Human Capital Depreciation*

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Abstract

Human capital can depreciate over time if skills are unused, and such depreciation may be a primary cause of structural duration dependence in non-employment. But measuring human capital depreciation is challenging, as worker skills or output are difficult to measure and less productive workers are more likely to spend time in non-employment. We overcome these challenges by using new data on teachers' assignments and their students' outcomes. In Greece, all education graduates are guaranteed public sector teaching positions; however, positions are typically not immediately available for new graduates. Teachers are thus quasi-randomly assigned to waitlists by degree conferral date, generating variation in time spent without formal employment. We find significant losses to output, as a one year increase in time without formal employment, and the associated forgone experience, leads to a 0.10 student standard deviation (2.2%) decline in students' average test scores in our micro empirical specification and a 0.11 student standard deviation (4.0%) decline in a district specification. To understand what channel drives this loss, we control for experience levels and isolate the effect of depreciation. We find that skill depreciation explains the full effect.

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1 Introduction

Human capital – the knowledge and skills of workers – is a key factor driving economic growth in the aggregate and labor market outcomes at the individual level. Human capital can be developed at home within the family, through formal education, and through labor market experience (Becker, 1962, 1964). But if acquired skills are not actively used, they may depreciate over time. Thus, high rates of skill depreciation may amplify the costs to unemployment or labor force detachment by lowering a worker's future productivity.

Indeed, the literature has found evidence of structural non-employment duration dependence, where the probability of callbacks, probability of re-employment, and wages upon re-employment decline in the length of the non-employment spell (e.g., Kroft, Lange, and Notowidigdo (2013), Autor, Maestas, Mullen, and Strand (2015), Jacobson, LaLonde, and Sullivan (1993)). Whether this duration dependence derives from skill depreciation or other explanations, such as stigma, changes to reservation wages (Schmieder, von Wachter, and Bender, 2016), or changes to match quality (Neal, 1995), is important for optimal policy design. For instance, if unemployment costs are driven by human capital depreciation, then policies that maintain part-time or temporary employment or provide structured activities that require using job-related skills might be particularly effective. Such policies could include part-time working subsidies,[1] public works programs, or even banning non-compete clauses. Furthermore, high degrees of skill depreciation imply particularly large costs from increases in aggregate non-employment through a depletion in aggregate levels of human capital. And yet, despite their policy importance, well-identified empirical estimates of skill depreciation rates remain elusive.

This paper studies how human capital changes with time spent without formal employment. To do so, we compile a new dataset on teachers in Greece and their assignments and exploit an institutional feature that quasi-randomly assigns time spent in formal employment. We find that an additional year without formal employment, with the associated forgone experience, leads to a 0.10 student standard deviation (σ) (2.2%) decline in a teacher's student test scores. Using aggregated data covering the entire country, we estimate a district's student test scores fall by 0.11σ (4.0%) if the teachers' mean time without formal employment increases by a year.

There are two key challenges in measuring the changes in human capital due to non-employment.

[1]For example, the Germany Kurzarbeit scheme subsidizes firms for part-time employment.

First, in many contexts worker productivity is difficult to measure, especially over time.[2] Second, even if productivity can be measured, unproductive workers are less likely to receive job offers, and hence are likely to spend more time not working. This may generate a negative correlation between time spent without employment and productivity, even if time without employment does not directly affect productivity.

We overcome these challenges by studying teachers in Greece. We address the first identification challenge by focusing on employees for whom we have a direct measure of output. Following a large literature (Rockoff, 2004; Chetty, Friedman, and Rockoff, 2014a), we infer teacher productivity based on her students' test scores. We use students' results on the Panhellenic Examinations, national exams that all Greek high school students take in grade 12, and which are the primary determinants of university enrollment.

We address the second empirical challenge by exploiting the unique system of teacher assignments in Greece. Individuals who graduate in good standing with an education degree are guaranteed a public sector teaching position. In nearly all years, however, there are not enough positions immediately available, and thus graduates enter long waitlists that determine assignments. By law, all university graduates are assigned waitlist rankings in order of their date of degree conferral. Because small differences in degree conferral dates are driven by heterogeneous course schedules, timing of oral defenses, and bureaucratic delays, we argue that within a degree conferral month-year, remaining variation is exogenous. This quasi-randomness in waitlist position can translate to considerable variation in how long similar teachers wait for assignments to formal positions.[3]

In addition to the data on national test scores, we compile novel data from the Greek Ministry of Education on the universe of Greek deputy teacher waitlist rankings and assignments between 2004 and 2011. The assignments designate the school district the teacher is assigned to for the following year. We supplement this with hand-collected data from 23 high schools that includes student-teacher assignments for each course and test scores by subject for all high school grades.

Our main specification relates a teacher's students' test scores to the accumulated number of years the teacher spent without formal employment. To address the concern that years without

[2]We use teacher productivity and ability interchangeably to describe the amount of output teachers produce in their students. See Bertrand and Schoar (2003) and Zivin and Neidell (2012) for a discussion on the challenges of estimating manager and worker productivity.

[3]Teachers lose waitlist eligibility if they take full-time employment. Because some teachers may work in the informal sector while waiting, we refer to our estimates as human capital changes from time without *formal* employment.

formal employment may be correlated with a teacher's "potential" productivity,[4] we control for the month-year in which the teacher earned her degree and further instrument for years without formal employment with *initial* waitlist rank.[5] Our estimates indicate significant loss in productivity from not working formally: 0.10σ decline in test scores per year.

To extend the analysis beyond these 23 schools, we employ a second specification where we aggregate to the school district level and estimate the effect of the labor force's average time spent without formal employment on test scores. Our estimates are very similar to the estimates from the teacher-level specification: a combined depreciation and forgone experience effect of 0.11σ.

Our identification strategy relies on the initial waitlist position, conditional on degree month-year, being orthogonal to (1) teacher potential productivity and (2) unobserved ability of a teacher's assigned students. We assess the validity of our identification strategy in several ways. First, we find no evidence of attrition related to initial waitlist position, conditional on degree month-year. Second, we find no evidence that teachers' initial waitlist position, conditional on degree month-year, correlates with the teacher's university achievement. Third, we find no statistical relationship between the mean (conditional) waitlist rank of the teachers assigned to a district and district characteristics like unemployment rates or class size. Finally, we repeat our analysis using the time without formal employment of a district's teachers in subjects that do not appear on the national tests. We find no relationship between their mean time without formal employment and students' test scores.

Our analysis pools comparisons of teachers at different experience levels and with different non-employment spell lengths. When we repeat our analysis on subsamples, we find the largest drops in student test scores among districts whose teachers have had shorter spells of without formal employment while we find similar effects across the distribution of past experience.[6] That productivity losses concentrated at the beginning of the spell is important for optimal design of unemployment benefits, which depends on how expected consumption upon reemployment changes throughout the spell (Chetty, 2006).

[4]We refer to "potential" productivity as the teacher's productivity had she been continuously employed as a public school teacher. We distinguish this from realized productivity, which may depend on how long a teacher has been without formal employment.

[5]We use initial rank as an instrument because, as we describe in Section 2, a teacher's waitlist rank evolves in the years since graduation, sometimes for reasons related to potential productivity.

[6]As experience and spell length are endogenous, we form our subsamples based on the expected experience or spell length among teachers that graduated in the same month-year.

The main estimates capture the combined effect of skill depreciation from not working in the formal sector plus forgone skill appreciation that would have accrued with experience. We decompose the effects of these channels by comparing teachers with the same levels of prior experience but who have exogenously waited different amounts of time for their assignments.[7] We estimate an annual skill depreciation rate of 0.23σ (6.5%) in the micro student specification and 0.13σ (4.8%) in the macro district specification. These imply that the combined effects are driven entirely by skill depreciation.

This paper helps delineate the specific mechanism that generates structural unemployment duration dependence. An extensive literature has documented the duration dependence of non-employment for callback rates (Kroft, Lange, and Notowidigdo (2013), Farber, Silverman, and von Wachter (2017)), wages upon re-employment (Jacobson, LaLonde, and Sullivan (1993), Card, Chetty, and Weber (2007), Centeno and Novo (2012), Schmieder, von Wachter, and Bender (2016)), and re-employment (Autor, Maestas, Mullen, and Strand, 2015). By focusing on a profession where output is observable, our paper distinguishes changes in productivity as a cause of duration dependence from other explanations, primarily stigma. Several papers have moved beyond wages to estimate effects on skills or output. Edin and Gustavsson (2008) estimates the effect of unemployment duration on skill measures, using a panel data fixed effects approach. Benhenda (2017) looks at teachers' absences and their effects on output. Our contribution is to combine output measures with quasi-random variation in non-employment that is robust to time-varying shocks.

The paper also contributes to a literature on human capital and productivity by estimating a key parameter used in many structural models in macroeconomics, labor, and finance, many of which study duration dependence (e.g., Alvarez, Borovičková, and Shimer (2016)). Comparing estimates of depreciation across these models is complicated as the form of depreciation is often specific to the model. Keane and Wolpin (2001) estimate a large amount of depreciation (31%) for white collar workers changing industries. Blundell, Dias, Meghir, and Shaw (2016) estimate human capital depreciation rates of 6-8%. Macroeconomic models incorporating human capital depreciation include Ljungqvist and Sargent (1998) (20% chance of losing skills) and Jarosch (2015) (15% depreciation rate). We offer a depreciation rate estimate that leverages quasi-random variation.

[7]These teachers will mechanically be of different ages on average. We discuss the role of age effects in Section 7.

Our work also contributes to a literature in public finance and labor economics, by estimating a parameter relevant to evaluating the effects of many policies. For example, there is a large literature on optimal unemployment insurance (Baily, 1978; Chetty, 2006; Shimer and Werning, 2006), which derives optimal benefit levels by trading off consumption smoothing benefits with changes to labor supply. If human capital depreciates when individuals are not working, this effect can affect optimal benefit levels. Non-compete clauses may also have harmful effects in the presence of human capital depreciation (Marx, Strumsky, and Fleming, 2009). Countries such as Germany also incentivize employers to reduce hours rather than lay off workers. Giroud and Mueller (2017) discuss how these policies led to lower unemployment during the Great Recession, with substitution in hours worked. These policies may have positive effects on human capital accumulation if they prevent deterioration.

Finally, in the most narrow but direct sense this paper joins a literature on teacher productivity. A number of papers study teacher productivity (e.g. Rockoff (2004)), and effects of teacher productivity on later life outcomes (Chetty, Friedman, and Rockoff, 2014a,b). Jackson, Rockoff, and Staiger (2014) provide a review of the literature on teacher productivity. This paper shows that time spent without formal employment has a significant impact on teacher productivity, as measured by student outcomes.[8]

The remainder of this paper is organized as follows. Section 2 discusses the institutional details that form the basis for our empirical strategy. Section 3 discusses the data used in our analysis. Section 4 introduces our empirical strategy. Section 5 presents the main empirical results at the student-level and then extends the analysis to the larger sample and conducts a district analysis. Section 6 supplies robustness checks and heterogeneous effects, and Section 7 decomposes the effects into skill depreciation and forgone returns to experience. Section 8 concludes and discusses avenues for future research.

[8] The next draft will include a more complete literature review.

2 Institutional Details

2.1 Types of Teacher Assignments in Greece

The education system in Greece appoints teachers to either permanent or temporary positions. Permanently appointed teachers ("permanent teachers") are considered to be civil servants and once hired they enjoy job security. They have the option to renew their contract each year at the same school. Teachers appointed to temporary positions ("temporary teachers") are employed on a contract basis for up to ten months and they have to re-apply for a new short-term appointment. Even if they receive an assignment in the following year, it will almost certainly be at a different school. These temporary teachers, formally called substitute or deputy teachers, can be either *full-time*, teaching 16-23 hours per week at a standardized salary,[9] or *hourly*, teaching up to 4 hours per week at a standardized hourly rate.

Schools may request a deputy teacher when there is a shortage of teaching staff. This occurs through retirements among permanent teaching staff, teachers taking long-term unpaid leaves (maternity, pregnancy, post-graduate studies, serious illness, or temporary moves abroad), or unexpected demand shocks. For example, if a school's enrollment increases more than expected, the school may request a deputy teacher to cover the additional class. Most temporary teacher assignments occur in September or October, but some of these events require mid-year assignments.

The fraction of teachers in temporary assignments has grown considerably over the last two decades. Between 2011 and 2015, there was a 35% increase in the number of deputy teachers who were employed by schools, such that now 15-20% of the teacher workforce are on temporary contracts (OECD, 2018). This share varies by district. Temporary teachers might be the minority in schools in affluent urban neighborhoods, but often dominate in small and remote areas, especially in the islands (OECD, 2018).

There are at least two reasons behind this trend. First, budgetary pressures since the financial crisis in 2008 have increased the use of temporary staff to cover teaching needs. As civil servants, permanent teachers count as a long-term liability to the national budget. In an attempt to reduce these committed expenditures, the European Commission agreed that European structural funds

[9] In practice, nearly all full-time deputy teachers teach the maximum number of hours (i.e., 23 hours per week). Less commonly, a full-time deputy teacher could agree to work between 5 and 15 hours per week. These teachers get monthly prorated payments.

could be used to cover the salaries of temporary teaching staff in Greece and other European countries (OECD, 2018). In practice, these expenditures do not represent salaries, but payments for educational services.[10]

Second, since 2009 Greece has had a hiring freeze in permanent staff. As hiring new temporary teaching staff has became the only way to cover teaching needs in schools, an increasing number of teachers has been hired on a contract basis. In particular, according to the Law No. 3255/2004,[11] new temporary teachers are hired through two different processes: (a) 60% of the new teachers are hired through an examination system (ASEP)[12] while (b) 40% of the new teachers come from centralized waitlists which are compiled annually.

2.2 How the System Determines Assignments from Waitlists

University graduates with a degree in education prior to 2011 were entitled to a teaching position in a Greek public school. Graduates, however, have to wait until a position opens in their academic subject. For each subject, there are two main types of waitlists for teachers depending on teachers' seniority.[13]

The first list is for fresh university graduates with no prior teaching experience. Each year fresh graduates are added to the ends of the waiting lists according to their exact date of degree conferral. While rare, if students share the same date of degree conferral, ties are broken in favor of the teacher with the higher university grades. Unlike other higher education systems where a university's graduates receive their degrees on the same day, in Greece degree conferral occurs once the pivotal course's grade is entered. The pivotal course is often a student-teaching assignment or involves a written thesis or oral defense. Heterogeneous course schedules, grading congestion,

[10]Thus, temporary teachers do not get paid during the summer, unlike permanent teachers.

[11]No. 3255/2004, Article 5, Paragraph 2a. The law can be found here: `https://www.e-nomothesia.gr/kat-ekpaideuse/n-3255-2004.html`.

[12]The ASEP examination system was introduced in 1997 (Law No. 2525/1997) to guarantee permanent positions to teachers that scored the highest on assessments that tested subject-specific and general pedagogic knowledge (Stylianidou, Bagakis, and Stamovlasis, 2004). The ASEP examination took place every two years until 2008, the last time it was offered (OECD, 2018).

[13]To be eligible for the waitlists, the following conditions must be met: (a) the applicants must be either Greek or from North-Epirus or Greeks from Constantinople/Istanbul and from the islands of Imvros and Tenedos (Law No. 3832 / 1958) or European Union citizens (Law No. 2431/1996), (b) male applicants must present a military certificate that shows that they have served their compulsory military service or a certificate that shows that the applicant has a military exemption, and (c) expatriates from Cyprus, Egypt, Turkey and North-Epirus must submit a birth certificate and a certificate to the Ministry certifying that they are Greeks. There is no age restriction.

and bureaucratic delays lead students to finish their degrees on different days, and this generates considerable variation in when students graduate, as seen in Figure 1. The left panel is a histogram of the month of the year in which teachers earn their degrees while the right panel is a histogram of the day of the month. Some months and days produce more degrees than others, but there is still extensive variation across and within month.

Once a teacher rises to the top of the list, the next time a school requests a temporary teacher, the position is offered to this teacher. The lists do not distinguish geographically, so the offered position may be anywhere in the country. The teacher has a week to file the requisite paperwork to accept the position.

After teachers complete their first temporary assignment, via the fresh graduates list, they enter the second subject-specific list, which consists of teachers who have some prior teaching experience or have taken a written assessment (ASEP). The experienced teacher waitlist is ordered by a teacher's accumulated number of credits (having more credits is better), which teachers collect based on their prior experience, score in ASEP, and other factors.[14] Once teachers rise to the top of the list, get assigned a temporary teaching position, and complete it, they earn additional credits and return to the experienced teachers waitlist for the next temporary assignment.

The time waiting for an assignment depends on the teacher's waitlist position, the length of the list, and the number of openings. We provide more extensive descriptive statistics in Section 3, but typical wait times during our sample period were several years (Tsakloglou and Cholezas, 2005). In recent years, the supply of teachers has outpaced demand, as expected wait times now reach 20 years.[15] These controversially long wait times have led to large protests and politicians have recently proposed changing the system.[16]

[14]Teachers earn 1 teaching credit for each month of prior teaching in a school that is located in an urban area, but they earn 2 teaching credits per month of prior teaching in remote areas and islands. They also collect credits based on their marital status and the number of children that they have. Job performance for prior teaching does not alter credits.

[15]See: http://www.ekathimerini.com/236797/article/ekathimerini/news/teachers-glut-causing-bottleneck-in-hirings.

[16]See: https://www.euronews.com/2019/01/14/over-3-000-greek-primary-school-teachers-clash-

2.3 Prospective Teachers' Actions

As part of this process, prospective teachers with university degrees in education have several decisions. While they wait for an assignment, teachers may find alternate ways to generate income.[17] Importantly, these activities may not include taking a full-time job, which would remove a teacher's public school teaching eligibility.[18] Greece's active informal employment sector means that some teachers may still take full-time jobs that are hidden from the government. We are in the process of conducting a survey of teachers about their activities while waiting for assignment. Anecdotally, prospective teachers describe spending their time by offering part-time private tutoring, teaching in night cram schools, working part-time as a restaurant waiter, getting more formal education, or starting a family. We will thus interpret our estimates as capturing the potential loss of skills relevant to a worker's desired profession (teaching) when the worker is unable to work formally in her desired profession. This, notably, does not rule out the acquisition of skills relevant for other professions nor does it preclude all activities that might use the same skills that matter for formal full-time employment.

If the prospective teacher takes full-time formal employment or notifies the government that she no longer wants to be considered for temporary public school teaching positions, then she will no longer appear on the lists. We will explore attrition in more detail in Section 4 as selective attrition based on a teacher's productivity would violate our identification assumptions.

Finally, if offered a position the teacher finds unacceptable, she may reject it. Rejection, however, is quite costly. The teacher is placed at the end of the waitlist and ineligible for any assignments in the following two years. We believe rejections are rare, but if common and selective, they would prove problematic for our identification strategy.

This institutional setting has several features that might produce high skill depreciation. First, because nearly all full-time teaching positions are in the formal sector, there are few available ways to spend one's waiting time that approximate the desired job. Second, teachers face limited financial or professional incentives to perform well once assigned. Student test scores or other outcomes do not factor into any form of teacher evaluation. Future assignments – both where and when – do not depend on performance nor does financial compensation. Thus, the incentives to

[17]Waiting teachers are not eligible for unemployment insurance.

[18]This includes teaching at private schools, though the Greek private education sector is small at 7% enrollment share.

insure against or take investments to counteract depreciation are limited, and the available tools are directly restricted.

The nature of work experience also stands out in this setting. On-the-job learning may involve general and firm-specific skills. Here, because each temporary position lasts less than a year and reassignment to the same school is exceedingly rare, the returns to experience may be limited as school-specific skills generate no future return. Furthermore, the short-term nature of the national assignment process means that each year of experience also potentially involves a costly relocation to a different part of the country. Finally, unlike many settings with time spent not working, here there is no need to search for a job, at least in the desired profession. While there is some uncertainty about when a position will be available, the teacher's own actions do not affect it.

3 Data

To conduct our analysis, we compiled national data on teacher waitlists, teacher assignments, and student test scores. We supplement these data sets with comprehensive data from 23 high schools that includes student-teacher linkages for each course taken.

3.1 Teacher Waitlists

The Ministry of Education compiles teacher waitlists centrally and maintains an online archive in which it posts some waitlists from prior years. We tracked down the archives and constructed the waitlists for deputy and hourly high school teachers from 2003 – 2011. Each year includes separate lists for each teaching subject and for fresh graduates versus experienced teachers. A teacher may appear on both the deputy and hourly lists, corresponding to her experience level, in the same year. The waitlists include each teacher's position on the list plus any characteristic or outcome that determines the waitlist order. Importantly, this includes when a teacher's university degree was conferred and teaching experienced accrued in each year. We restrict our sample to teachers who earned their university degrees prior to 2006, as our identification strategy, described in Section 4, will rely on cohorts where at least some members reach a second assignment in our sample period.

Table 1 displays summary statistics for the waitlist data for teachers in subjects that appear on the national examinations. The waitlists grow over time, with teachers appearing on the waitlists for an average of 2.9 years in our sample. The average waitlist spell lasts 2.6 years and 37% of the observations come from lists for new graduates (inexperienced teachers). Teachers have an average of 1.5 years of prior experience, and almost no teachers have any experience at private schools, as permanent teachers, or in other EU countries. Teachers of Greek and history comprise the majority of the list, with the rest are split between math and statistics, physics and biology, economics, and computer science.

In Figure 2 we show the full distribution of total (left) or consecutive (right) years without formal employment for our sample of teachers that appear on the waitlists between 2003 and 2011. Most teachers wait at least two years, with waits of 3-4 years being common. While subsequent assignments beyond the first tend to occur more quickly, the distribution of consecutive years without formal employment shows that total time without formal employment consists of a few long spells rather than many short spells.

3.2 Teacher Assignment Data

The Ministry of Education and the school districts collect information on teachers' temporary assignments to high schools around the country. They publicly announce these lists to inform teachers about their assignments, but also for transparency reasons. These assignments come from both teachers' waitlists – the fresh graduates waitlist and the experienced teachers' waitlists – and are usually announced in September or October based on schools' needs.

We obtained the assignment lists from the online archives of the Ministry of Education and school district authorities. These lists contain an assigned teacher's name, teaching categorization based on the subjects that they teach,[19] and the school district that the assignment school belongs to. We obtained these lists for each year in the academic years 2004 – 2011, with an average of 2,594 high school teachers assigned per year. Unless a district has only a single high school, we do not know the exact school the teacher was assigned to. But districts are fairly small. The 604 districts in our sample have an average of 2.3 high schools.

[19]There are several categorizations based on the subjects that teachers teach. Teachers obtain these specializations during their university undergraduate studies and they have to report their specialization when they enter these lists. For example, PE01 teachers specialize in teaching religion studies, PE02 teachers specialize in teaching Greek language, history and other literature subjects, PE03 teachers teach mathematics, PE04.01 teach physics etc. Teachers can only teach subjects that belong to their categorization.

3.3 Test Score Data

We also obtained student-level data from the Ministry of Education with test scores on the Panhellenic Examinations, national exams that all Greek high school students take in twelfth grade. Our data spans 2004 – 2011 and includes each student's total score and school attended. These exams are the most important determinant for university admission; given these high stakes, the exams are graded by external markers. The exams cover the core subjects (Mathematics, Greek Language, History, Biology, Physics), and thus we will restrict our analysis to teachers in these subjects unless otherwise noted. Students also take exams in other subjects depending on whether they have chosen the Classics, Science, or Exact Science track.

3.4 Micro School Data

Our national data has two main drawbacks. We only know teachers' district, not school, and we are limited to a single outcome in twelfth grade. We thus supplement the national data with micro data from 23 high schools. We obtained this data by visiting these schools in person, requesting all of their records, and digitizing them. The data set's key features are student and teacher course schedules for all high school grades that allow us to link a student to a specific teacher for each course and subject-specific exam scores. While the sample is limited, the more precise match between teacher and her students' outcomes will form the basis for our student-level empirical analysis.

4 Empirical Strategy

4.1 Target Parameter

Before we specify our empirical model, we define our target parameter of interest. To fix ideas, consider the example in Figure 3 of two teachers who earned their degrees in July, 2004. While otherwise identical, teacher 1 had her degree conferred earlier in the month than teacher 2, so teacher 1 started with a better waitlist position. This led to an immediate assignment in fall 2004 for teacher 1, while teacher 2 remained unassigned. Thus, at the end of the 2004-2005 school year, teacher 1 had accumulated 0 years without formal employment and 1 year of formal experience while teacher 2 had accumulated 1 year without formal employment and 0 years of formal experience. For the following school year, both teachers received assignments, such that at the end of the 2005-2006 school year, teacher 1 had accumulated 0 years without formal employment and 2 years of formal experience while teacher 2 had accumulated 1 year without formal employment and 1 year of formal experience.

Our target parameter is the causal effect on some output measure of a year without formal employment instead of a year with formal employment. Identifying this effect involves comparing outcomes for comparable teachers in the same school year, but with exogenous variation in the fraction of years since degree conferral that have been experience versus without formal employment. Specifically, we can compare teachers 1 and 2 in the same year – 2005-2006 – where the difference is teacher 1 has one fewer year without formal employment and one more year of formal experience. This target parameter captures the combined effect of skill depreciation, from not using skills in formal employment, and forgone experience that would have been accumulated if formally employed.[20] This combined effect is the relevant parameter for the effects of policies that shift employment status, as any additional year spent not working automatically includes forgone experience. A large causal effect, then, might be driven by high depreciation rates, large positive returns to experience, or a combination. In Section 7 we decompose the total effect into depreciation and forgone experience channels. Because this analysis imposes additional assumptions, we defer the discussion until Section 7.

4.2 Specification and Identification

Let j index teachers, k index assignment unit (classroom or district) and t index academic year. We would like to estimate the effect of a year without formal employment ($YearsNotEmp_{jt}$) on an output measure (y_{jkt}). But if the teacher hiring process favors more productive teachers, then regressing y_{jkt} on $YearsNotEmp_{jt}$ will not yield a consistent estimate of the true causal effect. Greece's centralized assignment process provides us with useful variation in $YearsNotEmp_{jt}$ that we argue is unrelated to a teacher's potential productivity. Define teacher j's risk set in year t, $m(j)t$, to be the set of teachers who had their degrees conferred in the same year-month as j, teach

[20]The year not working might also induce a change in effort once employed, for reasons like disillusionment with the profession or assignment process. If effort is changing, it would be part of our skill depreciation effect.

the same subject as j, and are eligible for assignment in year t (i.e., they have not attrited from the lists). If we control for a teacher's risk set, then we isolate variation in $YearsNotEmp_{jt}$ among teachers who completed their education at very similar points in time. But this remaining variation may still be related to a teacher's potential productivity. For instance, teachers may receive an assignment through their ASEP test scores. Or an assignment may come from the experienced list based on a high number of credits possibly accrued through a teacher's past actions.

Thus, we instrument for within-risk set variation in $YearsNotEmp_{jt}$ with a teacher's normalized waitlist position from the fresh graduates list in the first year the teacher appears in our sample.[21] Because a teacher may appear on both the deputy and hourly lists, we use the minimum normalized waitlist position across the two lists and label it as p_j. We only use the waitlist position from the fresh graduates list because the position on the experienced list may be related to a teacher's potential productivity. The fresh graduates list position, however, still strongly predicts the speed of assignments on the experienced list because an earlier first assignment starts the credit accrual process faster and moves the teacher up the experienced list.

Our empirical model is:

$$y_{jkt} = \alpha YearsNotEmp_{jt} + \mu_{m(j)t} + \epsilon_{jkt} \tag{1}$$

$$YearsNotEmp_{jt} = \lambda p_j + \theta_{m(j)t} + \nu_{jt} \tag{2}$$

where $\mu_{m(j)t}$ and $\theta_{m(j)t}$ are vectors of risk set-year fixed effects. Our exclusion restriction is that normalized waitlist position is independent of unobserved determinants of outcome y_{jkt} once we control for risk set-year. In our context, there are two types of identification threats: (1) waitlist position is (conditionally) correlated with a teacher's potential productivity or (2) waitlist position is (conditionally) correlated with student types. While our assumption of independence is untestable, our knowledge of the institutional environment and related data analysis offer support.

As described in Section 2, waitlist position is determined by the date of degree conferral, with ties broken by grade-point average. Teachers graduating in different semesters may differ in many ways. Thus, we isolate only fine timing differences by controlling for the month-year of

[21] We normalize the waitlist position by the length of the list so it runs from 0 to 1. The first year the teacher appears in our sample is the maximum of 2003 and the teacher's degree conferral year.

degree conferral and argue that remaining conferral date variation within the month is exogenous. Our identification strategy fails if within-month variation in graduation timing correlates with a teacher's potential productivity, perhaps because more productive prospective teachers pressure faculty members to enter grades quickly.[22] Even if there were a pattern in which more productive teachers graduate sooner, there is nothing special in the education system about graduating at the beginning of a calendar month. Thus, "expedited" graduates could earn their degrees faster than other students and still be at the end of a calendar month.

But to provide more evidence that within-month timing of graduation appears unrelated to teacher type, we regress the teacher's university grade point average (out of 10) on the teacher's waitlist position percentile (position normalized by the length of the list). In Table 2, we show a strong relationship between waitlist position, as determined by degree conferral date, and the teacher's university GPA. This could reflect grade inflation as later cohorts have higher (worse) waitlist positions and higher grades. In the second column, we add fixed effects for each graduation month-year combination, separately for each academic subject because the waitlists are subject-specific. Once we rely only on waitlist position variation from within-month differences in graduation timing (and estimate Equation 2, but replacing the left-hand-side with teacher's GPA), the relationship between waitlist position and teacher grades goes away, with a high degree of statistical precision.

Even if initial waitlist position were (conditionally) uncorrelated with teacher type, the assignment process could induce a correlation over time via attrition. As prospective teachers wait for an assignment, some may find full-time employment that would make them ineligible for public school teaching. If the prospective teachers who attrit differ in productivity from the teachers who remain on the waitlists, then initial waitlist position may be correlated with teacher potential productivity among the remaining teachers, even if we account for the direct impact of different time without formal employment. Whether attriting teachers would be positively or negatively selected on teaching potential productivity is unclear and depends on how teacher potential productivity correlates with productivity for other jobs.

Attrition is quite common, which is perhaps unsurprising given the long wait times until assignment. In Figure 4 we plot cumulative attrition rates for the 2003 graduating cohort, the first

[22] In our own experience, we have found such pressure to be unsuccessful. We hired a soon-to-be-graduate to be a research assistant on the project and have found it impossible to expedite his degree conferral, despite our requests.

one entering the waitlists after our sample period begins, where we define attrition as dropping off the waitlists without ever receiving an assignment.[23] We split the 2003 cohort into five quintiles based on their waitlist positions. Most graduates stay on the list for a second year, as attrition rates are below 5% for the 2004 list. Attrition steadily increases, however, and by the 2009 lists, 30-40% of the teachers have attrited. The patterns are similar for the five waitlist quintiles, and quintiles with the best (Quintile 1) and worst (Quintile 5) waitlist positions have attrition rates in between the quintiles with waitlist positions in between.

We further explore the relationship between our instrument – fresh graduates' waitlist position conditional on degree month-year – and attrition in Table 3. Our data sample includes all teachers belonging to risk sets that lead to any assignments, as these are the risk sets that will identify our causal estimates. We estimate Equation 2, but replace the left-hand-side with an indicator for attrition, and we fail to reject the null hypothesis that there is no relationship between conditional waitlist position and attrition. While within risk-set variation still predicts how quickly fresh graduates receive assignments, the within risk-set variation is much smaller than the across risk-set variation, and attrition rates only appear responsive to these larger differences.

Attrition appears to be unrelated to our instrument and also teachers' university academic achievement. In Table 4 we see that teachers with higher university grade point averages are no more likely to attrit, regardless of whether we control for risk set. Thus, while our assumption that there is no selective attrition is fundamentally untestable, the balanced attrition rates across our instrument and lack of relationship between attrition and teacher academic achievement are encouraging.

The second threat to identification realizes if teachers' waitlist positions are unrelated to their potential productivity but correlated with the characteristics of the students they teach. For instance, if economic conditions worsen, family life may be more stressful and students may perform poorly. Or perhaps some districts invest in smaller class size, which leads to better test outcomes. If these shocks or actions are correlated with changes to a school's demand for temporary teachers, then we might worry that our estimates confound the teacher assignment effects with local shocks.

We consider these threats unlikely, as schools request temporary teachers on a rolling basis, with fast churn such that it would be nearly impossible to target specific teachers. This is consistent

[23]Patterns are similar if we define attrition as dropping off the waitlists before receiving a second assignment.

with Table 5, where we regress a school district's twelfth grade cohort size or local unemployment rate on each assigned teacher's waitlist position. We find no relationship between waitlist position and these district characteristics in the cross-section, nor within district, when we compare changes over time by including district fixed effects.[24] Thus, teachers with different (conditional) waitlist positions do not appear to be assigned to schools in different types of districts.

5 Estimates

We estimate two versions of our empirical model, each adapted to a different level of aggregation in our data. The first will be a student-level model that describes how students' outcomes vary with the years without formal employment of their assigned teacher. This model exploits the detailed data on student outcomes and teacher classroom assignments from the 23 schools in the micro data. We will then extend the sample to the entire country with a district-level model that describes how a districts' mean student outcomes vary with the mean years without formal employment of their teachers.

5.1 Student Empirical Specification

We introduced our empirical model, Equations 1 and 2, at the teacher-level as that is the level of assignment. But our micro data set of 23 schools includes multiple output measures per teacher – individual student scores on each exam they take. We thus recast our model at the student-level but exploit the same identifying variation. Let i denote a student, s denote a subject, $j(i, s, t)$ denote student i's teacher for subject s in year t, and $k(i, t)$ denote student i's school in year t. We specify our model of a student-subject exam outcome y_{ist} as:

$$y_{ist} = \alpha YearsNotEmp_{j(i,s,t)t} + \mu_{m(j(i,s,t))t} + \epsilon_{ist} \tag{3}$$

$$YearsNotEmp_{j(i,s,t)t} = \lambda p_{j(i,s,t)} + \theta_{m(j(i,s,t))t} + \nu_{j(i,s,t)t} \tag{4}$$

Because this specification links teachers to the outcomes of the students they have in their

[24]The regression's outcome only varies across school districts and years. If we estimate an district specification, as introduced in Section 5, the results are qualitatively similar. We also find no relationship between the average university GPA of a district's assigned teachers and the teachers' mean conditional waitlist position.

classrooms, we will rely on outcomes from our micro data set of 23 schools. Unfortunately, these 23 schools only have a small number of temporary teachers in tested subjects. To maximize our statistical precision, we implement a within transformation on our empirical model. Let $\widetilde{x_{ist}}$ denote x_{ist} demeaned at the risk set level ($\widetilde{x_{ist}} = x_{ist} - x_{\overline{m(j(i,s,t))t}}$). If we apply the within-risk set transformation, our model becomes:

$$\widetilde{y_{ist}} = \alpha \widetilde{YearsNotEmp_{j(i,s,t)t}} + \widetilde{\epsilon_{ist}} \tag{5}$$

$$\widetilde{YearsNotEmp_{j(i,s,t)}} = \lambda p_{\widetilde{j(i,s,t)t}} + \nu_{\widetilde{j(i,s,t)t}}. \tag{6}$$

For the model observables that do not require student-level data ($\widetilde{YearsNotEmp_{jt}}, \widetilde{p_{jt}}$), we can perform this within-risk set transformation using the full national sample of teachers. But because we only observe outcome y_{ist} for a subset of 23 schools, we cannot estimate this model on the full sample. Instead, define $\eta_{ist} = \widetilde{\epsilon_{ist}} + y_{\overline{m(j(i,s,t))t}}$ and rewrite our model as:

$$y_{ist} = \alpha \widetilde{YearsNotEmp_{j(i,s,t)t}} + \eta_{ist} \tag{7}$$

$$\widetilde{YearsNotEmp_{j(i,s,t)t}} = \lambda p_{\widetilde{j(i,s,t)t}} + \nu_{\widetilde{j(i,s,t)t}}. \tag{8}$$

We estimate this model as our student-level specification. We construct $\widetilde{YearsNotEmp_{j(i,s,t)t}}$ and $p_{\widetilde{j(i,s,t)t}}$ using the risk set means from the full sample of teachers and then estimate the instrumental variables specification using the students and teachers in our micro data set. Provided our micro sample is representative, the identification assumptions are unchanged: if $E(p_{\widetilde{j(i,s,t)t}}\widetilde{\epsilon_{ist}}) = 0$ then $E(p_{\widetilde{j(i,s,t)t}}\eta_{ist}) = 0$ by construction.[25] This procedure allows us to use the national sample to control for differences across risk sets while using the micro sample for linked outcomes.

Before estimating the model, we present distributions of the demeaned waitlist positions ($\widetilde{p_{jt}}$) and accumulated years without formal employment ($\widetilde{YearsNotEmp_{jt}}$) for the full sample in Figure 5. We see that even when controlling for the degree month-year, there is still a lot of residual variation left in our instrument and the years without formal employment. This occurs in part because earlier first assignments generate faster second assignments and so forth, given how the

[25] $E(p_{\widetilde{j(i,s,t)t}}\eta_{ist}) = E(p_{\widetilde{j(i,s,t)t}}\widetilde{\epsilon_{ist}}) + E(p_{\widetilde{j(i,s,t)t}} y_{\overline{m(j(i,s,t))t}}) = 0 + E(p_{\widetilde{j(i,s,t)t}} y_{\overline{m(j(i,s,t))t}}) = E_{mt}(E((p_{\widetilde{j(i,s,t)t}} y_{\overline{m(j(i,s,t))t}}|m,t)) = E_{mt}(y_{\overline{m(j(i,s,t))t}} E(p_{\widetilde{j(i,s,t)t}})) = E_{mt}(y_{\overline{m(j(i,s,t))t}} * 0) = 0.$

experienced teachers waitlist is structured. Thus, we are able to implement fine controls for degree timing and still have enough variation left over to generate precise causal estimates.

Consistent with the details of the assignment process, we observe a strong relationship between demeaned waitlist positions and years without formal employment. We show a binscatter plot of the two variables in Figure 6. Worse waitlist position from the fresh graduates list, even after controlling for risk set, strongly predicts a higher number of years without formal employment. Relative to a teacher at the front of the waitlist, a teacher at the waitlist's median is expected to wait almost an additional year.

We now turn to our main causal estimates. We estimate our empirical model on the student-level data using students' total exam score in a subject-year as the outcome.[26] We express the score in three different units: student standard deviations (σ), levels, and logs. We present OLS and IV results in Table 6, where in each model we cluster the standard errors at the teacher level. In the first column, we show OLS estimates from a model that controls for each teacher's risk set but does not instrument for the number of years without formal employment. We also add fixed effects for the school-year, school, grade, and subject to account for the heterogeneity we introduce from pooling teachers and their courses.[27] We find a fairly precise estimate of no effect of years without formal employment on student total exam scores. In many settings, we might expect the OLS estimates to be biased downward, if potential productivity correlates negatively with years without formal employment and negatively with output. The Greek teachers assignment process, however, complicates this argument, as other elements like attrition could reverse the sign of the bias.

Before we introduce our IV estimates, we assess the first stage in our micro data sample, shown in column 2. We find a strong relationship between (demeaned) waitlist percentile and (demeaned) years without formal employment, with an F-statistic above 30 despite our small sample of teachers.[28] In column 3 we introduce the reduced form specification with total exam score expressed in standard deviation units and in the final three columns we display our IV estimates. We estimate that an additional year without formal employment lowers student test scores by 0.10 student

[26]Total exam score combines two midterms and a final.

[27]Our within-transformation by risk set-year complicates the subsequent inclusion of school fixed effects as school varies within risk set. For instance, our exclusion restriction could fail if teachers' demeaned waitlist position correlates with the fraction of a school's workforce the teacher makes up. In our data, we find a precise zero relationship between these variables. We also find similar results if we use test outcomes already residualized by school fixed effects.

[28]Our current estimates rely on 19 unique teachers. Future versions will seek to expand this sample by improving the matching across datasets.

standard deviations, or 2.2%.[29]

In Table 7 we change the measure of output to a student's grade-point average, out of 20. Our estimates are considerably more precise, perhaps because grade-point average is a more precise signal of a student's achievement. We estimate that students assigned to teachers with an additional year without formal employment have grade-point averages in that academic year that are 0.09 student standard deviations, or 2.0%, lower. These estimates indicate that beyond the loss of income teachers face from the undersupply of teaching positions, there are large consequences to output.

5.2 District Empirical Specification

We now turn to a model of district-year student outcomes. We sacrifice the precise measurement of an individual teacher's multiple outputs but potentially gain increased statistical power and external validity by extending the analysis to the national level. Additionally, the district results are robust to concerns that within-school assignment of students to teachers' courses may change depending on the type of the newly-assigned temporary teacher.

We derive our district empirical specification from the same empirical model, Equations 1 and 2, by introducing an aggregation matrix, A. An element $(A_{r,c})$, corresponding to row $r = dt$ and column $c = j$, is the fraction of district d's teacher work force that teacher j comprises in year t. Each column will have 0 in all elements corresponding to year t except one, as each teacher works in a single district in a given year. If a district has 20 high school teachers in tested subjects, then each of those 20 teachers will have $1/20$ as their non-zero element. Unfortunately, we only observe the number of students, not the number of teachers, in each district-year in our data. We therefore estimate the number of teachers based on the number of students and the mean class size in our micro data. For these 23 schools, we find limited variation in class size and that our imputation measure performs well.

We perform the within risk-set transformation on Equations 1 and 2, as in the student empirical specification. Then we left multiply the demeaned equations by matrix A, which yields our district-

[29]Analysis using test scores as outcomes rarely expresses effects in log test score units. We provide the estimate here because the human capital depreciation literature typically expresses the depreciation parameter in percentage terms. We acknowledge that test score scales were not necessarily designed for log transformations.

level model:

$$y_{dt} = \beta Years\tilde{NotEmp}_{dt} + \eta_{dt} \tag{9}$$

$$Years\tilde{NotEmp}_{dt} = \gamma \tilde{p_{dt}} + \tilde{\nu_{dt}}. \tag{10}$$

with $\tilde{x_{dt}} = \frac{1}{N_{dt}} \sum_{j \in J_{dt}} \tilde{x_{jt}}$ where J_{dt} is the set of teachers in district d in year t and N_{dt} is the size of this set. We let each permanent teacher be her own risk set, as we do not have exogenous variation in years without formal employment for these teachers. As above, we do not demean the outcome, y_{jt}, so

$$\eta_{dt} = \tilde{\epsilon_{dt}} + \frac{1}{N_{dt}} \sum_{j \in J_{dt}} \bar{y_{m(j)t}} = \tilde{\epsilon_{dt}} + \frac{1}{N_{dt}} \sum_{j \in J_{dt}} YearsNo\bar{t}Emp_{m(j)t}. \tag{11}$$

Before presenting the results, we revisit the identifying assumptions now that we have a district model. We maintain the prior assumptions that waitlist position is conditionally independent from teacher's unobserved potential productivity and assigned student types. We further assume that within a region assignees' (demeaned) waitlist positions are independent from the region's other assignees' types. We cannot test this directly, though we do not know of any way a district could target teachers from a specific part of the waitlist, controlling for risk set. But as the specification of η_{dt} clarifies, we need $\tilde{p_{dt}} \perp \frac{1}{N_{dt}} \sum_{j \in J_{dt}} YearsNo\bar{t}Emp_{m(j)t}$, which is testable. We present the test in Table 8, where we fail to reject no statistical relationship.

Given this assumption of independence in assignments, we might worry that the law of large numbers eliminates any variation in the instrument or endogenous regressor across district-years. But the small number of high school temporary teachers (in tested subjects) assigned for most district-years leaves considerable sampling variation. We show the remaining variation in Figure 7. Compared to Figure 5, the aggregation shrinks the dispersion, especially of our instrument. But we still have enough variation left to generate precise estimates of causal effects.

We present our district-level estimates in Table 9. Our OLS regression in column 1 yields a positive, and statistically significant, association between teacher time without formal employment and students' twelfth grade test scores. But the relationship flips signs once we instrument for the district's mean teacher years without formal employment. In column 2 we present the district-level first stage regression. Despite the aggregation, we still have a strong instrument to predict differences in mean years without formal employment across district-years. Our IV estimates show significant decreases in student test scores when a district's teachers have spent longer without formal employment. For each year increase in average time without formal employment, we estimate that student test scores fall by 3.7 test score percentiles, or 0.11 student standard deviations (σ). In terms of log scores, we estimate a decrease of 4%.[30]

These estimates are similar to our student-level model estimates. The effects on student standard deviations are nearly identical, while the effects in logs are slightly higher for the district-level estimates. Compared to estimates of the impact of teachers on student test scores, these effects are large. Chetty, Friedman, and Rockoff (2014a) estimate that a standard deviation in teacher value-added is 0.14σ in mathematics and 0.10σ in English. Our estimates would thus imply that a year without formal employment moves a teacher a full standard deviation within the teacher distribution of value-added. These effects combine depreciation and forgone experience, which we will decompose in Section 7. As a benchmark, Kane, Rockoff, and Staiger (2008) estimates that relative to a third-year teacher, a first-year teacher has 0.06σ lower mathematics value-added and 0.03σ lower reading value-added. Our larger estimates indicate either that experience returns are higher in this context or that depreciation plays a large role in determining teacher productivity.

6 Robustness and Heterogeneity

6.1 Robustness Checks

In Section 4 we discussed the primary threats to identification and offered analysis in support of our assumptions. To establish further that the results are causal, and in particular not confounded by school- or district-level shocks, we implement additional placebo tests.

We start by exploiting the timing of assignments. Our district-level estimates include district fixed effects, which would control for time invariant district differences, such as differential teacher attrition rates, that might relate to teacher assignment. Including district fixed effects would be insufficient, however, if districts have particularly high attrition – or face some other time-varying

[30]As in the student model, our inclusion of fixed effects that cleave risk sets – in this case, the district fixed effects – is not fully consistent with our within risk-set transformation. We find very similar results without including district fixed effects, though standard errors are larger.

shock – in some years, for reasons that cause test score changes even if new assignments had an average amount of time out of formal employment. If such shocks are serially correlated, and correlate with our IV, then we would expect that the following year's assignments' time without formal employment would correlate with this year's test score outcomes, even though they cannot have a direct effect as they only show up in the district a year later. We therefore extend our IV specification to include the average time out of the labor force of the following year's assigned teachers to that district.[31] Table 10 shows that our result is largely unchanged – test scores fall strongly in response to additional time without formal employment of the current cohort of assignees. Furthermore, the time without formal employment of the subsequent cohort does not predict student test score decreases.

Our second placebo test examines the role of teachers assigned through the exact same process but who teach subjects that do not appear on the twelfth grade exams. These subjects include music, foreign languages, physical education, and household economics. If there is some district- or school-level confounder that correlates with our instrument, it is likely to correlate with waitlist percentiles among all assigned teachers, not just those in tested subjects. We present the relationship between time without formal employment, instrumented with waitlist percentiles, and student test scores for teachers in untested subjects in Table 11. We fail to reject no statistical relationship, albeit with large standard errors. Thus, the effects we find appear specific to the assignments of teachers in tested subjects. Any confounders would have to apply specifically to these subjects.[32]

6.2 Heterogeneous Effects

The main results assume constant treatment effects and pool spells without formal employment of different lengths. But the depreciation process may not involve constant losses for each year without formal employment. The curvature of depreciation can be relevant for policy design as the generosity of unemployment benefits during the unemployment spell would depend on the size of the skill loss in each period. We investigate whether the causal effect of not having formal employment varies with the length of a non-formal employment spell by dividing district-years based on the *expected* average spell length of the new assignees. The expectation is based on the

[31] We add another instrument – the mean waitlist rank for the following year's assigned teachers.

[32] Teachers in non-tested subjects could still affect outcomes through spillovers, such as lowering work force morale. Our results indicate little evidence of this.

assignees' risk set means. Thus, we divide districts according to whether they pull new assignments from risk sets that on average had more or less time out of formal employment. We then identify the treatment effect for this subsample by comparing (instrumented) differences in average realized time without formal employment. To be more concrete, suppose two districts each are assigned a single new teacher. Both new teachers come from "lucky" risk sets where many teachers that graduated in these year-months received fast assignments. Thus, each risk set has an average time out of formal employment of one year. But within the risk set, there is variation. The first district received a teacher who was at the front of her risk set and did not have to wait any years while the second district received a teacher from the middle of her risk set whose wait time was one year. Thus, we would compare the outcomes for these two districts to generate our causal estimates.[33]

We present the results in the first two columns of Table 12. The causal effects appear concentrated in shorter spells without formal employment. Among districts whose assignees had expected wait times of four years or less, we find that an additional year without formal employment leads to a decrease in student test scores of nearly 11 percentiles. For districts whose assignees had expected wait times beyond four years, we find little difference. Thus, policy interventions aimed at insuring against this loss in output should target the earlier part of a non-employment spell.

We also explore the impact of variation in time without formal employment at different points in a teacher's career. We might expect that output losses differ for teachers with and without experience. For instance, teachers without experience move from formal education into a possibly unstructured period of waiting for assignment. If these teachers fail to develop strong work habits – like showing up on time – due to the sudden transition from operating in a formal structure to no structure, then depreciation might be higher for inexperienced teachers. On the other hand, experienced teachers may have accumulated skills during their prior assignments and thus there is more room for depreciation.

We test for heterogeneous effects by splitting the districts based on the *expected* average past experience of the new assignees. We present the results the last two columns of Table 12. Our point estimates indicate higher annual output losses from non-formal employment for more experienced teachers, but the estimates are noisy and we fail to reject equal effects.

[33] Our understanding of the assignment process is that regions cannot target certain types of teachers. Therefore, unlike most heterogeneity analysis where the source of heterogeneity is not randomly assigned, here we have exogenous variation in samples. Risk sets may, of course, differ in many ways beyond the spell lengths of their members.

7 Mechanisms

Our estimates capture the effect of a year without formal employment on worker output. This is the policy-relevant estimate for policies that shift (formal) employment. But whether this effect is driven by high levels of depreciation or missing out on the returns to experience matters for some other policies. For example, policies that provide unemployed workers with a structured environment that reviews material learned in formal education might be particularly desirable if depreciation is the dominant mechanism.

To demonstrate how we will separate the impact of forgone experience from depreciation, we return to Figure 3. We have previously described comparing the two teachers in the same academic year. Consider instead a comparison that controls for experience levels – specifically, suppose we compare teacher 1 in her first year of experience (2004-05) with teacher 2 in her first year of experience (2005-06). We call this the conditional comparison, but because it introduces additional complications, we add the following assumptions:[34]

Assumption A1. *(Additive Separability) Depreciation and return to experience are additively separable.*

Assumption A2. *(No Age Effects) The causal effect of age on teacher output is 0.*

These assumptions allow us to compare across years (no age effects) and characterize the difference between our baseline estimates (depreciation and forgone experience) and these conditional estimates (depreciation only) as the return to experience (additive separability). Ruling out age effects is a strong assumption, though the teacher value-added literature has not generally identified large age effects. If, instead, we only impose the weaker assumption that age effects are weakly positive, then our conditional estimate will be a lower bound on depreciation.[35]

We implement our estimate of the depreciation-only effect by altering the risk set definition. Previously, a teacher's risk set (m) consisted of the teachers in the same subject whose degrees were conferred in the same year-month. We then interacted m with time to generate μ_{mt} fixed

[34] We also assume that we have already controlled for any year effects. Our empirical specification controls for year via fixed effects, and several of our test score outcomes are standardized by year. We thus do not list "no year effects" as an additional assumption.

[35] The conditional estimates combine depreciation and age effects. If the age effects are weakly positive and change in output from depreciation is weakly negative, then the combined effect will understate the depreciation component.

effects for the empirical specification. We now add a further condition to the risk set – teachers must have the same number of years of prior experience. Denote this new risk set as m'. We then implement the same empirical specification, but instead of $\mu_{m(j)t}$ fixed effects, we specify $\mu_{m'(j,t)}$ fixed effects, and do the within-transformation accordingly.

We present the results of the student-level model in Table 13. Our estimates of the depreciation rates are large but noisy. For all three test score functional forms, we estimate effects twice as large as the baseline estimates, but the standard errors are large enough that we fail to reject no depreciation. We have more precision for the three grade-point-average outcomes. Here we find estimates statistically different from 0 and of a larger magnitude than the baseline estimates. This implies a negative return to experience, though we cannot reject a zero return.

Table 14 presents the results from the district-level model. We estimate a precise skill depreciation rate of 0.13σ, or 4.8%. This, again, is larger in magnitude than the baseline estimates, implying a slightly negative return to experience. This small, or even negative, return to experience may be surprising. But we reiterate that experience in this context is accrued at a different school and automatically includes a disruption cost of having to move one's family to a different school district.

In all of our specifications, these conditional estimates imply large skill depreciation rates that can account for the entire causal effect of time without formal employment on student outcomes. Even if we only impose that age effects are weakly positive, skill depreciation rates still account for at least the full causal effect of $0.10 - 0.11\sigma$.

8 Concluding Remarks

This paper demonstrates that human capital depreciates – workers become less productive if they spend time without formal employment. We show that teacher who are quasi-randomly assigned additional years without formal employment are less productive than teachers with less time without formal employment. The effects are large, as an additional year without formal employment leads to a $10 - 11\%$ of a standard deviation reduction in student test scores. We estimate that these effects are due to skill depreciation rather than missing out on a year of accrued experience.

These large estimates describe persistent costs of unemployment and highlight a benefit of policies that promote employment or labor force attachment. In the absence of such policies, activities that mimic the work environment and provide structure may help non-employed workers maintain their skills. Our analysis has little to say about the exact type of skills that are depreciating. We thus encourage more research that collects skill measurements and assess how they vary with employment status.

We also caution that the results focus on a particular profession – teachers – in a small European country. Human capital depreciation rates might be larger or smaller in other professions or contexts. We encourage future work that explores whether our results generalize to other settings.

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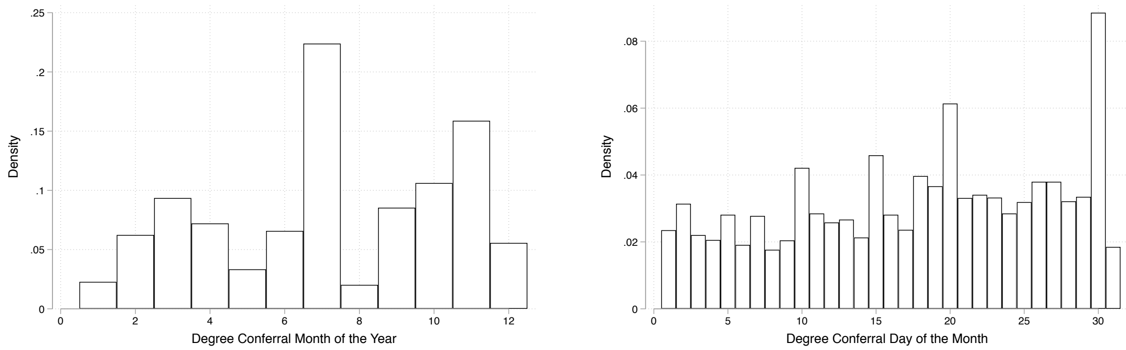
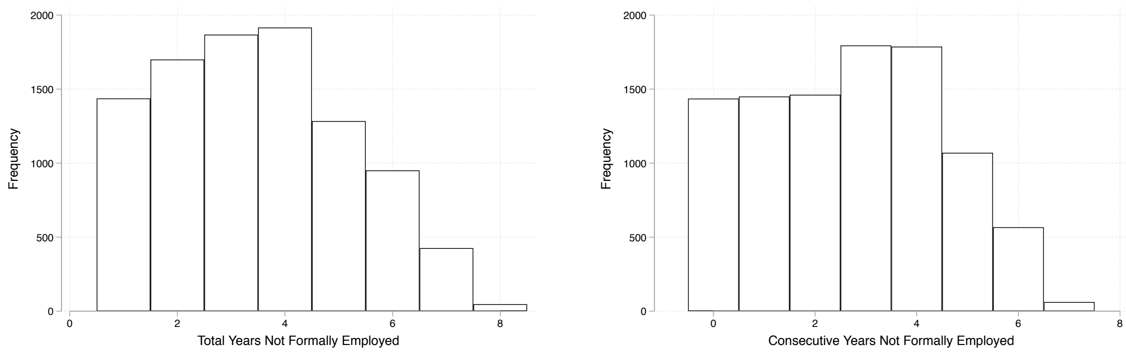
Figure 1: Degree Conferral Months and Days



Notes: The figure shows the histograms of the degree conferral month of the year (left) and day of the month (right). An observation is a teacher on a waitlist between 2004 and 2011 whose degree was conferred before 2006.

Figure 2: Years without Formal Employment

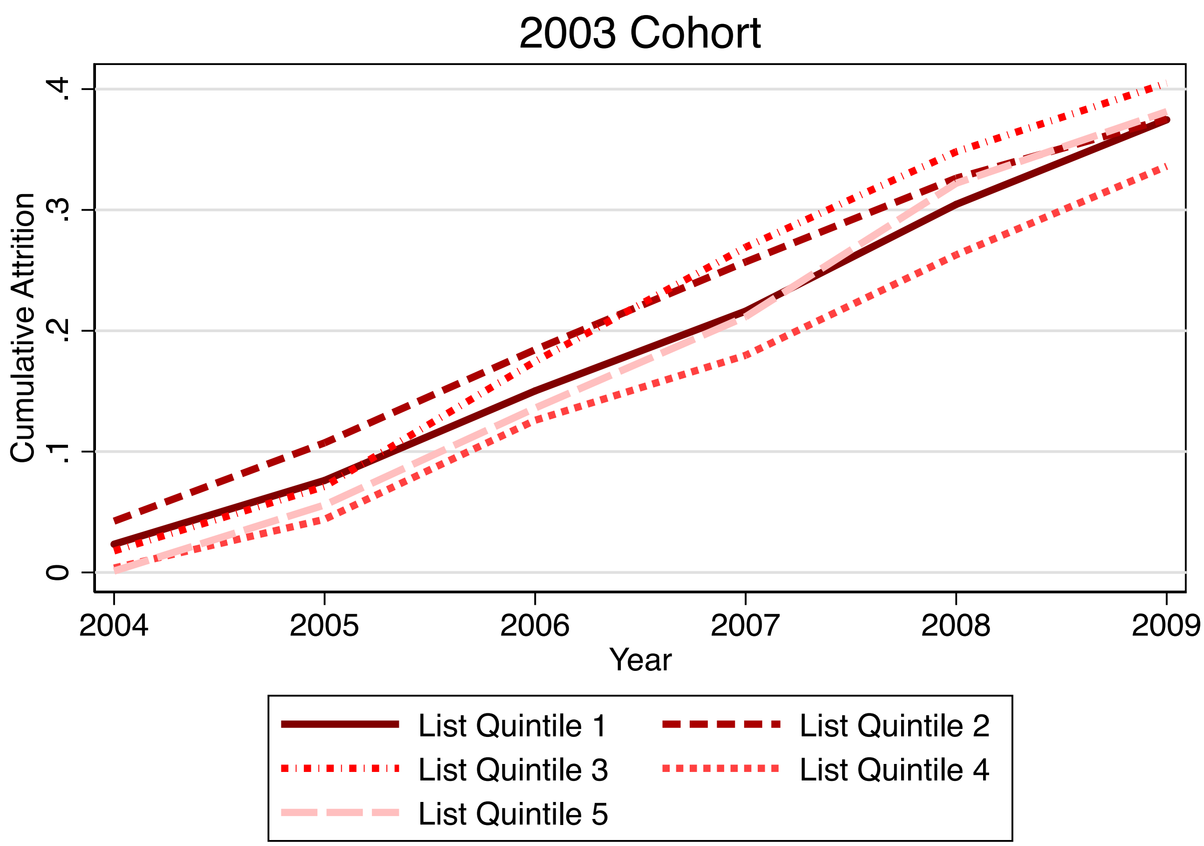


Notes: The figure shows the histograms of the total years without formal employment (left) and the consecutive years without formal employment (right). An observation is a teacher on a waitlist between 2004 and 2011 whose degree was conferred before 2006.

Figure 3: Example Comparison

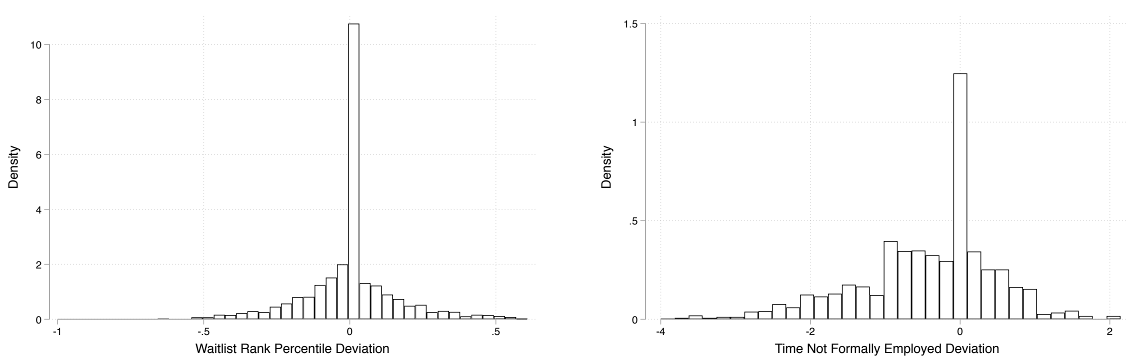
	2004-05	2005-06
Teacher 1	Graduates in July and Assigned (Years Not Emp, Experience) = (0,1)	Assigned (0,2)
Teacher 2	Graduates in July but Not Assigned (Years Not Emp, Experience) = (1,0)	Assigned (1,1)

Figure 4: Attrition by Waitlist Percentile – 2003 Graduating Cohort



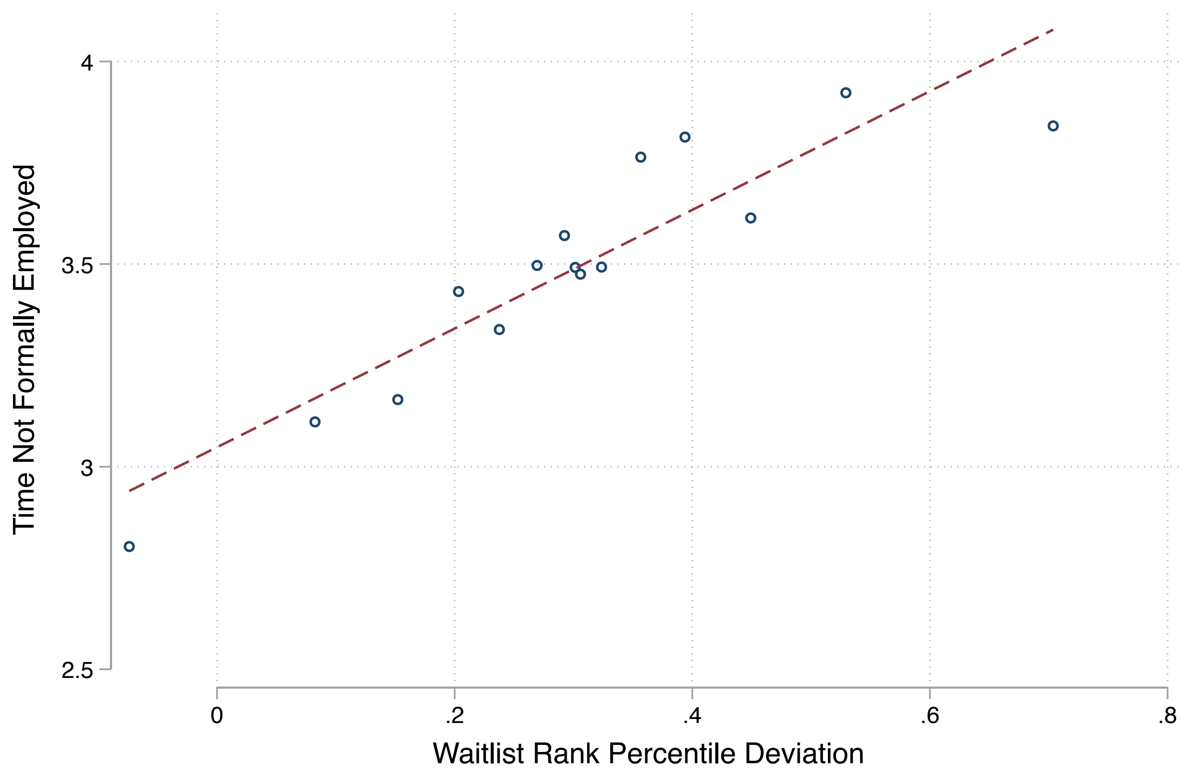
Notes: An observation is a teacher that had his or her degree conferred in 2003. The figure splits teachers into quintiles within each degree month-year based on waitlist position. Attrition is whether the teacher left the waitlist prior to receiving an assignment.

Figure 5: Waitlist Rank and Years without Formal Employment – Deviations from Risk Set Mean



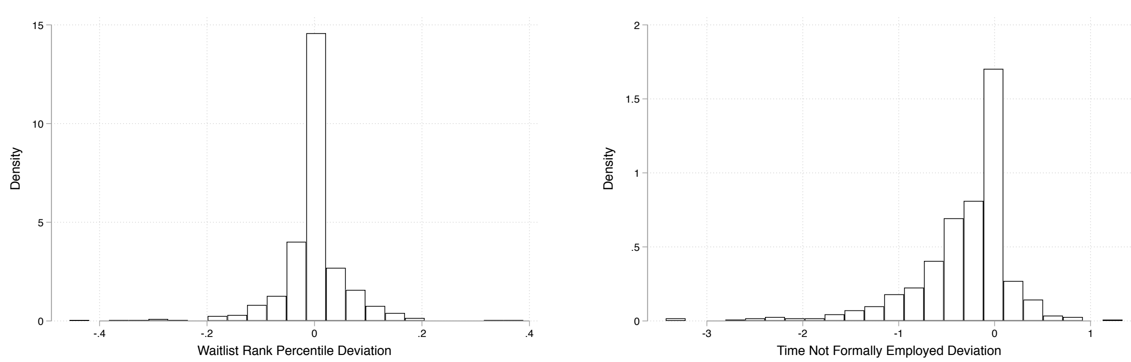
Notes: The figure shows histograms of the demeaned waitlist percentile (left) and years without formal employment (right) where the mean is calculated for each risk set-year. A risk set is a degree conferral year-month and subject combination. The sample includes all assigned teachers between 2003 and 2011 whose degree was conferred before 2006.

Figure 6: Time without Formal Employment and Waitlist Rank – Teacher Level



Notes: The binscatter figure shows the relationship, at the teacher level, between initial position on fresh graduates waitlist and mean number of accumulated years without formal employment. The sample includes all teacher on a waitlist between 2003 and 2011 whose degree was conferred before 2006.

Figure 7: Aggregated Waitlist Rank and Aggregated Years without Formal Employment – Deviations from Risk Set Mean



Notes: The figure shows histograms of the district-year mean (demeaned) waitlist percentile (left) and (demeaned) years without formal employment (right) where the district-year mean is calculated over the assigned teachers' (demeaned) variables. A risk set is a degree conferral year-month and subject combination. The sample includes all district-years that received temporary teachers between 2003 and 2011.

Table 1: Waitlist Summary Statistics

Variable	N	Mean	St Dev	Min	Max
Year2003	383,644	0.0006	0.0245	0	1
Year2004	383,644	0.0188	0.1358	0	1
Year2005	383,644	0.0446	0.2064	0	1
Year2006	383,644	0.0983	0.2977	0	1
Year2007	383,644	0.1099	0.3128	0	1
Year2008	383,644	0.1770	0.3817	0	1
Year2009	383,644	0.1459	0.3530	0	1
Year2010	383,644	0.2263	0.4184	0	1
Year2011	383,644	0.1785	0.3829	0	1
Degree Mark	379,631	6.7104	0.6966	0	10
Years Experience	383,197	1.5337	1.9622	0	16
Any Private Experience	264,789	0.0027	0.0522	0	1
Any EU Experience	266,488	0.0018	0.0418	0	1
Any Permanent Experience	266,488	0.0002	0.0123	0	1
Inexperienced	383,663	0.3734	0.4838	0	1
Greek & History	383,663	0.5089	0.4999	0	1
Math & Stats	383,663	0.1497	0.3568	0	1
Physics & Biology	383,663	0.1990	0.3993	0	1
Economics	383,663	0.0773	0.2670	0	1
Computer Science	383,663	0.0624	0.2418	0	1
Degree Year	380,724	1999.9560	6.0055	1936	2011
Years on Waitlist	71,024	2.8776	1.9071	1	9
Length of Waitlist Spell	71,024	2.6374	1.8739	1	8

Notes: The table shows summary statistics from the waitlist data. “Degree Mark” refers to the teacher’s university grade-point average, out of 10.

Table 2: University Grade Point Average by Waitlist Position

	Teacher GPA	Teacher GPA
Waitlist Percentile	0.242***	-0.0209
	(0.0741)	(0.100)
Mean DV	6.993	7.060
N	1747	1189
Risk Set	None	Yes

Notes: The table shows the relationship between teachers' university grades (out of 10) and their waitlist position on the fresh graduates list. Waitlist position is normalized by the list length to be in percentiles. Risk set indicates that risk set fixed effects are included.

Table 3: Attrition by Waitlist Position

	Attriter
Waitlist Percentile	0.00648
	(0.0257)
Mean DV	0.569
N	18248
Risk Set	Yes

Notes: The table shows the relationship between a teacher's initial waitlist position on the fresh graduates list and whether she leaves the waitlists without ever receiving an assignment. Waitlist position is normalized by the list length to be in percentiles. The sample includes all cohorts (degree month-year) with degree conferral before 2006 in which a member ever receives an assignment.

Table 4: Attrition by University Grade Point Average

	Attriter	Attriter
Teacher GPA	-0.00433	0.00452
	(0.00280)	(0.00301)
Mean DV	0.434	0.434
N	62806	62806
Risk Set	No	Yes

Notes: The table shows the relationship between teachers' university grade point average (out of 10) and whether they leave the waitlists without ever receiving an assignment.

Table 5: District Characteristics by Waitlist Position

	Cohort Size	Unemployment Rate	Cohort Size	Unemployment Rate
Waitlist Rank Percentile	-5.651	-0.253	-0.404	-0.00555
	(5.261)	(0.449)	(2.525)	(0.257)
Mean DV	48.98	10.90	48.98	10.90
N	1738	2235	1738	2235
Year FE	Yes	Yes	Yes	Yes
District FE	No	No	Yes	Yes
Risk Set	Yes	Yes	Yes	Yes

Notes: The table shows the relationship between district characteristics and their assigned teachers' waitlist position on the fresh graduates list. Waitlist position is normalized by the list length to be in percentiles.

Table 6: Effect of Years without Formal Employment on Students' Subject Exam Scores

	OLS	FS	RF	IV	IV	IV
	Score (σ)	Yrs w/o Formal Emp	Score (σ)	Score (σ)	Score	Log Score
Yrs w/o Formal Emp	0.0143			-0.0986***	-0.243*	-0.0224
	(0.0312)			(0.0305)	(0.147)	(0.0147)
Waitlist Perc		3.954***	-0.390***			
		(0.710)	(0.0577)			
Mean DV	-0.0913	-0.0303	-0.0922	-0.0922	13.10	2.531
N	2328	1781	1781	1781	1781	1778
Risk Set	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Subject FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table includes OLS, first stage, reduced form, and IV regressions. An observation is a student-subject-year and the IV outcome is the student's total exam score, expressed in student standard deviation units (σ), levels, or logs. Standard errors are clustered by teacher.

Table 7: Effect of Years without Formal Employment on Students' Grade Point Average

	IV	IV	IV
	GPA (σ)	GPA	Log GPA
Yrs w/o Formal Emp	-0.0937***	-0.266**	-0.0199***
	(0.0299)	(0.106)	(0.00736)
Mean DV	0.0108	14.46	2.652
N	1762	1762	1759
Risk Set	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Grade FE	Yes	Yes	Yes
Subject FE	Yes	Yes	Yes

Notes: The table includes IV regressions. An observation is a student-subject-year and the IV outcome is the student's grade-point-average (out of 20), expressed in student standard deviation units (σ), levels, or logs. Standard errors are clustered by teacher.

Table 8: District (Demeaned) Waitlist Position and Mean Expected Years without Formal Employment

	Mean Expected Years wo Formal Emp	Mean Expected Years wo Formal Emp
Mean (Demeaned) Waitlist Position	0.158	0.197
	(0.658)	(0.338)
Mean DV	3.688	3.688
N	536	536
Year FE	No	Yes
District FE	No	Yes

Notes: An observation is a district-year. The dependent variable is the mean expected years without formal employment where the expected years without formal employment is the mean over a teacher's risk set and the first mean is taken over the teachers assigned to the district in a given year. The explanatory variable is the district-year's mean of its assignees' demeaned waitlist position.

Table 9: Effect of Years without Formal Employment on Districts' Panhellenic Exam Scores

	OLS	FS	RF	IV	IV	IV
	Percentile	Yrs w/o Formal Emp	Percentile	Percentile	Score (σ)	Ln Score
Yrs w/o Formal Emp	1.207**			-3.711**	-0.107*	-0.0404*
	(0.583)			(1.478)	(0.0587)	(0.0209)
Waitlist Perc		2.794***	-10.37**			
		(0.408)	(4.694)			
Mean DV	39.93	-0.315	39.93	39.93	-0.229	2.503
N	536	536	536	536	536	536
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Risk Set	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table includes OLS, first stage, reduced form, and IV regressions. An observation is a district-year and the IV outcomes are measures of student performance on the national twelfth grade exams, as mean percentile, mean level (in student standard deviation units) or mean log scores.

Table 10: Placebo Test – Future Assignments

	Percentile
Yrs w/o Formal Emp	-4.834***
	(1.535)
Years w/o Formal Emp t+1	3.296*
	(1.870)
Mean DV	39.88
N	258
Year FE	Yes
District FE	Yes
Risk Set	Yes

Notes: An observation is a district-year and the outcome is the mean percentile on the national twelfth grade exams.

Table 11: Placebo Test – Untested Subjects

	Score (σ)	Ln Score
Years w/o Formal Exp	0.132	0.0302
	(0.175)	(0.0544)
Mean DV	-0.117	2.577
N	278	278
Year FE	Yes	Yes
District FE	Yes	Yes
Risk Set	Yes	Yes

Notes: An observation is a district-year and the outcome is the mean percentile on the national twelfth grade exams. The sample includes all teachers assigned in subjects that are not included on the twelfth grade exams.

Table 12: Heterogeneous Effects

	Spell Length	Spell Length	Experience	Experience
	Percentile	Percentile	Percentile	Percentile
Yrs w/o Formal Emp	-10.88***	-1.656	-2.721	-6.754
	(3.660)	(1.450)	(1.946)	(4.537)
Mean DV	39.22	40.80	39.24	40.51
N	294	242	245	291
Year FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Risk Set	Yes	Yes	Yes	Yes
Range	0-4	4+	0-1	2+

Notes: An observation is a district-year and the outcome is the mean percentile on the national twelfth grade exams. Regression samples are split by mean length of spell out of formal employment or mean past experience levels where the mean is calculated as the average of the risk set the teacher is a member of.

Table 13: Effect of Years without Formal Employment on Students' Subject Exam Scores – Controlling for Experience

	Score (σ)	Score	Log Score	GPA (σ)	GPA	Log GPA
Yrs w/o Formal Emp	-0.230	-0.587	-0.0649	-0.158***	-0.405***	-0.0317***
	(0.183)	(0.549)	(0.0760)	(0.0449)	(0.133)	(0.00741)
Mean DV	-0.0922	13.10	2.531	0.0108	14.46	2.652
N	1781	1781	1778	1762	1762	1759
Risk Set	Incl Exp	Incl Exp	Incl Exp	Incl Exp	Incl Exp	Incl Exp
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Subject FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: An observation is a student-subject-year and the IV outcome is the student's total exam score, expressed in student standard deviation units (σ), levels, or logs. Standard errors are clustered by teacher. Risk sets condition on levels of past experience.

Table 14: Effect of Years without Formal Employment on Districts' Panhellenic Exam Scores – Controlling for Experience

	Percentile	Score (σ)	Ln Score
Time Not Formal Emp	-3.899**	-0.133**	-0.0484**
	(1.570)	(0.0638)	(0.0227)
Mean DV	39.93	-0.229	2.503
N	536	536	536
Year FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Risk Set	Incl Exp	Incl Exp	Incl Exp

Notes: An observation is a district-year and the IV outcomes are measures of student performance on the national twelfth grade exams, as mean percentile, mean level (in student standard deviation units) or mean log scores. Risk sets condition on levels of past experience.