycle at career start did have a direct effect on the individual's teaching effectiveness, we would estimate the total effect of starting in a recession on subsequent career productivity in teaching, comprising the combined effect of selection into teaching and the direct impact on individual's productivity in teaching. The reduced-form estimate still represents a causal effect.

suggest that policymakers would be able to hire better teachers if they increased teacher pay. Would such a policy be efficient? Chetty et al. (2014b) find that students taught by a teacher with a one SD higher value-added measure at age 12 earn on average 1.3% more at age 28. Using this figure, our preferred recession effect translates into differences in discounted lifetime earnings of around $13,000 per classroom taught each school year by recession and non-recession teachers (evaluated at the average classroom size in our sample). This is equivalent to more than 20% of the average teacher salary in Florida ($46,583 in the school year 2012-2013 according to the Florida Department of Education).

Do these private benefits exceed the public costs associated with an increase in teacher pay intended to attract more effective teachers? To shed light on this question, assume that the entire recession effect is driven by earnings losses in the private sector during recessions. To compute these earnings losses, we use the median earnings of BA degree holders ($59,488 in 2010, the year Chetty et al.'s figures refer to) as a benchmark for the average outside option of potential teachers. The adverse impact of graduating in a recession has previously been estimated to be around 2%–6% of initial earnings per percentage point increase in the unemployment rate (e.g., Kahn, 2010). This translates into 4%–12% earnings differences between recession and non-recession teachers in our sample. Based on the median earnings of BA degree holders, this implies on average between $2,379 and $7,140 lower earnings during recessions. This admittedly coarse comparison suggests that it may be efficient to increase pay for new teachers and thereby improve average teacher effectiveness. Yet this conclusion comes with the caveat that it may be difficult for policymakers to increase pay only for incoming teachers. Our evidence does not imply that increasing pay for the existing stock of teachers would yield benefits. Moreover, there are likely cost-neutral ways to make the total compensation package offered to new teachers more attractive. For example, Fitzpatrick (2015) shows that the value teachers place on pension benefits is much lower than the cost to the government of providing them and would prefer higher salary levels.

Magnitudes aside, a supply-side explanation therefore also suggests that policymakers may be able to attract more effective individuals into the teaching profession by raising the economic benefits of becoming a teacher. This is not a trivial result. If intrinsic motivation positively affects teachers' effectiveness, then increasing teacher pay may attract more extrinsically motivated, but less effective individuals into the teaching profession.

Finally, our results indicate that recessions might serve as a window of opportunity for the public sector to hire more effective personnel than during normal economic periods. Hiring more teachers during downturns would increase the effectiveness of the teacher labor force. As teachers are a critical input in the education production function affecting students' lives way beyond schooling, hiring more teachers in economic downturns would appear an attractive strategy to improve American education.

6 Conclusion

We provide causal evidence on the importance of recessions at career start for the quality of teachers. We combine a novel identification strategy with a direct and well-validated measure of teacher effectiveness. Our reduced-form estimates show that teachers who entered the profession during recessions are significantly more effective than teachers who entered the profession during non-recessionary periods. This finding is consistent with a Roy-style model in which more able individuals prefer teaching over other professions during recessions due to less opportunities in alternative occupations. While the settings differ, our productivity effects are similar to recession effects on the productivity in other areas such as PhD economists (Boehm and Watzinger, 2015). Recessions may serve as a window of opportunity for recruitment in the public sector.

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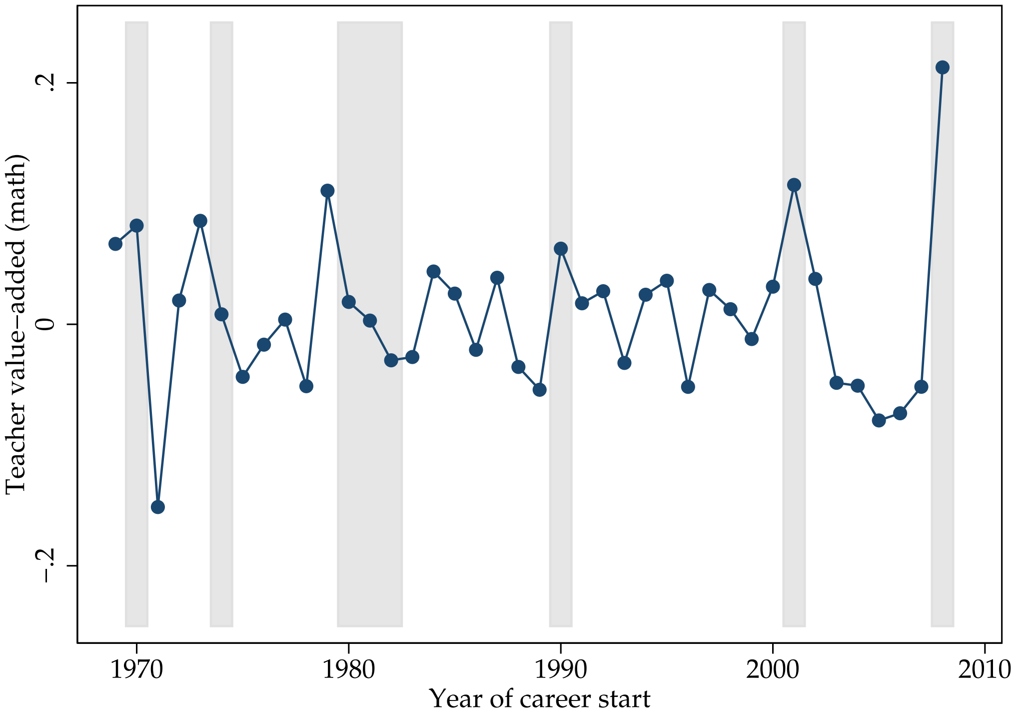
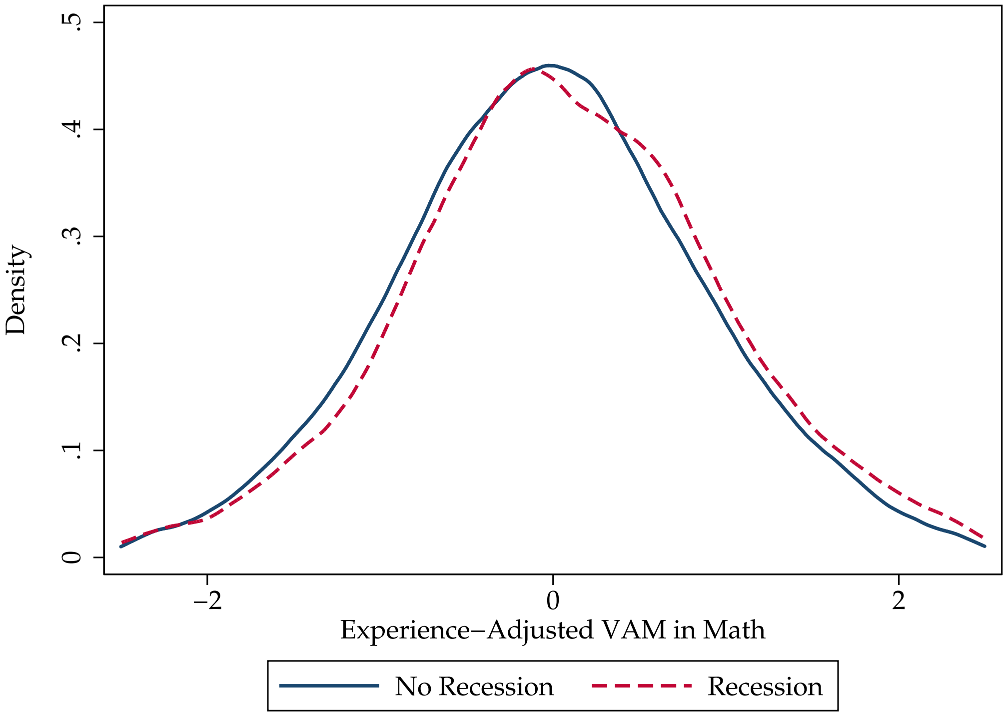
Figure 1: Mean Teacher Math Effectiveness by Year of Career Start



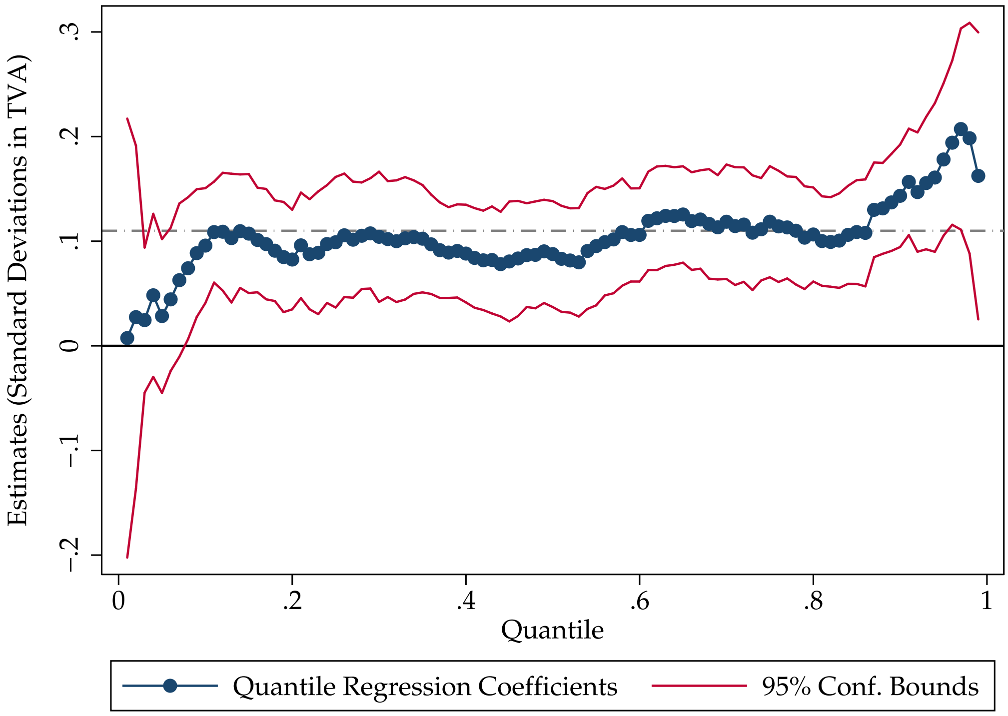
Notes: Cohort means of VAM in math (controlling for yearly experience dummies up to 30 years). Shaded areas are recession periods as defined by the NBER.

Figure 2: Recession at Career Start and Teacher Math Effectiveness (Kernel Density Estimates)



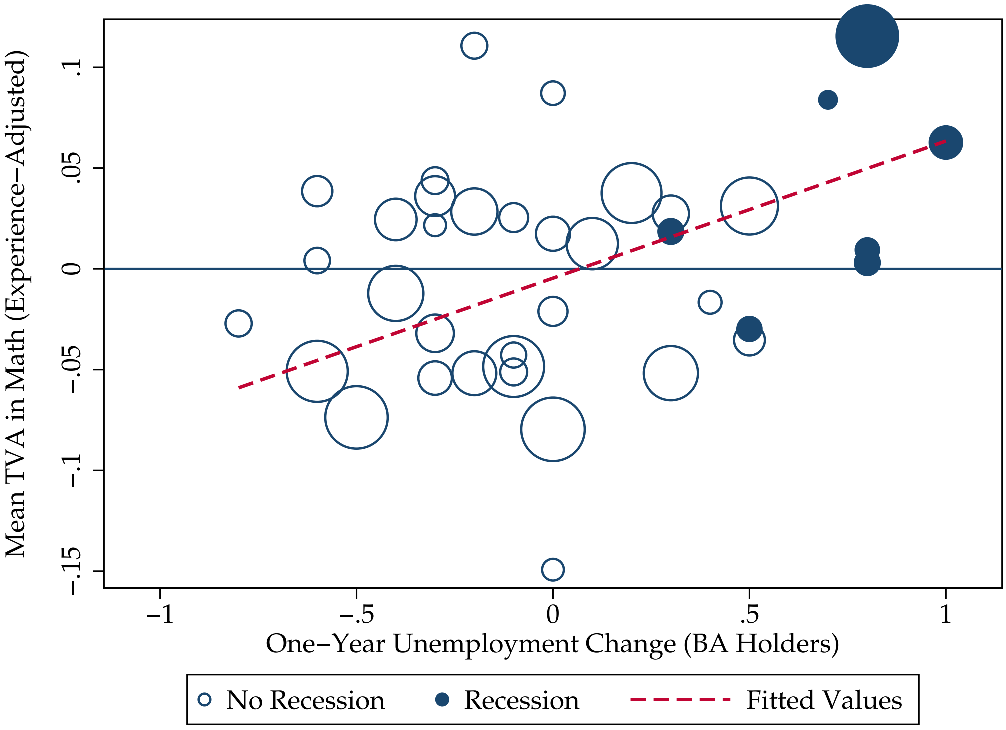
Notes: Kernel density estimates of VAM in math (controlling for yearly experience dummies up to 30 years) by recession cohort status. Excludes teachers with experience-adjusted $|VAM| > 2.5$ for better visibility (which excludes 611 of 32,585 teachers). Experience-adjusted VAM is normalized to have mean 0 and standard deviation 1 among all teachers. A Kolmogorov-Smirnov test shows that the distributions are statistically significantly different ($p < 0.01$).

Figure 3: Recession at Career Start and Teacher Math Effectiveness (Quantile Regressions)



Notes: Coefficients and 95% confidence bounds from separate quantile regressions of VAM in math (controlling for yearly experience dummies up to 30 years) on NBER recession indicator at career start at different quantiles. Dashed (grey) line: OLS estimate from Table 2, Column 2. Standard errors adjusted for clustering at the career start year level.

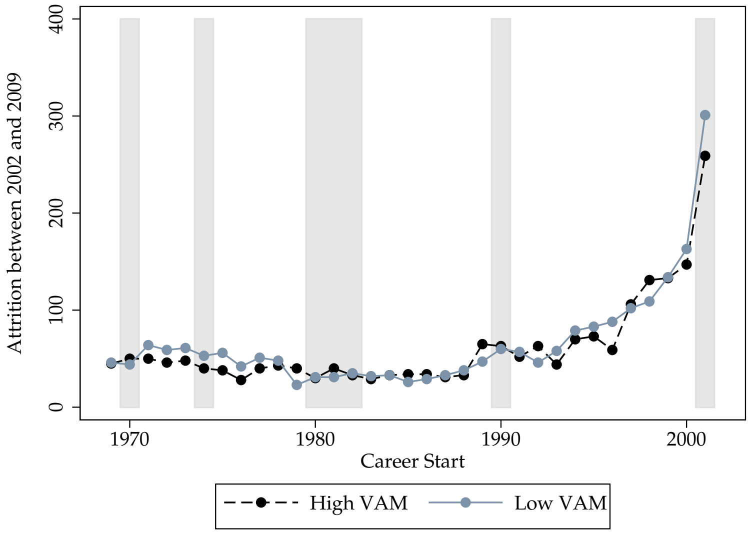
Figure 4: One-Year Unemployment Change and Mean Teacher Math Effectiveness



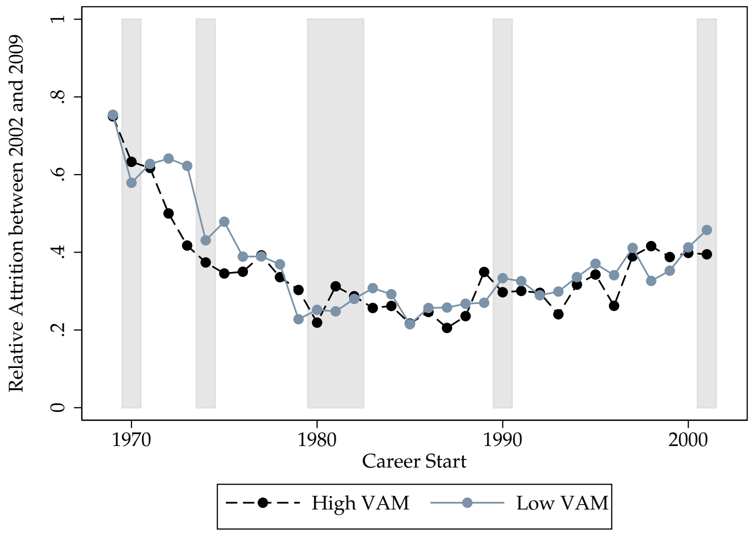
Notes: Cohort means of VAM in math (controlling for yearly experience dummies up to 30 years) and one-year unemployment change for college graduates. Unemployment rates are taken from the BLS. The 2008-09 cohort is excluded as an outlier (unemployment change=2.2, mean experience-adjusted VAM=0.21).

Figure 5: Attrition by Year of Career Start and Teacher Math Effectiveness

a) Absolute Numbers

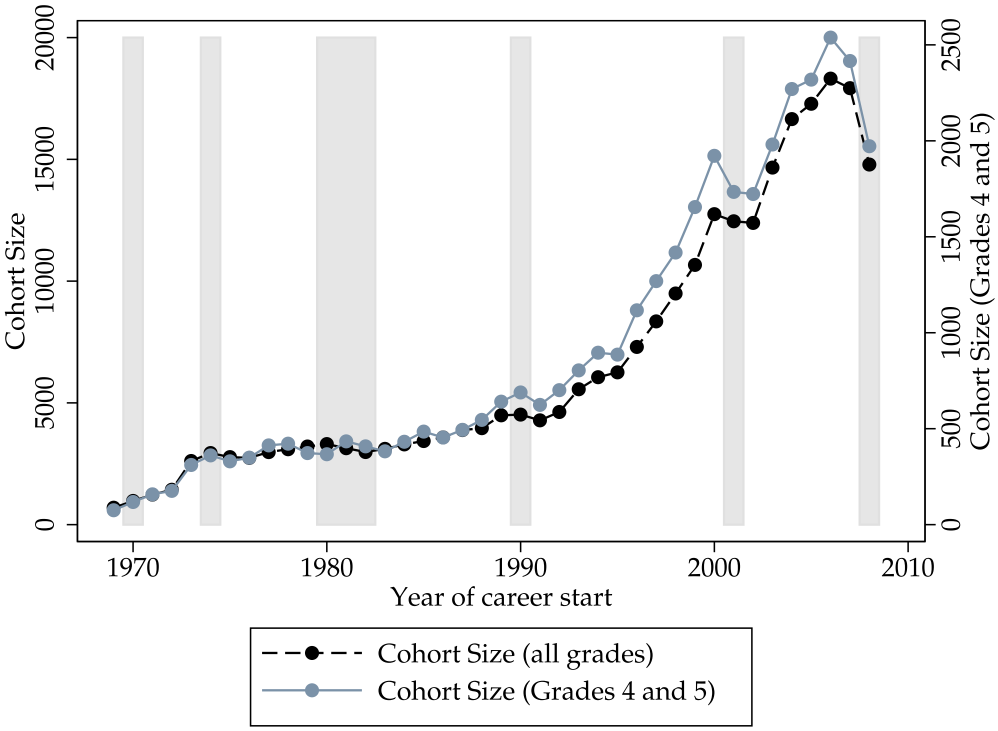


b) Relative to Cohort Size



Notes: Number of teachers observed in 2002 that dropped out of the Florida public school system by 2009 by career start year and teacher value-added in math. *High VAM* refers to teachers with math value-added above median math TVA of teachers observed in 2002 and *low VAM* refers to teachers with math value-added below median math TVA. Panel (b) divides the absolute numbers reported in Panel (a) by the number of teachers of each career start year observed in 2002.

Figure 6: Cohort Size by Year of Career Start



Notes: Cohort size by year of career start of Florida teachers in grades pre-K through grade 12 (left scale) and teachers in grades 4 and 5 (right scale), respectively, observed during school years 2000-01 through 2008-09. Shaded areas are recession periods as defined by the NBER. Appendix Table A7 shows that cohort size is not significantly related to recession at career start.

Table 1: Summary Statistics by Recession Status at Career Start

	Recession	Non-recession	Diff.	p-value	Overall 10th Pct.	Overall 90th Pct.
Unemp. (College)	2.93	2.24	0.69	0.00	1.8	3
Unemp. diff. (College)	0.91	-0.12	1.02	0.00	-0.5	0.8
Size of own cohort (sample)	1310.35	1303.59	6.75	0.99	360	2077
Male	0.12	0.13	-0.01	0.47	0	1
Master's or PhD	0.41	0.37	0.04	0.25	0	1
White	0.71	0.76	-0.05	0.38	0	1
Black	0.15	0.14	0.02	0.11	0	1
Hispanic	0.12	0.10	0.03	0.50	0	0
Experience	10.98	8.11	2.87	0.55	0	24
Exp. >0 outside FL	0.25	0.21	0.03	0.66	0	1
Career Start	1993.06	1996.56	-3.50	0.47	1980	2006
Age at career start	30.30	30.63	-0.33	0.66	23	43
Year of birth	1962.77	1965.94	-3.17	0.46	1949	1980
% black (School)	0.25	0.24	0.01	0.56	0.03	0.68
% free/red. lunch (School)	0.57	0.55	0.02	0.45	0.20	0.87
VAM (math)	0.07	-0.01	0.08	0.04	-1.21	1.22
VAM (reading)	0.04	-0.01	0.05	0.42	-1.22	1.16
Obs.	5,176	27,409			32,585	

Notes: Recession status at career start based on NBER business cycle dates. P-values of t-tests are adjusted for clustering observations at the career start year.

Table 2: Recession at Career Start and Teacher Math Effectiveness

Dependent variable: VAM in math	(1)	(2)	(3)	(4)	(5)
Recession	0.084**	0.111***	0.102***	0.097***	0.112***
	(0.040)	(0.023)	(0.025)	(0.025)	(0.025)
Year of birth			-0.018***	-0.017***	-0.015**
			(0.006)	(0.005)	(0.006)
Age at career start			-0.023***	-0.022***	-0.019***
			(0.005)	(0.005)	(0.005)
Master's or PhD				0.068***	0.069***
				(0.010)	(0.010)
Male				-0.037**	-0.037**
				(0.017)	(0.017)
White				-0.060**	-0.063**
				(0.026)	(0.027)
Teacher cohort size (in 100)					-0.008**
					(0.003)
Experience Dummies	no	yes	yes	yes	yes
Clusters (Career start years)	40	40	40	40	40
Obs. (Teachers)	32585	32585	32585	32585	32585
R^2	0.001	0.022	0.025	0.027	0.028

Notes: Regressions of VAM in math on NBER recession indicator at career start. Experience dummies include yearly experience dummies (up to 30 years). Standard errors in parentheses adjusted for clustering at the career start year level. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table 3: Recession at Career Start and Teacher Reading Effectiveness

Dependent variable: VAM in reading	(1)	(2)	(3)	(4)	(5)
Recession	0.052	0.052***	0.046***	0.043***	0.045***
	(0.064)	(0.016)	(0.014)	(0.014)	(0.015)
Year of birth			-0.012**	-0.012**	-0.012**
			(0.005)	(0.005)	(0.005)
Age at career start			-0.014***	-0.014***	-0.013***
			(0.005)	(0.005)	(0.005)
Master's or PhD				0.040***	0.040***
				(0.013)	(0.013)
Male				-0.137***	-0.137***
				(0.018)	(0.018)
White				-0.029	-0.029
				(0.019)	(0.020)
Teacher cohort size (in 100)					-0.001
					(0.002)
Experience Dummies	no	yes	yes	yes	yes
Clusters (Career start years)	40	40	40	40	40
Obs. (Teachers)	32585	32585	32585	32585	32585
R^2	0.000	0.026	0.027	0.030	0.030

Notes: Regressions of VAM in reading on NBER recession indicator at career start. Experience dummies include yearly experience dummies (up to 30 years). Standard errors in parentheses adjusted for clustering at the career start year level. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table 4: Recession at Career Start and Teacher Math Effectiveness (Subgroups)

	Dependent variable: VAM in math							
Subsample:	Male	Female	Master's/PhD	Bachelor's	White	Non-white	≤ Median age at career start	>Median age at career start
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Recession	0.166***	0.102***	0.104***	0.109***	0.075***	0.163***	0.089***	0.140***
	(0.039)	(0.022)	(0.030)	(0.029)	(0.025)	(0.040)	(0.022)	(0.040)
Clusters (Career start years)	40	40	40	40	40	40	40	40
Obs. (Teachers)	4137	28448	12361	20224	24456	8129	17114	15471
R^2	0.034	0.022	0.012	0.028	0.028	0.023	0.024	0.023

Notes: Coefficients from separate regressions of VAM in math (controlling for yearly experience dummies) on NBER recession indicator at career start for different subsamples. Standard errors in parentheses adjusted for clustering at the career start year level. Median age at career start is 28. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table 5: Robustness Analysis: Recession at Different Points in Life and Teacher Math Effectiveness

	Dependent variable: VAM in math			
Recession at:	(1)	(2)	(3)	(4)
Career Start	0.111***	0.112***	0.102***	0.106***
	(0.023)	(0.025)	(0.024)	(0.023)
Career Start -1 yr.		0.010		
		(0.030)		
Career Start -2 yrs.		-0.006		
		(0.021)		
Career Start -3 yrs.		0.005		
		(0.025)		
Career Start +1 yr.			0.035	
			(0.022)	
Career Start +2 yrs.			-0.012	
			(0.022)	
Career Start +3 yrs.			-0.028	
			(0.027)	
Age 18				0.004
				(0.014)
Age 20				0.010
				(0.018)
Age 22				-0.018
				(0.012)
Age 24				-0.023
				(0.015)
Age 26				-0.028*
				(0.015)
Age 28				-0.024
				(0.017)
Age 30				-0.026
				(0.017)
Age 32				0.017
				(0.018)
Clusters (Career start years)	40	40	40	40
Obs. (Teachers)	32585	32585	32585	29667
R^2	0.022	0.022	0.023	0.020

Notes: Regressions of teacher VAM in math on NBER recession indicator (controlling for yearly experience dummies) at different points in time. The number of observations is lower in the last column because young teachers drop out. Standard errors in parentheses adjusted for clustering at the career start year level. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table 6: Recession at Career Start and Teacher Math Effectiveness (Single Recessions)

	Dependent variable: VAM in math		
Recession year (career start)	(1)	(2)	(3)
1970	0.100**	0.100**	0.091**
	(0.041)	(0.041)	(0.041)
1974	0.020	0.019	0.016
	(0.026)	(0.026)	(0.027)
1980	0.019	-0.004	-0.033
	(0.035)	(0.034)	(0.034)
1981	0.001		
	(0.033)		
1982	-0.033		
	(0.031)		
1990	0.075***	0.075***	0.090***
	(0.016)	(0.016)	(0.010)
2001	0.140***	0.140***	0.138***
	(0.016)	(0.016)	(0.024)
2008	0.268***	0.268***	0.244***
	(0.037)	(0.037)	(0.041)
Included cohorts:	all	all	+/- 2 years around recessions
Clusters (Career start years)	40	40	28
Obs. (Teachers)	32585	32585	21026
R^2	0.023	0.023	0.024

Notes: Regressions of VAM in math (controlling for yearly experience dummies up to 30 years) on separate dummies for cohorts starting during each NBER recession (recession cohorts). In Columns 2 and 3, cohorts entering in 1980 through 1982 are combined. Standard errors in parentheses adjusted for clustering at the career start year level. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table 7: Recession at Career Start and Teacher Math Effectiveness (Subsamples)

	Dependent variable: VAM in math				
	(1)	(2)	(3)	(4)	(5)
Recession	0.111***	0.016	0.151***	0.219***	0.148***
	(0.023)	(0.027)	(0.025)	(0.032)	(0.022)
Subsample:	Baseline	Early cohorts	Late cohorts	Full teacher career observed	No exp. in other state
Clusters (Career start years)	40	20	20	8	40
Obs. (Teachers)	32585	6801	25784	14149	25460
R^2	0.022	0.003	0.025	0.029	0.029

Notes: Regressions of VAM in math (controlling for yearly experience dummies up to 30 years) on recession indicator at career start. Column 1 replicates the baseline estimate. Column 2 only includes teachers who started their career in the first half of our sample and Column 3 only teachers who started their career in the second half of our sample. Column 4 only includes teachers who started their career 2002 or later. Column 5 only includes teachers without any teaching experience outside Florida. Standard errors in parentheses adjusted for clustering at the career start year level. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table 8: Recession at Career Start and Teacher Math Effectiveness (Alternative Business Cycle Measures)

	Dependent variable: VAM in math					
	(1)	(2)	(3)	(4)	(5)	(6)
Recession	0.111***					
	(0.023)					
GDP growth		-0.015**				
		(0.006)				
Unemp. (College)			0.053**			
			(0.022)			
Unemp. diff. (College)				0.084***		
				(0.015)		
Nonagriculture industries					0.041***	
					(0.011)	
Agric. private wage and salary workers						0.016
						(0.011)
Clusters (Career start years)	40	40	40	39	40	40
Obs. (Teachers)	32585	32585	32585	32426	32585	32585
R^2	0.022	0.021	0.021	0.022	0.022	0.021

Notes: Coefficients from separate regressions of VAM in math (controlling for yearly experience dummies) on alternative business cycle measures at career start. Unemployment (college) refers to BLS unemployment rates of college graduates (4 years and above until 1991, degree holders after 1991) and is available from 1970 only. All unemployment rates are from the BLS; GDP growth (2009 constant dollars) from the BEA. Agriculture industries refers to private wage and salary workers. Standard errors in parentheses adjusted for clustering at the career start year level. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

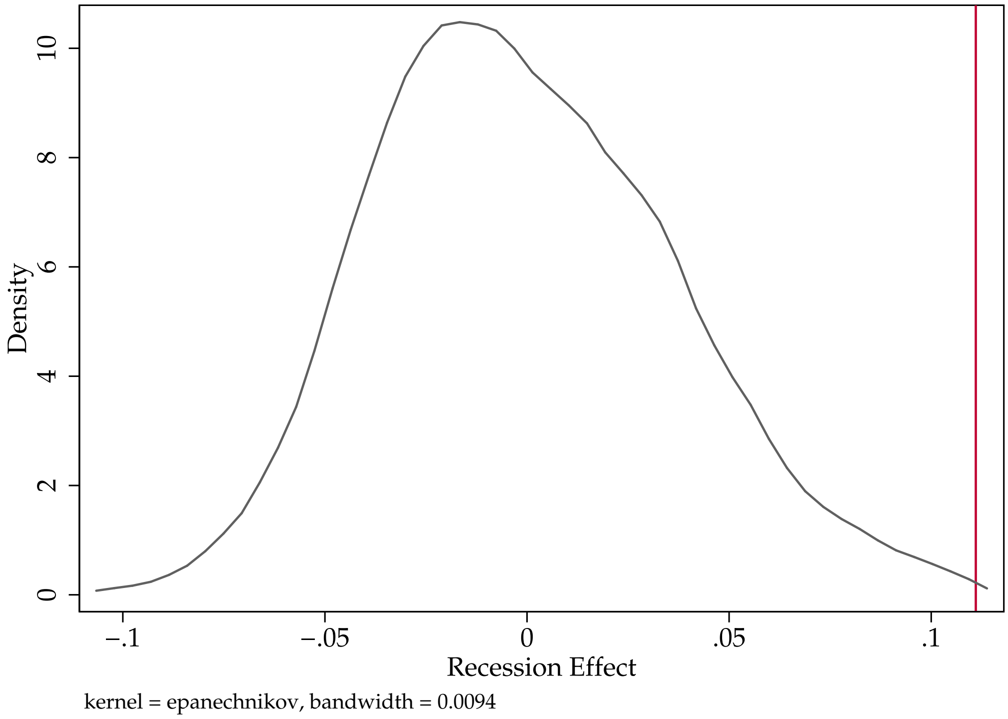
Table 9: Recession at Career Start, Attrition, and Teacher Math Effectiveness

Dependent	Attrition		VAM in math	
variable:	(1)	(2)	(3)	(4)
Recession	0.038	-0.020	0.187***	0.335***
	(0.043)	(0.014)	(0.026)	(0.033)
VAM (math)	-0.013***	-0.028***		
	(0.005)	(0.006)		
Recession*VAM (math)	0.006	0.027***		
	(0.007)	(0.006)		
Career Start	-0.004***	-0.044***		
	(0.001)	(0.004)		
Recession*Experience			-0.008***	-0.066***
			(0.002)	(0.011)
Clusters (Career Start Years)	39	7	40	8
Obs. (Teachers)	32061	13625	32585	14149
R^2	0.010	0.038	0.023	0.031

Notes: Dependent variable: Columns 1 and 2: attrition dummy, which equals 1 if the teacher is not observed during the last school year in our sample period (2008-09) and 0 otherwise; Columns 3 and 4: VAM in math. Columns 3 and 4 also include yearly experience dummies up to 30 years. Standard errors in parentheses adjusted for clustering at the career start year level. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Appendix

Figure A1: Permutation Test



Notes: Kernel density plot of randomization inference test for simulated recession effect on math VAM using 500 repetitions. (Red) vertical line: baseline recession effect from Column 2 in Table 2. For a description of the randomization inference test, see Section 3.2.

Table A1: Regression Estimates from First-Stage and Second-Stage Teacher Value-Added Estimation

	First stage		Second stage	
	Math	Reading	Math	Reading
	(1)	(2)	(3)	(4)
Math score (twice lagged)	0.657***			
	(0.001)			
Reading score (twice lagged)		0.599***		
		(0.001)		
Math scores (lagged)			0.961***	
			(0.001)	
Reading score (lagged)				1.034***
				(0.002)
Student male	0.037***	–0.031***	–0.029***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Student black	–0.234***	–0.204***	–0.071***	–0.018***
	(0.005)	(0.005)	(0.005)	(0.006)
Student Hispanic	–0.133***	–0.096***	–0.024***	–0.002
	(0.005)	(0.005)	(0.005)	(0.006)
Student American Indian	–0.111***	–0.090***	–0.050***	0.004
	(0.014)	(0.014)	(0.013)	(0.015)
Student mixed ethnicity	–0.140***	–0.082***	–0.043***	–0.021***
	(0.006)	(0.006)	(0.006)	(0.007)
Student white	–0.120***	–0.074***	–0.046***	–0.007
	(0.005)	(0.005)	(0.005)	(0.005)
Student: free-lunch eligible	–0.080***	–0.089***	–0.004**	–0.004*
	(0.002)	(0.002)	(0.002)	(0.002)
Student: reduced-lunch eligible	–0.041***	–0.051***	0.001	–0.005**
	(0.002)	(0.002)	(0.002)	(0.003)
Student: limited English proficiency	–0.098***	–0.185***	0.069***	0.122***
	(0.005)	(0.004)	(0.004)	(0.005)
Student: special education	–0.111***	–0.141***	0.004*	0.031***
	(0.003)	(0.003)	(0.002)	(0.003)
Class size	–0.001***	–0.001***	–0.001***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Class: lagged math score	0.650***	–0.092***	–0.428***	0.178***
	(0.005)	(0.005)	(0.005)	(0.005)
Class: lagged reading score	–0.160***	0.638***	0.241***	–0.402***
	(0.005)	(0.005)	(0.005)	(0.006)
Class: % male	–0.087***	0.052***	0.070***	–0.058***

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Table A1 – continued from previous page

	First stage		Second stage	
	Math	Reading	Math	Reading
	(1)	(2)	(3)	(4)
	(0.009)	(0.009)	(0.008)	(0.010)
Class: % black	0.338***	0.192***	–0.233***	–0.129***
	(0.028)	(0.028)	(0.027)	(0.030)
Class: % Hispanic	0.194***	0.083***	–0.155***	–0.069**
	(0.028)	(0.027)	(0.026)	(0.030)
Class: % American Indian	0.184**	0.111	–0.203***	–0.105
	(0.072)	(0.071)	(0.068)	(0.078)
Class: % mixed ethnicity	0.173***	0.021	–0.122***	–0.009
	(0.033)	(0.032)	(0.031)	(0.036)
Class: % white	0.142***	0.012	–0.118***	–0.017
	(0.026)	(0.026)	(0.025)	(0.029)
Class: % reduced-lunch eligible	0.142***	0.147***	–0.078***	–0.098***
	(0.008)	(0.008)	(0.008)	(0.009)
Class: % reduced-lunch eligible	0.071***	0.089***	–0.029**	–0.068***
	(0.013)	(0.012)	(0.012)	(0.014)
Class: % limited English proficiency	0.239***	0.373***	–0.002	–0.178***
	(0.016)	(0.015)	(0.015)	(0.017)
Class: % special education	0.162***	0.231***	–0.093***	–0.122***
	(0.011)	(0.011)	(0.011)	(0.012)
School: % black	–0.146**	–0.213***	–0.389***	–0.036
	(0.064)	(0.063)	(0.060)	(0.069)
School: % white	–0.171***	–0.223***	–0.356***	–0.041
	(0.062)	(0.061)	(0.058)	(0.067)
School: % Hispanic	–0.124*	–0.180***	–0.413***	–0.050
	(0.065)	(0.064)	(0.061)	(0.070)
School: % free-lunch eligible	0.026	0.038**	0.093***	–0.002
	(0.017)	(0.017)	(0.016)	(0.018)
School: % reduced-lunch eligible	–0.077*	–0.088**	–0.088**	0.120**
	(0.045)	(0.044)	(0.042)	(0.049)
School enrollment	–0.000	–0.000	–0.000***	–0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Year fixed effects	X	X	X	X
Urban indicators (8 categories)	X	X	X	X
Observations	615,751	622,286	615,751	622,286

Notes: Dependent variable: Column 1: lagged math score; Column 2: lagged reading score; Column 3: current math score; Column 4: current reading score. These models are estimated using data on 5th-grade students. The reference category of student's ethnic group is Asian students. Standard errors in parentheses. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table A2: Recession at Career Start and Teacher Effectiveness: Wild Cluster Bootstrap

Dependent variable:	VAM in math			VAM in reading		
	(1)	(2)	(3)	(4)	(5)	(6)
Recession	0.111			0.052		
	(0.023)***			(0.016)***		
	[0.000]***			[0.019]***		
Unemp. (College)		0.053			0.018	
		(0.022)**			(0.012)	
		[0.027]**			[0.011]*	
Unemp. diff. (College)			0.084			0.038
			(0.015)***			(0.011)***
			[0.000]***			[0.015]***
Experience dummies	yes	yes	yes	yes	yes	yes
Clusters (Career start years)	40	40	39	40	40	39
Obs. (Teachers)	32585	32585	32426	32585	32585	32426
R^2	0.022	0.021	0.022	0.026	0.026	0.026

Notes: Regressions of teacher value-added in math (Columns 1–3) and reading (Columns 4–6) on different recession measures. All specifications control for yearly experience dummies. Standard errors reported in parentheses allow for clustering at the career start year level. Unemployment measures for college graduates are available from 1970 only. Standard errors reported in brackets use wild cluster bootstrap (using Stata's *cgmwildboot* command) and 400 repetitions. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table A3: Recession at Career Start and Teacher Math Effectiveness (Quantile Regressions)

	Dependent variable: VAM in math						
Recession	0.028	0.096***	0.099***	0.088***	0.119***	0.143***	0.178***
	(0.038)	(0.028)	(0.032)	(0.026)	(0.027)	(0.025)	(0.037)
Quantile	5	10	25	50	75	90	95
Obs. (Teachers)	32585	32585	32585	32585	32585	32585	32585
R^2	0.020	0.021	0.022	0.022	0.021	0.015	0.008

Notes: Coefficients from separate quantile regressions of VAM in math (controlling for yearly experience dummies up to 30 years) on NBER recession indicator at career start at different quantiles of the VAM distribution. Standard errors in parentheses adjusted for clustering at the career start year level. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table A4: Unemployment at Career Start and Teacher Math Effectiveness

	Dependent variable: VAM in math					
	(1)	(2)	(3)	(4)	(5)	(6)
Unemp. (College)	0.053**					
	(0.022)					
Unemp. diff. (College)		0.084***				
		(0.015)				
Unemp. (Nat.)			0.034***			
			(0.010)			
Unemp. diff. (Nat.)				0.047***		
				(0.012)		
Unempl. (FL)					0.028***	
					(0.008)	
Unempl. diff. (FL)						0.024**
						(0.011)
Clusters (Career start years)	40	39	40	40	40	40
Obs. (Teachers)	32585	32426	32585	32585	32585	32585
R^2	0.021	0.022	0.022	0.022	0.022	0.021

Notes: Coefficients from separate regressions of VAM in math (controlling for yearly experience dummies up to 30 years) on alternative business cycle measures at career start. Columns 1 and 2 replicate the results of Columns 3 and 4 of Table 8. Unemployment (college) refers to BLS unemployment rates of college graduates (4 years and above until 1991, degree holders after 1991) and is available from 1970 only. The other unemployment rates are not graduate-specific. All unemployment rates are from the BLS. Standard errors in parentheses adjusted for clustering at the career start year level. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table A5: Unemployment at Career Start and Teacher Math Effectiveness (w/o NBER recessions)

	Dependent variable: VAM in math					
	(1)	(2)	(3)	(4)	(5)	(6)
Unemp. (College)	0.013					
	(0.021)					
Unemp. diff. (College)		0.036**				
		(0.018)				
Unemp. (Nat.)			0.023**			
			(0.010)			
Unemp. diff. (Nat.)				0.033***		
				(0.010)		
Unempl. (FL)					0.016**	
					(0.008)	
Unempl. diff. (FL)						0.009
						(0.009)
Clusters (Career start years)	32	31	32	32	32	32
Obs. (Teachers)	27409	27250	27409	27409	27409	27409
R^2	0.026	0.026	0.026	0.026	0.026	0.026

Notes: Coefficients from separate regressions of VAM in math (controlling for yearly experience dummies up to 30 years) on alternative business cycle measures at career start. All specifications exclude teacher cohorts who started during recessionary periods as defined by the NBER. Unemployment (college) refers to BLS unemployment rates of college graduates (4 years and above until 1991, degree holders after 1991) and are available from 1970 only. The other unemployment rates are not graduate-specific. All unemployment rates are from the BLS. Standard errors in parentheses adjusted for clustering at the career start year level. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table A6: Recession at Career Start and Teacher Math Effectiveness (Alternative VAMs)

	Dependent variable: Various VAMs in math						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Recession	0.111***	0.092***	0.059***	0.069**	0.092***	0.083***	0.111***
	(0.023)	(0.023)	(0.017)	(0.029)	(0.029)	(0.027)	(0.023)
Fixed Effects (TVA)	none	school	school-year	student	none	none	none
Weights	none	none	none	none	student obs.	teacher obs.	none
IV (Instrumenting the lagged test score)	yes	yes	yes	yes	yes	yes	no
Clusters (Career start years)	40	40	40	40	40	40	40
Obs. (Teachers)	32585	32585	32585	32585	32585	32585	32585
R^2	0.022	0.018	0.015	0.012	0.020	0.020	0.022

Notes: Coefficients from separate regressions of different VAMs in math (controlling for yearly experience dummies up to 30 years) on NBER recession indicator at career start. Column 1 replicates the baseline recession effect from Column 2 in Table 2. Standard errors in parentheses adjusted for clustering at the career start year level. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.

Table A7: Recession at Career Start and Teacher Cohort Size

Panel A: Dependent variable: Cohort Size						
	(1)	(2)	(3)	(4)	(5)	(6)
Recession	-209.688	-52.689	-85.578	-1208.344	192.574	-297.564
	(262.767)	(227.509)	(236.923)	(1964.757)	(664.140)	(604.448)
Trend		45.672***	-4164.877*		407.540***	-62341.26***
		(6.849)	(2444.482)		(33.234)	(8523.72)
Sq. Trend			1.059*			15.777***
			(0.616)			(2.145)
Sample	Sample	Sample	Sample	All	All	All
Obs.	40	40	40	40	40	40
R^2	0.018	0.704	0.744	0.009	0.819	0.949

Panel B: Dependent variable: Log Cohort Size						
	(1)	(2)	(3)	(4)	(5)	(6)
Recession	-0.313	-0.110	-0.110	-0.221	0.011	0.006
	(0.283)	(0.210)	(0.199)	(0.329)	(0.099)	(0.104)
Trend		0.059***	0.092		0.068***	-0.595
		(0.006)	(2.184)		(0.004)	(1.646)
Sq. Trend			-0.000			0.000
			(0.001)			(0.000)
Sample	Sample	Sample	Sample	All	All	All
Obs.	40	40	40	40	40	40
R^2	0.028	0.858	0.858	0.012	0.924	0.924

Notes: Regressions of teacher cohort size on the binary recession at career start indicator, using data collapsed at the career start year level. Dependent variable: (absolute) cohort size in Panel A; log cohort size in Panel B. Columns 1–3 are based on all teachers in our estimation sample; Columns 4–6 includes all teachers (i.e., teachers in grades pre-K through 12) who are observed during the school years 2000-01 to 2008-09 in the administrative data of the Florida Department of Education. *Sq. Trend* denotes the squared career start year. Robust standard errors in parentheses. Significance levels: *** p< 1%, ** p< 5%, * p< 10%.