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ABSTRACT

Human capital can depreciate if skills are unused. But estimating human capital depreciation is challenging, as worker skills are difficult to measure and less productive workers are more likely to spend time in non-employment. We overcome these challenges with new administrative data on teachers' assignments and their students' outcomes, and quasi- random variation from the teacher assignment process in Greece. We find significant losses to output, as a one-year increase in time without formal employment lowers students' test scores by 0.09 standard deviations. Using a simple production model, we estimate a skill depreciation rate of 4.3% and experience returns of 6.8%.

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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, several studies find 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), changes to match quality (Neal, 1995), or cohort effects (Oreopoulos, Von Wachter and Heisz, 2012), 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. 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 0.09 student standard deviation (σ) decline in a teacher's student test scores. Using

[1] For example, the well-known German Kurzarbeit scheme subsidizes firms for part-time employment. A number of other European countries operate similar schemes to encourage firms to cut hours rather than employees. Many countries used such policies during the 2020 pandemic recession.

aggregated data covering the entire country, we estimate that a district's student test scores fall by 0.07σ per-class if the teachers' mean time without formal employment increases by a year.

There are two challenges in measuring the changes in human capital due to non-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, using administrative data from the entire country. We address the measurement issue by focusing on employees for whom we have a direct measure of output. Following a large literature (e.g., Rockoff (2004); Chetty, Friedman and Rockoff (2014)), we infer a teacher's 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 admission. We also include university enrollment outcomes.

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]

[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.

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 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: a 0.085σ decline in test scores per year. Losses are similar in the first and second semester exams, and continue to post-secondary outcomes. Students with a teacher who had an additional year without formal experience are 2.2 percentage points (3.3%) less likely to be admitted to a university.

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. These estimates are robust to within-district sorting of students to teachers. Our estimates are very similar to the estimates from the teacher-level specification: a combined depreciation and forgone experience effect of 0.068σ per class, or 1.5 percentage points in university admissions probability.

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 examine attrition, which we view as the most obvious threat. 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,

[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.

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.

The main estimates capture the combined effect of skill depreciation, independent of employment, 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. Using our district-level model, we estimate that, controlling for experience, an extra year without formal employment lowers test scores by 0.044σ per class. We specify a simple production model of student test scores and teacher human capital that relates our causal estimates to experience returns and depreciation rates. Our estimates imply a skill depreciation rate of 4.3% and experience returns of 6.8% per year.

This paper delineates a 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) estimate the effect of unemployment duration on skill measures, using a panel data fixed effects approach. Benhenda (2017) and Wiswall (2013) look at teachers' absences and career interruptions, respectively, 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 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, which is similar to that in Imai and Keane (2004). Blundell, Dias, Meghir and Shaw (2016) estimate human capital depreciation rates of 6-8%. Macroeconomic models incorporating human capital depreciation tend to use very different parameterizations, including Ljungqvist and Sargent (1998) (20% chance of losing skills), Kehoe, Midrigan and Pastorino (2019) (1.4% during a non-employment spell), Jarosch (2015) (15% depreciation rate) and Manuelli and Seshadri (2014) (21% depreciation rate). We offer a depreciation rate estimate that leverages quasi-random variation in time not working.

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. A large literature on optimal unemployment insurance (Baily, 1978; Chetty, 2006; Shimer and Werning, 2006) 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 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. The importance of policies preventing the deterioration of human capital due to workers being out of the labor force is particularly relevant at the time of writing. The COVID-19 pandemic and recession has led to historically high unemployment and furlough rates in many countries. Our estimates suggest that the resulting decline in human capital may have aggregate effects.

Finally, in the most direct sense this paper joins a literature on the returns to experience (e.g., Angrist (1990); Altonji and Williams (1992)), especially for teachers. A number of papers have estimated high experience returns early in a teacher's career that then flatten (Rockoff, 2004; Rice, 2013), while Wiswall (2013) and Papay and Kraft (2015) estimate positive returns even at high levels of experience. Herrmann and Rockoff (2012) estimate the effects of teacher absence on students, mainly driven by the productivity of the substitute teacher. Jackson, Rockoff and Staiger (2014) provide a review of the broader literature on teacher productivity.[6] This paper estimates the effect of not working on productivity and decomposes the effect into within-worker experience returns and depreciation rates. We rely on quasi-random variation that avoids the common assumption that teachers select into experience samples based only on time-invariant factors.

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 describes the data used in our analysis. Section 4 introduces our empirical strategy. Section 5 presents the student-level estimates and then Section 6 extends the analysis to the larger sample and conducts a district-level analysis. Section 7 decomposes the effects into skill depreciation and forgone returns to experience. Section 8 concludes and discusses avenues for future research.

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. Every year they have the option to remain at their previous school. Teachers appointed to temporary positions ("temporary teachers") are employed on a contract basis for up to ten months and have to re-apply through a centralized assignment system 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 classroom hours per week

[6] Other recent work has focused on measurement (Hanushek and Rivkin, 2010), information and evaluation (Rockoff, Staiger, Kane and Taylor, 2012; Taylor and Tyler, 2012), instructor characteristics (Hoffmann and Oreopoulos, 2009), match quality (Jackson, 2013), peer effects (Jackson and Bruegmann, 2009; Opper, 2019), and performance pay (Lavy, 2002, 2009).

at a standardized salary,[7] 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 classes. 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 2008 financial crisis 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 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.[8] Second, since 2009 Greece has had a hiring freeze for permanent staff. As hiring new temporary teaching staff has became the only way to cover teaching needs in schools, an increasing number of teachers have been hired on a contract basis.

[7] In practice, nearly all full-time deputy teachers teach the maximum 23 hours per week, where hours refer to classroom instruction time. Less commonly, a full-time deputy teacher could agree to work between 5 and 15 hours per week. These teachers get monthly prorated payments. Full-time permanent teachers with fewer than 6 years of experience teach 23-24 hours per week while more experienced teachers cover 20-21.

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

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.[9]

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. If graduates 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, and bureaucratic delays lead students to finish their degrees on different days, and this generates considerable variation in when students graduate, as seen in the top row of 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. Occasionally, the position will involve teaching at multiple schools in the same district.

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

[9]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.

experience or have taken a written assessment (ASEP).[10] 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.[11] 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. Lists are released publicly, and thus there is little to no scope for manipulation or changing one's order once assigned.

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 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 the prospect of eventually receiving a permanent teaching position with a high salary and job security may overcome the need to wait for the position.[12] Furthermore, many university students choose teaching degrees without expecting such a long wait. In Table A.1, we show summary statistics from an online survey, which we describe further in Section 3, of 200 current and former teachers. Just 34% of teachers were aware of the centralized assignment system at the point they were deciding on a profession; but once they entered the system, 76% report understanding the assignment process and 73% knew their exact waitlist position. The increasingly long wait times have led to large protests and politicians have recently proposed changing the system.[13]

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

[10]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 et al., 2004). The ASEP examination took place every two years until 2008, the last time it was offered (OECD, 2018).

[11]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.

[12]See Kathimerini for a discussion. Stylianidou et al. (2004) discusses the relative attractiveness of teaching to other options.

[13]See Euronews for a discussion.

income.[14] Importantly, these activities may not include taking a full-time job, which would remove a teacher's public school teaching eligibility.[15] Greece's active informal employment sector means that some teachers may still take full-time jobs that are hidden from the government. In Table A.2 we document surveyed teachers' activities while waiting for assignment. Just over half (54%) of the surveyed teachers used their time waiting to offer private lessons, 39% worked in non-education positions (e.g., as a restaurant waiter), and 33% got more formal education. A smaller number of teachers used their wait to start a family or on other activities. 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 certain forms of selective attrition would violate our identification assumptions.

Finally, if offered a position the teacher finds unattractive or unacceptable, a teacher may reject it. Rejection, however, is quite costly. The teacher is placed at the end of the waitlist and becomes ineligible for any assignments in the following two years. Rejections tend to be rare, but if they were more common and selective they might pose identification challenges.

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. Neither financial compensation nor future assignments – both where and when – depends on performance. Thus, the incentives to insure against or take investments to counteract depreciation are limited, and the available tools are directly restricted.

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

[15] This restriction prevents teachers from working at private schools, though the Greek private education sector is small at 7% enrollment share.

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 effect of experience on output 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 likely 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 actions do not affect it.

3 Data

We compiled novel 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 and a survey of current and former teachers. We provide more details about the data set construction in Online Appendix A.

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 experience 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.

The top panel of Table 1 displays summary statistics for the waitlist data, for teachers in subjects that appear on the national examinations. Nearly half of the waitlist teachers have yet to accumulate experience in the public Greek system, and prior experience in private schools or other EU countries is rare. 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. We will later compare teachers in the same subject with degree conferral dates in the same month-year. These cohorts are fairly large, with an average size of 62 teachers.

In the bottom row of Figure 1 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,[16] and the assigned school's district. We obtained these lists from 2004 – 2011, with an average of 2,491 high school teachers assigned per year. Unless a district has only a single high school, we do not know the exact school within the district that the teacher was assigned to. But districts are fairly small; the 608 districts in our sample have an average of 2.3 high schools.

[16] 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, etc. Teachers can only teach subjects that belong to their categorization.

We present summary statistics on the assigned teachers' subjects in the middle panel of Table 1. The distribution of assignments is similar to the distribution of teachers on the waitlists, with math and statistics and physics and biology having somewhat larger relative assignment shares while economics and computer sciences have lower relative assignment shares. Different regions of the country rely more on the assignment of temporary teachers. In Figure A.1 we show how the number of assignments varies geographically and note that the islands are particularly dependent on temporary teachers.

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. We convert the test scores to student standard deviation units by normalizing scores to have mean 0 and standard deviation 1 in each year.

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. These schools are distributed throughout the country and cover a diverse set of areas.

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. Teachers cover an average of 2.18 subjects and teach 2.95 classes per subject (bottom panel of Table 1). While the sample is limited to 23 schools, the more precise match between teacher and her students' outcomes will form the basis for our student-level empirical analysis.

This micro dataset's additional outcomes allow us to test for treatment effects on subjects and grades not part of the national exam. We also observe grade point averages and semester-specific test scores. Finally, we merged the sample to university admissions outcomes, which allows us to extend our analysis to teacher's effects on consequential, non-test outcomes. In our sample, 64% of students are admitted to a university and 62% to a university with an academic focus, with considerable dispersion in the selectivity of university attended (bottom panel of Table 1).

3.5 Teacher Survey

Finally, we supplement the national and micro data with an online survey, of 200 current and former teachers, we conducted from December, 2019 – January, 2020. The responses yield information on teachers' awareness and expectations of the assignment system, non-teaching activities while waiting for an assignment or once assigned, and investments made to maintain skills. We provide recruitment details and the survey text, in Greek and English, in Online Appendix B.

4 Empirical Model and Strategy

We seek to estimate the effect of a year out of the formal labor force on teacher output, as measured by student outcomes. To fix ideas, consider the hypothetical example, illustrated below, 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.

	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)

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 teachers in the same school year, but with exogenous variation in the fraction of years since degree conferral without formal employment . Specifically, we can compare teachers 1 and 2 in the same academic year – 2005-2006 – where the difference is that teacher 1 has one fewer year without formal employment and one more year of formal experience. The difference in outcomes is a relevant input for certain policymaking problems. In this section, we further characterize this estimand with a simple production model. The model serves several purposes: (a) it motivates the functional forms we use in our estimating equations; (b) it clarifies the relationship between the estimand and human capital technological parameters; and (c) it highlights the potential threats to identification. We then lay out our empirical strategy to obtain consistent estimates.

4.1 Production Model

Let i index students, j index teachers, and t index academic years. We start with a general model of student achievement and teacher human capital:

$$y_{it} = f\left(H_{jt}, \varepsilon_{it}\right) \tag{1}$$

$$H_{jt} - H_{j,t-1} = g\left(H_{j,t-1}, e_{j,t-1}\right) \tag{2}$$

where y_{it} is student i's output (e.g., test score) in year t, H_{jt} is teacher j's stock of teaching-relevant human capital in year t, $e_{j,t-1}$ is whether j accrued formal teaching experience in $t-1$, and ε_{it} captures student-year shocks. We label $f()$ as the student educational production function and $g()$ as the teacher human capital production function.

We first parameterize the teacher human capital production function. We follow Rosen (1976), Blinder and Weiss (1976), and a generalized version of Ben-Porath (1967) in assuming that $g()$ is homogeneous of degree one in the prior year's human capital:[17]

$$H_{jt} - H_{j,t-1} = \left(\gamma e_{j,t-1} - \delta\right) H_{j,t-1} \tag{3}$$

where γ is the return to experience and δ is the human capital depreciation rate.[18] Given this production function, we can derive human capital as a function of teacher j's human capital at the time of graduating from university (H_{j0}), the number of years since graduating from university (N_{jt}), and the number of prior years of experience $E_{jt} = \sum_{s=0}^{t-1} e_{j,s}$:

$$H_{jt} = (1+\gamma-\delta)^{E_{jt}} (1-\delta)^{N_{jt}-E_{jt}} H_{j0}. \tag{4}$$

The literature provides less guidance on parameterizing student achievement as a function of teacher human capital, which is typically an unobserved input. But we follow the teacher value-added literature in assuming that a teacher's impact on student outcomes is additively separable from other determinants. Specifically, we adopt a log-linear specification, which is equivalent to Cobb-Douglas if the outcome is expressed in logs:

$$y_{it} = \alpha \ln(H_{jt}) + \varepsilon_{it} \tag{5}$$

As human capital is not measured directly, we will make inference about a teacher's human capital based on her student's output. Thus, we insert Equation 4 into Equation 5 to express

[17] These papers focus on optimal human capital investments to maximize discounted lifetime earnings and their production functions characterize how human capital varies with the fraction of time spent working. Here we focus only on the technology and treat each year's work experience as binary.

[18] The literature sometimes refers to the depreciation rate as the change in human capital only when an individual is not working, or the change associated with a disruption like switching industries. We could redefine δ as being multiplied by non-employment and our model would be the same except the multiplicative change in human capital while working would be γ instead of $\gamma-\delta$.

student output as a function of the determinants of human capital:

$$y_{it} = \alpha \ln(1+\gamma-\delta)E_{jt} + \alpha \ln(1-\delta)(N_{jt}-E_{jt}) + \alpha \ln(H_{j0}) + \varepsilon_{it} \tag{6}$$

If we rearrange terms to express student output in terms of number of years since graduating and number of years not working ($U_{jt} \equiv N_{jt} - E_{jt}$), we have:

$$y_{it} = \alpha\left(\ln(1-\delta) - \ln(1+\gamma-\delta)\right)U_{jt} + \alpha \ln(1+\gamma-\delta)N_{jt} + \alpha \ln(H_{j0}) + \varepsilon_{it}. \tag{7}$$

This model, with student outcomes as a linear function of U_{jt}, will form the basis for our estimating equations, and in Section 4.2 we discuss how we identify the coefficient on U_{jt}. As the model clarifies, this target parameter combines experience returns (γ) and depreciation (δ), as well as a scale factor between human capital and student output (α). The 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. But in Section 7 we will decompose the total effect into depreciation and forgone experience channels.

Before proceeding, we make several observations about the parameter interpretations and functional forms. The return to experience, γ, captures any effect of formal experience on teaching-relevant human capital. This may include increased curricular knowledge, human capital investments complementary to being formally employed, improved time management skills, and changes in motivation or effort from working. The effect of formal experience is relative to the actions taken while not formally working, as characterized in Section 2.

Similarly, the depreciation rate, δ, captures any changes in teaching-relevant human capital independent from whether the teacher is formally employed. This may include forgetting material learned in formal education, (constant) age effects, and changes in motivation from being part of a complicated assignment system. The composite parameters (γ, δ) are relevant for characterizing the current assignment process and for considering policies that shift employment status but not the experience returns or depreciation rates. But because some of these components might change depending on policy, might involve costly actions, or might have spillover effects outside of education, we will comment on the extent to which the estimates seem to be driven by different factors. Finally, ε_{it} may capture student-year shocks as well as any transient shocks to teacher productivity independent of experience.[19]

The linear relationship we will estimate between student outcomes and teacher experience is the subject of debate in the economics of education literature. Some consensus has formed that experience returns are high for the first years of a teacher's career and then become flatter (Rockoff, 2004; Rice, 2013), while Wiswall (2013) and Papay and Kraft (2015) show that different identifying assumptions to solve the age-period-cohort problem can generate positive returns even at high levels of experience. We are hesitant to rely on this literature to guide our functional form choice because we view estimating experience returns as a result, rather than an assumption, of this paper. While we will not trace out a non-parametric experience return function, we will use exogenous variation in experience that avoids some strong assumptions in the literature.[20] That said, we address the linearity assumption in two ways. First, the papers that find a non-linear relationship between experience and student outcomes typically find that the relationship is close to linear at low levels of experience. As we describe below, our variation will be largely driven by teachers early in their careers – just 1.1% of our estimation sample has greater than 4 years of experience. Our variation thus lies in the experience interval where the literature has found linear returns. Second, we will provide robustness checks to our main results that allow student output to vary with the log of experience and that vary the curvature in the student outcome.

4.2 Empirical Model and Identification

We now turn to our empirical implementation and how we will identify the model's parameters. We would like to estimate the effect of a year without formal employment (U_{jt}) on an output measure (y_{it}) where $j = j(i)$. But if the teacher hiring process favors more productive

[19]Persistent shocks to teacher productivity would affect the returns to experience given Equation 3. Alternately, if Equations 3 and 5 were additively separable in levels, Equation 7 would still be linear in experience and ε_{it} could include persistent human capital shocks.

[20]Most of the literature employs within-teacher estimators that rule out selection into experience levels based on time-varying factors and estimates a combined experience and age effect. When we use these standard assumptions, we estimate early career experience returns of 0.04 student standard deviations per year and later career returns of 0.01 student standard deviations, though we cannot reject equality. For different identifying assumptions, Wiswall (2013) relies on the exogeneity of measured career interruptions like decisions to take time off. Herrmann and Rockoff (2012) use teacher absences before versus after student testing dates, though the parameter of interest is the change in the productivity of the classroom's teacher rather than how an individual teacher's productivity changes as her experience varies exogenously.

teachers, then regressing y_{it} on U_{jt} will not yield a consistent estimate of the causal effect. Greece's centralized assignment process provides us with useful variation in U_{jt} that we argue is unrelated to a teacher's potential productivity. Define teacher j's risk set $m = m(j)$ in year t, to be the set of teachers who had their degrees conferred in the same year-month as j, teach 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 U_{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 a high number of accrued experience credits.

Thus, we instrument for within-risk set variation in U_{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 z_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.[22]

Our empirical model is:

$$y_{it} = \beta U_{jt} + \mu_{mt} + \eta_{it} \tag{8}$$

$$U_{jt} = \lambda z_j + \theta_{mt} + \nu_{jt} \tag{9}$$

where μ_{mt} and θ_{mt} are vectors of risk set-year fixed effects. Relating Equation 8 to the production model (Equation 7),

$$\beta = \alpha\left(\ln(1-\delta) - \ln(1+\gamma-\delta)\right) \tag{10}$$

$$\mu_{mt} = \alpha \ln(1+\gamma-\delta) N_{jt} + \alpha \ln(\bar{H}_{j0}) \tag{11}$$

$$\eta_{it} = \alpha\left(\ln(H_{j0}) - \ln(\bar{H}_{j0})\right) + \varepsilon_{it} \tag{12}$$

[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.

[22] In the student empirical specification in Section 5, we will include some older cohorts of teachers that appeared on fresh graduates lists before 2003. We describe below how we impute their original waitlist position.

where the average $\ln(\bar{H}_{j0})$ is taken over teachers in risk set m in year t.

Our exclusion restriction is that normalized waitlist position is independent of unobserved determinants of outcome y_{it} 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 $\left(\mathbb{E}[z_j \ln(H_{j0})|m,t] \neq 0\right)$ or (2) waitlist position is (conditionally) correlated with student types or choices $\left(\mathbb{E}[z_j \varepsilon_{it}|m,t] \neq 0\right)$. 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 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. 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 the first column of 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 9, but replacing the left-hand-side with teacher's GPA and keeping each teacher's first waitlist observation), 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 when teachers graduate, 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. For example, 98% of the 2003 graduating cohort remain on the waitlists through 2004 but by 2010, 52% have dropped off. We explore the relationship between our instrument – fresh graduates' waitlist position conditional on degree month-year – and attrition in the middle columns of Table 2, where attrition corresponds to dropping off the waitlists before the end of the sample period and without accruing any formal experience. 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 9, 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.[23] Thus, 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.

We provide further visual evidence in Figure A.2, where we summarize attrition rates by the teacher's degree conferral day. When we control for risk set, this within-month degree conferral timing generates our identifying variation. The figure shows no clear pattern between attrition rates and time of the month.

Attrition appears to be unrelated to our instrument and also teachers' university academic achievement. In the last column of Table 2 we see that teachers with higher university grade

[23]Column 3 of Table 2 shows a negative relationship between waitlist position and attrition, when we do not control for risk set. Recent graduating cohorts are less likely to have attrited by the end of our sample and appear at the back of lists that grow each year.

point averages are no more likely to attrit when 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. One potential reason for a lack of selective attrition is that teachers are neither evaluated nor compensated on the basis of student performance. Thus, the attractiveness of remaining a teacher may be relatively unrelated to productivity.

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 with Table 3, 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. And the null effect remains when we add region-year fixed effects. Thus, teachers with different (conditional) waitlist positions do not appear to be assigned to schools in different types of districts.

Teacher assignment could still interact with student type based on within-school class assignments. If a principal realizes the new deputy teacher is unprepared, she may want to place the teacher in a classroom with a non-random set of students. This is unlikely to be an issue because Greece requires high school teachers to be assigned randomly to students (Lavy and Megalokonomou, 2020). We also provide two empirical tests to assess this threat. First, we will show that teacher time spent not working is unrelated to students' lagged grade-point average. And second, we will include a district-level specification that is robust to selection on assignment of students across teachers in the same school district. We present the results after introducing our individual and district specifications in Sections 5 and 6.

Teacher experience may directly affect ε_{it} if students take actions to compensate for a low human capital teacher. For example, students might receive additional parental tutoring or reallocate study time toward courses with inexperienced teachers. We could consider these actions as part of the effect of teacher experience on output that we seek to identify. But these actions may also incur additional costs to the system (parental time; lower output in other courses) we would not capture with a student's output in a single course. We will explore the possibility of cross-course spillovers by testing whether the time spent not working for teachers in non-tested subjects affects students' test scores.

5 Individual-Level Estimates

We estimate two versions of our empirical model, each adapted to a different level of aggregation in our data. First, we present 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. Our micro data set includes student test results for each subject. We thus extend our empirical model to a student-subject-year unit of analysis. Let i denote a student, s denote a subject, and $j = j(i,s,t)$ denote student i's teacher for subject s in year t. Let $m = m(j)$ denote teacher j's risk set. We specify our model of a student-subject exam outcome y_{ist} as:

$$y_{ist} = \beta U_{jt} + \mu_{mt} + \eta_{ist} \tag{13}$$

$$U_{jt} = \lambda z_j + \theta_{mt} + \nu_{jt} \tag{14}$$

While our micro data only has outcomes from 23 schools, we have waitlist and experience measures for the whole country. To leverage both sets of data, we implement a within transformation on our empirical model (Frisch and Waugh, 1933; Lovell, 1963; Giles, 1984). Let $\tilde{x_{ist}}$ denote x_{ist} demeaned at the risk set level ($\tilde{x_{ist}} = x_{ist} - \bar{x_{mt}}$). If we apply the within-risk set transformation, our model becomes:

$$\tilde{y_{ist}} = \beta \tilde{U_{jt}} + \tilde{\eta_{ist}} \tag{15}$$

$$\tilde{U}_{jt} = \lambda \tilde{z}_{jt} + \tilde{\nu}_{jt}. \tag{16}$$

For the model observables that do not require student-level data $(\tilde{U}_{jt}, \tilde{z}_{jt})$, 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 $\xi_{ist} = \tilde{\eta}_{ist} + \bar{y}_{mt}$ and rewrite our model as:

$$y_{ist} = \beta \tilde{U}_{jt} + \xi_{ist} \tag{17}$$

$$\tilde{U}_{jt} = \lambda \tilde{z}_{jt} + \tilde{\nu}_{jt}. \tag{18}$$

We estimate this model as our student-level specification, clustering standard errors by teacher. We construct $\tilde{U}_{jt}$ and $\tilde{z}_{jt}$ 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 $\mathbb{E}[\tilde{z}_{jt}\tilde{\eta}_{ist}] = 0$ then $\mathbb{E}[\tilde{z}_{jt}\xi_{ist}] = 0$ by construction.[24] 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 $(\tilde{z}_{jt})$ and accumulated years without formal employment $(\tilde{U}_{jt})$ for the full sample in Figure 2. 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 subsequent assignments, given how the 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 demeaned years without formal employment. We show a binscatter plot of the two variables in Figure 3. 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

[24]Note that $\mathbb{E}[\tilde{z}_{jt}\xi_{ist}] = \mathbb{E}[\tilde{z}_{jt}\tilde{\eta}_{ist}] + \mathbb{E}[\tilde{z}_{jt}\bar{y}_{mt}] = 0 + \mathbb{E}[\tilde{z}_{jt}\bar{y}_{mt}] = \mathbb{E}_{mt}[\mathbb{E}[\tilde{z}_{jt}\bar{y}_{mt}|m,t]] = \mathbb{E}_{mt}[\bar{y}_{mt}\mathbb{E}[\tilde{z}_{jt}]] = \mathbb{E}_{mt}[\bar{y}_{mt} * 0] = 0.$

waitlist's median is expected to wait almost an additional year.

We start by assessing whether within-school assignment of teachers to students might be related to a teacher's level of human capital. We estimate Equations 17 and 18 via two-stage least squares where the student outcome is her grade-point average *in the year prior to the assignment of a deputy teacher*.[25] Because the outcome realized before the deputy teacher taught the student, it is a placebo test for whether there is selective assignment of students to teachers.[26] We present the results in Appendix Table A.5 (col. 1). We find no statistically significant relationship, consistent with our identifying assumption. Given that we find no evidence of selection, we will use a student's lagged GPA as a control variable for our main analysis.

We now turn to estimating the effect of time out of formal employment on students' average exam score in a subject-year. In the first column of Table 4, 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 find a negative estimate of the effect of years without formal employment on student total exam scores, but we cannot statistically reject no effect. 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 complicates this argument, however, as assignments are driven by factors besides productivity and elements like attrition could reverse the sign of the bias. We thus introduce our reduced form and first stage estimates, in columns 2 and 3, respectively. We find a strong relationship between (demeaned) waitlist percentile and (demeaned) years without formal employment.

Columns 4 through 8 display our IV estimates, starting with the average test score during the year ("Score"). We estimate that an additional year without formal employment lowers

[25] To gain precision for the student-level analysis, we expand our sample in two ways. First, we include deputy teachers even if they graduated too long before our sample to appear on a fresh graduates list in our sample period. We use the waitlist's assignment rule – by degree conferral date – to impute original waitlist positions. Then because we do not know how many teachers were on these original fresh graduates lists, we rescale the instrument to run from 0 to 1 within risk set. Second, we include permanent teachers in the analysis and treat each as having her own risk set. In case permanent teachers teach at different types of schools, and thus violate $\mathbb{E}[\tilde{z_{jt}}\tilde{\eta_{ist}}] = 0$, we add a dummy variable for whether the teacher is a deputy teacher. These permanent teachers simply add precision to our controls, introduced below. We discuss the instrument construction in more detail in Online Appendix D.

[26] We use lagged GPA rather than lagged test scores because students in different grades often take courses in different subjects.

student test scores by 0.085 student standard deviations, or 2.5 percentiles.[27] This is a large effect – the standard deviation of teacher value-added, estimated as in Chetty et al. (2014), is 0.20 student standard deviations in our data. Thus, the effect of a year without formal employment is equivalent to 43% of the cross-sectional standard deviation. The effect appears to be strong for teachers in STEM fields, though for non-STEM fields our estimates are very imprecise (Table A.6).

Such a large effect of a year without formal employment may reflect skill loss that rebounds quickly. Teachers who spent a few years not working may forget some curricular material; but if they are able to re-learn the material quickly, then short-run estimates may overstate the impact. Further, optimal unemployment benefits depend on how skill depreciation rates vary during an unemployment spell (Shimer and Werning, 2006). We therefore break out our estimates by first and second semester tests and end-of-year exam, in columns 6 through 8. We see a very stable set of estimates that get slightly larger in magnitude for later tests. Thus, the change in teacher output is persistent for at least a year.

Students with teachers who have been out of formal employment for longer are at a large disadvantage on tests. We show similar effects on students' grade-point averages in Table A.7. We now explore whether this disadvantage carries over to post-secondary outcomes in Table 5. The Greek university admissions process takes the Panhellenic exam scores and reweights the sections to generate an admissions score. Students then submit ordered preference lists of institution-program combinations and are admitted in order of admissions score. Column 1 presents the results on the natural log of the admission score.[28] Students with less experienced high school teachers have lower admissions scores. Perhaps to compensate for these lower scores, the students include more university-programs on their preference lists (column 2), but they remain at an admissions disadvantage. Students with teachers who had an additional year without formal employment are 2.2 percentage points less likely to be admitted to a university, with a baseline admissions rate of 67% (column 3). Of this 2.2 percentage point

[27] The 2SLS standard error on years without formal employment is lower than the OLS standard error. This occurs when we cluster the errors, where there is no general result that 2SLS standard errors are necessarily higher. When we do not cluster our errors, the OLS standard errors are slightly lower than the 2SLS standard errors.

[28] The raw scores are highly skewed so we show the effect on the log score. In Online Appendix C we show effects on raw score.

effect, 1.4 percentage points comes from reduced admissions to academic university programs (column 4) while the rest consists of students not attending technical universities. On the margin of *where* a student is admitted, we see that students with inexperienced teachers are admitted to less selective institution-programs. Column 5 shows that having a teacher with one year fewer of formal experience causes the student to be admitted to an institution-program that is 2.5 percentiles lower-ranked in terms of admitted students' admissions scores. These large effects on post-secondary outcomes confirm that formal experience matters beyond the time the student is in the teacher's class. We provide more interpretation of the estimates after describing our district-level estimates.

In Online Appendix C we provide robustness checks for our student-level estimates. We explore robustness to the sample, bootstrapped standard errors, the functional form of formal experience, our use of controls, the test score units, and our measure of university selectivity.

6 District-Level Estimates

We now turn to a model of district-year mean student outcomes. We sacrifice the precise measurement of an individual teacher's multiple outputs for robustness to possible within-school or within-district assignment of students to teachers' courses depending on the type of the newly-assigned temporary teacher. We also gain external validity by extending the analysis to the national level.

We derive our district empirical specification from the same empirical model, Equations 8 and 9, 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 deputy 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 deputy teachers in tested subjects, then each of those 20 teachers will have $1/20$ as their non-zero element.[29]

We perform the within risk-set transformation on Equations 8 and 9, as in the student

[29] We implicitly assume that teachers in the same district-year have similar numbers of students. We cannot test this on the national sample, but for the 23 schools in the micro data, we find limited within-school variation in class sizes.

empirical specification. Then, we left multiply the demeaned equations by matrix A, which yields our district-level model:

$$y_{dt} = \beta \tilde{U}_{dt} + \xi_{dt} \tag{19}$$

$$\tilde{U}_{dt} = \lambda \tilde{z}_{dt} + \tilde{\nu}_{dt}. \tag{20}$$

with $\tilde{x}_{dt} = \frac{1}{N_{dt}} \sum_{j \in J_{dt}} \tilde{x}_{jt}$ for each variable x where J_{dt} is the set of deputy teachers in district d in year t and N_{dt} is the size of this set. As above, we do not demean the outcome, y_{dt}, so

$$\xi_{dt} = \tilde{\eta}_{dt} + \frac{1}{N_{dt}} \sum_{j \in J_{dt}} y_{\bar{m}(j)t}. \tag{21}$$

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. Otherwise, the waitlist position might be an appropriate instrument at the individual level but due to aggregation, might correlate with the district-level error. 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. We offer an indirect test by checking for a statistical relationship between $\tilde{z}_{dt}$ and the types of risk sets contributing the district's assignees. Specifically, we test whether $\tilde{z}_{dt}$ is related to the district-aggregated mean experience, where the mean is calculated over the risk sets that include the district's assignees. We present the test in Table A.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 4. Compared to Figure 2, the aggregation shrinks the dispersion, especially of our instrument. But with controls we still have enough variation left to generate precise causal estimates. We include district and region-year fixed effects to absorb cross-district persistent test score differences and region-level shocks and include the log of the district's mean class size as a control (demeaned by risk set).[30]

6.1 Baseline Estimates

We present our district-level estimates in Table 6.[31] Our OLS regression in column 1 yields a positive but statistically insignificant 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 can still predict differences in mean years without formal employment across district-years.[32] 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 0.34 student standard deviations (σ), or 8.9 test score percentiles.

The interpretation of the effect sizes for the district results differs slightly from the individual analysis, because students' Panhellenic test scores cover multiple subjects while the individual test scores are subject specific. Twelfth-grade students take five courses in subjects that appear on the standard Panhellenic exams. We thus divide the estimates by 5 and report the per-class estimate. Our individual estimates from Section 5 suggest a decrease of 0.085σ for students in a given course if a teacher has an additional year out of formal employment, while the district-level estimate is 0.068σ. We cannot statistically reject equality.

We find that the effects carry over to the post-secondary outcomes of the district's twelfth graders, though our estimates are less precise (columns 5-9 of Table 6). We estimate that having teachers with one additional year out of formal employment lowers the log university

[30]As in the student model, our inclusion of controls that cleave risk sets – in this case, the district and region-year fixed effects – is not fully consistent with our within risk-set transformation. We include robustness checks in Online Appendix C. Further, we find no relationship between a teacher's waitlist position and her assigned district's mean class size or mean log class size.

[31]We provide visual counterparts with binscatters for the first stage (Figure A.3) and reduced form (Figure A.4).

[32]The interpretation of the first stage differs in the aggregated specification for two related reasons. First, to increase precision in the individual analysis, we included teachers who already entered the experienced waitlists by the beginning of our data sample. This led to a more experienced sample. Second, because we included these experienced teachers, we had to use imputed waitlists positions based on the system's design, rather than actual positions. While these imputations preserve exogeneity, the scale changes because we do not know how long the original lists were as many teachers may have attrited prior to our sample period. In the individual analysis we thus calculated within risk-set waitlist percentiles while here we calculate full list percentiles.

score by 0.024, on a per-class basis. This translates into lower university admissions rates – 1.5 percentage points per-class – and enrolling at less selective institutions.

6.2 Placebo Tests

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, 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 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. This would occur even though future assignees 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.[33] We present the test in Table 7. The first column runs our baseline regression, on the sample of districts for which we have assignments the following year. The sample reduction does not change our main point estimate. The second column includes the years out of formal employment of the *following* year's assigned deputy teachers. We see no statistical relationship between future assignees' human capital and current test scores. The coefficient on current assignees' years waiting becomes less precise, but we can reject equality between the two coefficients.

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 likely correlates with waitlist percentiles among all assigned teachers, not just those in tested subjects. The

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

test also evaluates whether there are spillovers, either across teachers in the same district or from students reallocating effort across classes based on teachers' characteristics. We present the relationship between years without formal employment, instrumented with waitlist percentiles, and student test scores for teachers in untested subjects in the last two columns of Table 7. We fail to reject no statistical relationship, and we reject that the coefficient is the same as our tested subjects' coefficient (from Table 6). 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.

In Online Appendix C we provide robustness checks for our district-level estimates. We explore robustness to the sample, the functional form of formal experience, controls, the test score units, our measure of university selectivity, the aggregation matrix, and regression weighting.

6.3 Interpretation

As in the individual model, we estimate large effects of being out of formal employment. The effect of one year waiting for a formal position translates to 34% of the cross-sectional standard deviation in teacher value-added. Such large effects could hint at changes to effort. Effort certainly matters for how much student output a teacher helps create; but because the educational system has no evaluation or rewards based on a teacher's performance, we speculate that experience returns are unlikely explained by changes in effort due to incentives.[34] Perhaps a more direct effort channel comes from how teachers spend their time waiting. If teachers develop non-teaching human capital during part-time work – e.g., they open a successful tutoring business – then their teaching output may be lower once they are assigned, but this could reflect split effort between their formal work and continuing the activities they started while waiting. From the perspective of the formal educational system, the loss in teaching output remains high. But in linking the change to human capital, we might overstate the losses to the teacher's complete set of skills.

While we cannot rule out such a possibility, we note that many deputy assignments involve relocating to a new part of the country. Thus, the teachers' activities, to the extent they depend on geography, are likely to change. Second, we collected survey data on teachers' activities

[34]Changes in motivation unrelated to incentives could also constitute part of a teacher's human capital.

while working as a deputy and offer a summary in Table A.3. Over a quarter of teachers engage in part-time work *while* serving as a deputy teacher, and the most common type of work is offering private lessons.[35] A very similar percentage of teachers reports engaging in part-time work, and specifically private lessons, that they continued from their waiting period. That the propensity to offer private lessons is similar for teachers continuing and not continuing activities from their waiting period makes us skeptical that the waiting time itself is causing teachers to change how they spend their teaching time. We further find no correlation between the years a teacher waited until her first assignment and whether she worked part-time once assigned (Table A.4). We thus argue our estimates of large output loss likely reflect changes in human capital.

The output loss from the deputy assignment system is distributed unevenly. Districts differ in their reliance on deputy teachers, and the percentage of deputy teachers correlates negatively with district mean test scores. Thus, we conduct a back-of-the-envelope exercise where we estimate the distributional changes from an assignment system that leads to one fewer year of waiting on average as well as a system that eliminates all waiting. Specifically, we take the absolute value of our point estimate from column 3 of Table 6, multiply it by each district's 2010 share of teachers that are deputies, and multiply by either one year or the district's assignees' mean years waiting, depending on the counterfactual.

The actual cross-district standard deviation of test scores is 0.340σ. We estimate that eliminating one year of waiting would lower the standard deviation to 0.328σ, a slight compression of cross-district differences. In the top panel of Figure A.5, we plot how this system change would reorder districts in terms of their mean scores. The x-axis shows districts' actual rank based on mean test scores, with a higher rank better, while the y-axis shows the rank we predict from eliminating one year of waiting. Most districts remain in the same part of the distribution they started, though a non-trivial share of districts are higher up the actual test score distribution because they rely less on deputies. We then implement a more extreme counterfactual, where we eliminate all waiting in the system. Because districts differ in both their reliance on deputies and their assignees' time without formal employment, districts' test score ranks change considerably under the counterfactual.[36] In the bottom panel of Figure A.5, we

[35] This is not specific to deputy teachers. Permanent teachers regularly offer private lessons on the side.

[36] This latter source of variation – the characteristics of a district's deputies – varies mostly over time within

see that the elimination of waiting would dramatically reorder districts in terms of mean test scores. The output effects of time without formal employment thus potentially explains much of the cross-district variation in test scores.

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 as the production model in Section 4 highlighted, this effect could be driven by high levels of depreciation or by missing out on large returns to experience, and the specific mechanism matters for some 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 the motivating example given at the beginning of section 4. 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 implement this comparison 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 effects for the empirical specification. We now drop the within-year comparison and 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 $m' = m'(j,t)$.[37]

We then estimate:

$$y_{it} = \beta^c U_{jt} + \mu^c_{m'} + \eta^c_{it} \tag{22}$$

$$U_{jt} = \lambda^c z_j + \theta^c_{m'} + \nu^c_{jt} \tag{23}$$

district such that much "disadvantage" is across cohorts in the same district.

[37]We are now comparing teachers who graduated at the same time across different school years. Thus, the new risk set takes both j and t as arguments.

where we index parameters with superscript c to indicate these are conditional on experience. We again do the within-transformation, as above, but with the different risk set definition. Relating Equation 22 to the production model (Equation 6),

$$\beta^c = \alpha \ln(1-\delta) \tag{24}$$

$$\mu^c_{m'} = \alpha \ln(1+\gamma-\delta)E_{jt} + \alpha \ln(\bar{H}_{j0}) \tag{25}$$

$$\eta^c_{it} = \alpha \left(\ln(H_{j0}) - \ln(\bar{H}_{j0})\right) + \varepsilon_{it} \tag{26}$$

where the average $\ln(\bar{H}_{j0})$ is taken over teachers in risk set m'.

We present the results of the student-level model in the first two columns of Table 8. Our estimates of the effect of years waiting, conditional on experience, are large but noisy. For both test score functional forms, we estimate effects considerably larger than our baseline estimates, but the standard errors are large enough that we fail to reject no effect. Our district-level model generates considerably more precise estimates, as shown in the last two columns. We estimate a per-class drop in output, conditional on experience, of 0.044σ, or 1.41 percentiles. Thus, even controlling for experience levels, we still find that time out of formal employment matters.

To translate these district-level estimates to statements about experience returns or depreciation rates, we rely on our production model's relationship to the empirical model (Equations 10 and 24). Solving for γ and δ as functions of the empirical parameters, we have:

$$\delta = 1 - \exp(\alpha^{-1}\beta^c) \tag{27}$$

$$\gamma = \exp(\alpha^{-1}(\beta^c - \beta)) - \exp(\alpha^{-1}\beta^c) \tag{28}$$

If we normalize $\alpha = 1$ such that log human capital is expressed in per-class test score units, we estimate that $\delta = 0.043$ and $\gamma = 0.068$.[38] The net gain from a year of experience – the depreciation from a year passing plus the experience return associated with working – is 2.5% of initial human capital. Thus, experience returns are just enough to counteract fairly large human capital depreciation rates, and not working leads to large losses in human capital. Of

[38] Our individual model estimates of β^c are highly imprecise. But if we use our individual model point estimates of β and β^c, we estimate that $\delta = 0.276$ and $\gamma = 0.064$.

course, δ and γ remain composite parameters. In particular, δ includes any constant age effects. But provided the age effect is weakly positive for early-career teachers, our estimate of δ would be a lower bound on the true human capital depreciation rate.

8 Concluding Remarks

This paper demonstrates that workers become less productive if they accrue time without formal employment. We show that teachers 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 $7-9\%$ of a student standard deviation reduction in student test scores. We estimate that these effects are driven by large skill depreciation rates and that the skills gained during employment just compensate for depreciation, while periods of not working lead to large reductions in human capital. Our paper identifies a specific channel that generates structural unemployment duration dependence, which has been widely found in the literature.

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. These results are particularly important at the time of writing, given that a large portion of the workforce is furloughed or working remotely due to the COVID-19 pandemic. Our estimates suggest that the fact that many workers are not working may have long term effects on productivity, which warrants further study.

While our work demonstrates that human capital depreciates, there remain a number of important venues for further research. Our analysis has little to say about the exact type of skills that are depreciating. 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, particularly in settings that require more or less training and skill, or where workers are more readily substitutable. We thus encourage more research that collects skill measurements and assesses how these skills vary with employment status in different settings.

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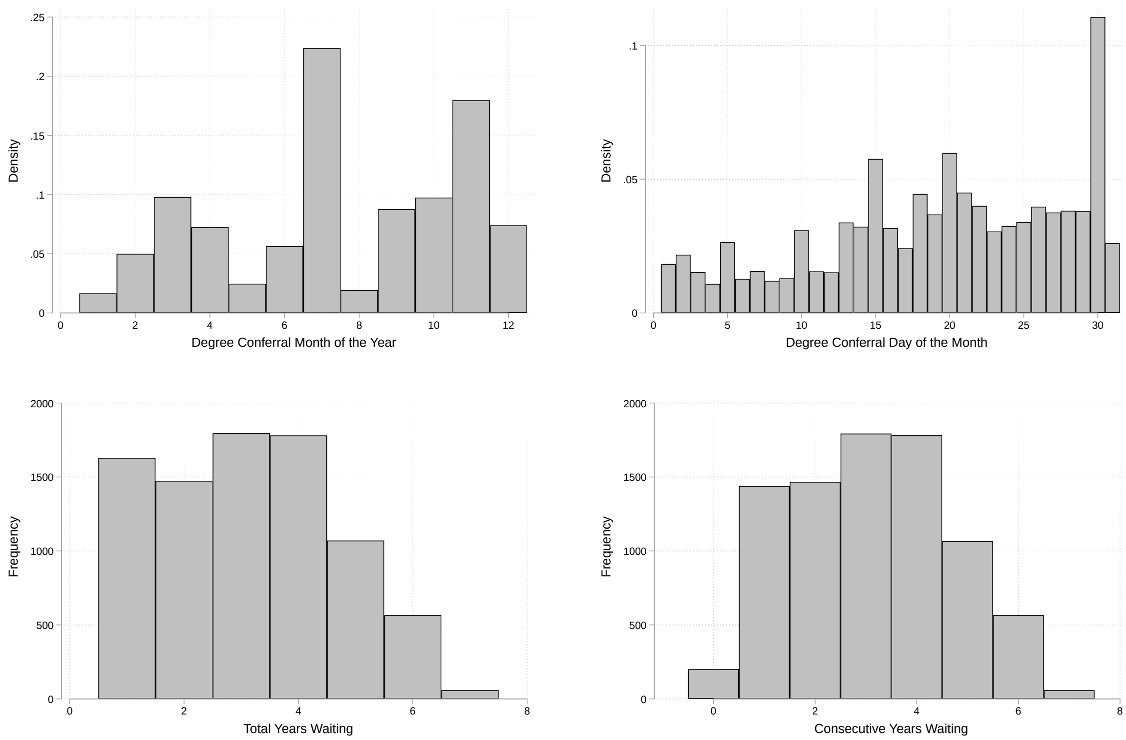
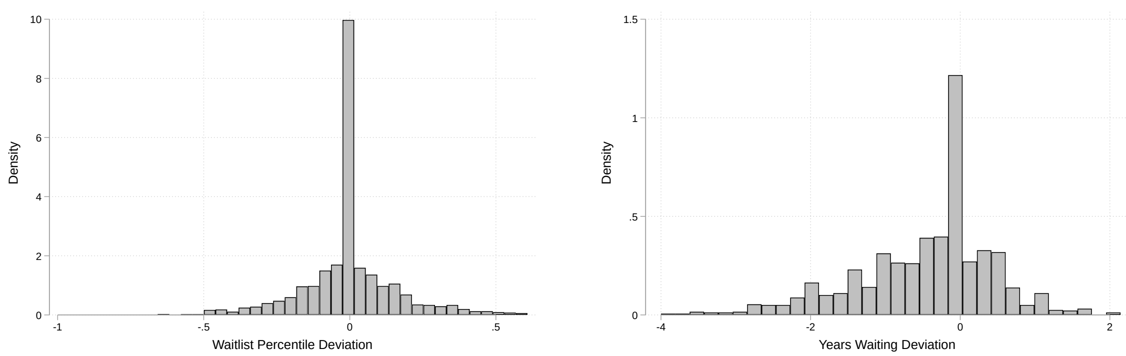
Figure 1: Degree Conferral Months and Days and Years without Formal Employment



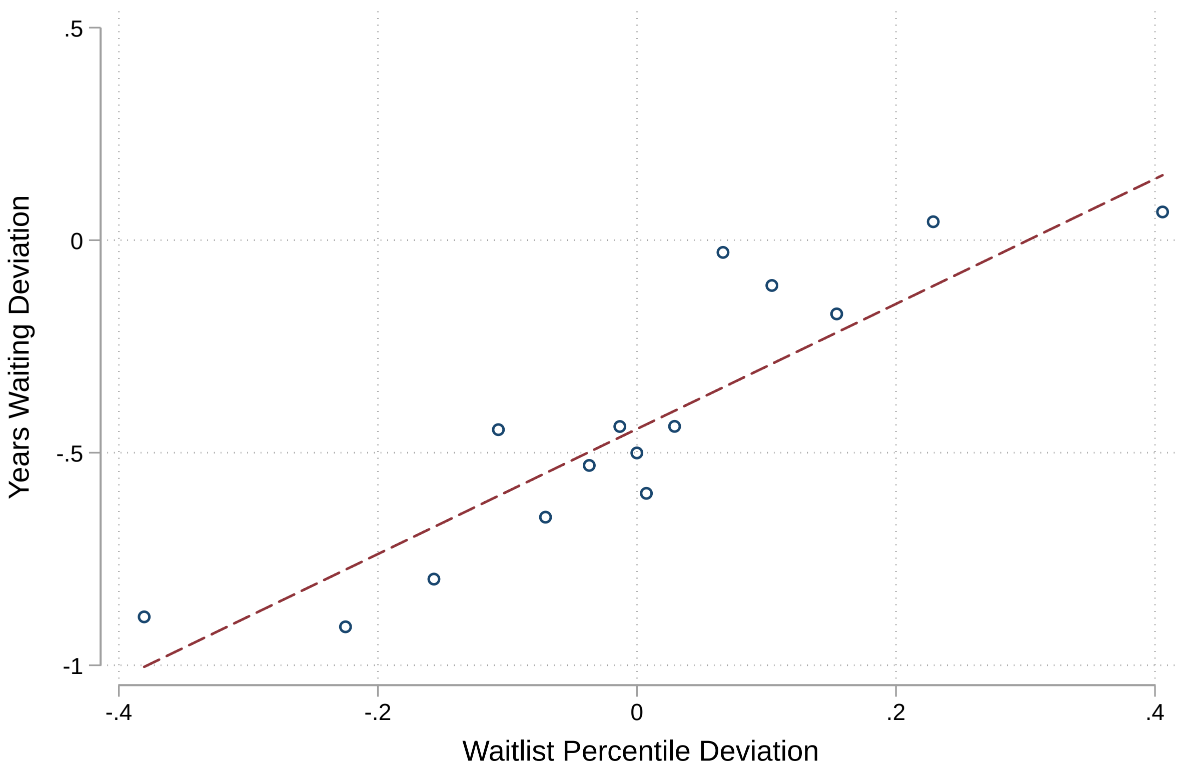
Notes: The top panel of this figure shows the histograms of the degree conferral month of the year (left) and day of the month (right). The bottom panel of this figure shows the histograms of the total years without formal employment (left) and the consecutive years without formal employment (right).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

Figure 2: Waitlist Rank and Years without Formal Employment – Deviations from Risk Set Mean



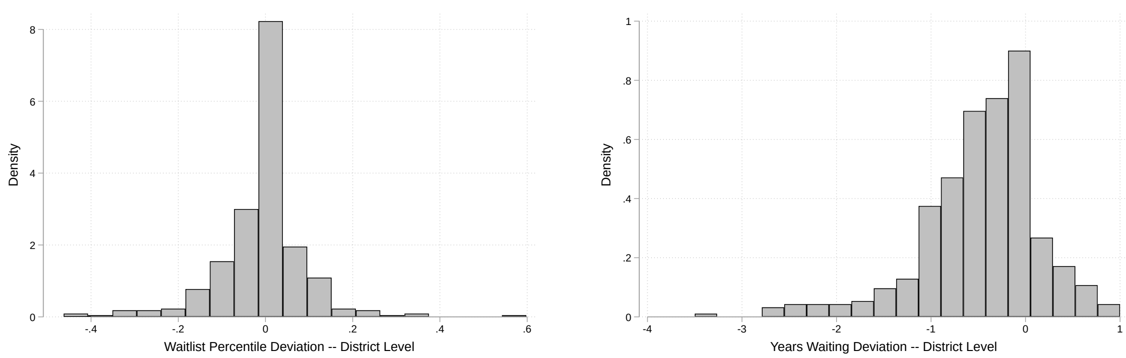
Notes: The figure shows histograms of the demeaned waitlist percentile (left) and years without formal employment (right) where the demeaning is done by risk set-year. A risk set is a degree conferral year-month and subject combination. The waitlist percentile is the initial position on the fresh graduates waitlist, normalized to vary from 0 to 1. An observation is a teacher-year. The sample includes all assigned teachers between 2003 and 2011 whose degree was conferred before 2006.

Figure 3: Years without Formal Employment and Waitlist Position – Teacher Level



Notes: The binscatter figure shows the relationship, at the teacher-year level, between demeaned waitlist percentile and demeaned years without formal employment where the demeaning is done by risk set-year. A risk set is a degree conferral year-month and subject combination. The waitlist percentile is the initial position on the fresh graduates waitlist, normalized to vary from 0 to 1. The sample includes all teachers on a waitlist between 2003 and 2011 whose degree was conferred before 2006.

Figure 4: District-Level Waitlist Position and District-Level Years Waiting – 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 2010.

Table 1: **Summary Statistics**

	Obs	Mean	Std. Dev.	Min	Max
Waitlist Data					
Degree Mark	455,105	6.71	0.69	0	10
Years Experience	455,393	1.57	1.99	0	16
Inexperienced (0 Years)	455,393	0.45	0.50	0	1
Any Private Experience	311,247	0.00	0.05	0	1
Any EU Experience	314,170	0.00	0.04	0	1
Any Permanent Experience	314,170	0.00	0.01	0	1
Greek & History	456,953	0.50	0.50	0	1
Math & Stats	456,953	0.15	0.36	0	1
Physics & Biology	456,953	0.20	0.40	0	1
Economics	456,953	0.07	0.26	0	1
Computer Science	456,953	0.07	0.26	0	1
Degree Month-Year-Subject Cohort Size	22,952	61.96	72.93	1	566
Assignment Data					
Greek & History	24,906	0.46	0.50	0	1
Math & Stats	24,906	0.21	0.41	0	1
Physics & Biology	24,906	0.26	0.44	0	1
Economics	24,906	0.03	0.16	0	1
Computer Science	24,906	0.05	0.21	0	1
Micro School Data					
Grade 10	105,237	0.23	0.42	0	1
Grade 11	105,237	0.25	0.43	0	1
Grade 12	105,237	0.52	0.50	0	1
(Teacher) Subjects Taught	1,101	2.18	1.15	1	6
(Teacher) Classes Taught	787	2.95	1.73	1	11
GPA	104,355	14.68	2.94	0	20
Subject Exam Score	82,904	12.03	5.57	0	20
Admitted	97,745	0.64	0.48	0	1
Admitted to Acad Univ	62,911	0.62	0.48	0	1
Application List Length	76,385	25.29	22.07	1	235
Univ Selectivity Percentile	62,911	48.88	28.30	0	100

Notes: The table shows summary statistics from the waitlist, assignment, and micro school data. "Degree Mark" refers to the teacher's university grade-point average, out of 10. "Degree Month-Year-Subject Cohort Size" indicates the number of teachers who had their degrees conferred in the same year, month, and subject. "Admitted" is an indicator for whether the student was admitted to university, "Application List Length" is the number of degree-institution combinations the student listed in applying to university, and "Univ Selectivity Rank" is the selectivity percentile of the university the student attended, where selectivity is measured by the mean entrance score of the enrolled students.

Table 2: University Grade Point Average and Attrition by Waitlist Position

	Teacher GPA	Teacher GPA	Attriter	Attriter	Attriter	Attriter
Waitlist Percentile	0.199***	0.00535	-0.0547***	0.00533		
	(0.0180)	(0.0331)	(0.0129)	(0.0263)		
Teacher GPA					-0.0285***	0.00677
					(0.00437)	(0.00493)
Mean DV	6.852	6.860	0.428	0.346	0.412	0.347
N	21160	20655	19244	16366	24029	21151
Risk Set	No	Yes	No	Yes	No	Yes

Notes: The table shows the relationship between teachers' university grades (out of 10), whether they attrit from the waitlists, and their waitlist position on their first fresh graduates list. Waitlist position is normalized by the list length to be in percentiles from 0 to 1. "Attriter" is an indicator for whether the teacher left the waitlists before the end of the sample and without accruing any formal experience. "Risk Set" indicates whether risk set fixed effects are included, where a risk set is a set of teachers in a single subject who have their degrees conferred in the same month-year. The sample consists of teachers with degrees conferred before 2006 who are in risk sets that generated at least one assignment during the sample period. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

Table 3: Assigned District Characteristics by Waitlist Position

	Cohort Size	UE Rate	Cohort Size	UE Rate	Cohort Size	UE Rate
Waitlist Percentile	0.645	-0.421	-0.639	-0.324	-1.063	0.000339
	(5.233)	(0.573)	(0.912)	(0.281)	(0.678)	(0.00217)
Mean DV	50.14	9.738	50.14	9.738	50.14	9.738
N	1653	1653	1653	1653	1653	1653
Year FE	Yes	Yes	Yes	Yes	No	No
District FE	No	No	Yes	Yes	Yes	Yes
Reg-Yr FE	No	No	No	No	Yes	Yes

Notes: The table shows the relationship between teachers' waitlist position, on the fresh graduates list, and the characteristics of the district the teacher is assigned to. Waitlist position is normalized by the list length to be in percentiles. "Cohort Size" is the average number of 12th grade students at a school in the district, while "UE Rate" is the local unemployment rate. "Reg-Yr FE" are region-year fixed effects where regions typically include several districts. Teachers may appear in the regressions multiple times if they have received multiple assignments during the sample period. Standard errors are clustered by teacher. $^{*}p < .1$, ** $p < .05$, *** $p < .01$.

Table 4: Effect of Years without Formal Employment on Students' Subject Exam Scores

	OLS	RF	FS	IV	IV	IV	IV	IV
	Score (σ)	Score (σ)	Years Waiting	Score (σ)	Score (Perc)	First Sem (σ)	Second Sem (σ)	Exam (σ)
Years Waiting	-0.0481			-0.0853***	-2.467***	-0.0625***	-0.0712***	-0.0848***
	(0.0339)			(0.0164)	(0.398)	(0.0204)	(0.0223)	(0.0165)
Deputy	-0.138*	-0.134	-0.397	-0.168*	-5.184*	-0.118	-0.159	-0.156*
	(0.0773)	(0.0908)	(0.442)	(0.0992)	(2.730)	(0.100)	(0.121)	(0.0899)
Prior Year GPA	0.737***	0.737***	0.000525	0.737***	21.89***	0.598***	0.645***	0.703***
	(0.00878)	(0.00878)	(0.00175)	(0.00877)	(0.223)	(0.0123)	(0.0138)	(0.00838)
Waitlist Perc		-4.050***	47.49***					
		(0.791)	(5.958)					
Mean DV	0.0355	0.0355	-0.0160	0.0355	51.01	0.110	0.0746	-0.0253
Clusters	383	383	383	383	383	383	383	381
N	54851	54851	54851	54851	54851	54841	54804	54549
Risk Set	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table includes OLS, reduced form ("RF"), first stage ("FS"), and IV regressions. An observation is a student-subject-year. "Years Waiting" is the deputy teacher's years without formal employment and "Waitlist Perc" is the imputed waitlist position, normalized by the risk set size to be in percentiles. Both variables are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. "Score" is the student's average subject-specific test score during the year. "First Sem" and "Second Sem" are the semester-specific test scores, and "Exam" is the end-of-year exam. This exam is a national exam for 11th graders before 2006 and 12th graders in all years; otherwise it is the school exam. Test results are expressed in student standard deviation units (σ) or percentiles. Standard errors are clustered by teacher. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

Table 5: Effect of Years without Formal Employment on Students' Post-Secondary Outcomes

	IV	IV	IV	IV	IV
	Ln Univ Score	List Length	Admitted	Acad Univ	Selectivity (Admission)
Years Waiting	-0.0214***	2.081***	-0.0222***	-0.0140*	-2.468***
	(0.00577)	(0.665)	(0.00709)	(0.00794)	(0.665)
Deputy	-0.0286	3.373	-0.00530	0.0228	-3.327
	(0.0228)	(2.168)	(0.0293)	(0.0314)	(2.544)
Prior Year GPA	0.243***	-3.580***	0.293***	0.307***	24.51***
	(0.00385)	(0.185)	(0.00404)	(0.00422)	(0.247)
Mean DV	9.532	25.25	0.670	0.625	49.33
Clusters	363	363	383	363	363
N	49175	59133	73352	49175	49175
Risk Set	Yes	Yes	Yes	Yes	Yes

Notes: The table includes instrumental variable estimates with (demeaned) imputed waitlist position as the instrument. An observation is a student-subject-year where the outcomes do not vary by subject but the teachers do. "Years Waiting" is the deputy teacher's years without formal employment, normalized by the risk set size to be in percentiles. "Years Waiting" and the instrument are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. "Ln Univ Score" is the natural log of the student's university admissions score. "List Length" is the number of institution-programs the student lists on her ordered list for admissions. "Admitted" and "Acad Univ" are whether the student is admitted to any university and an academic university, respectively. The non-academic university option is a technical university. For "Selectivity (Admission)" we calculate the mean university/admissions score for the class of students admitted to each university-program and order university-programs from highest to lowest. The selectivity measure is the percentile of this ordering where 100 is the program whose admits have the highest mean score. Standard errors are clustered by teacher. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

Table 6: Effect of Years without Formal Employment on Districts' Panhellenic Exam Scores

	OLS	FS	IV	IV	IV	IV	IV	IV	IV
	Score (σ)	Years Waiting	Score (σ)	Score (Perc)	Ln Univ Score	List Length	Admitted	Acad Univ	Selec.
Years Waiting	0.0911		-0.342***	-8.905***	-0.120**	1.040	-0.0740*	-0.0728	-5.645**
	(0.0971)		(0.121)	(3.008)	(0.0481)	(2.092)	(0.0380)	(0.0468)	(2.576)
Ln Class Size	0.242	-0.0538	0.239**	8.012***	0.198***	3.246*	0.0225	0.0912**	9.139***
	(0.164)	(0.232)	(0.0986)	(2.036)	(0.0535)	(1.675)	(0.0357)	(0.0393)	(2.262)
Waitlist Perc		1.785**							
		(0.834)							
Per Class	0.0182		-0.0684	-1.7810	-0.0240	0.208	-0.0148	-0.0146	-1.129
Mean DV	-0.0440	-0.155	-0.0440	49.07	9.518	25.24	0.818	0.610	48.77
N	390	390	390	390	390	390	390	390	390
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg-Yr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Risk Set	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table includes OLS, first stage ("FS"), and IV regressions. An observation is a district-year. "Years Waiting" is the deputy teacher's years without formal employment and "Waitlist Perc" is the waitlist position, normalized by the list length to be in percentiles. Both variables are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. Test score outcomes are measures of student performance on the national twelfth grade exams, in student standard deviation units (σ) or percentiles ("Perc"). "Ln Univ Score" is the natural log of the student's university admissions score. "List Length" is the number of institution-programs the student lists on her ordered list for admissions. "Admitted" and "Acad Univ" are whether the student is admitted to any university and an academic university, respectively. The non-academic university option is a technical university. For "Selec." we calculate the mean university/admissions score for the class of students admitted to each university-program and order university-programs from highest to lowest. The selectivity measure is the percentile of this ordering where 100 is the program whose admits have the highest mean score. "Per Class" indicates the per-class effect, which is the main coefficient divided by 5 for the 5 classes twelfth graders take in tested subjects. "Reg-Yr FE" are region-year fixed effects. Standard errors are heteroskedasticity-robust. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

Table 7: Placebo Tests – Future Assignments and Untested Subjects

	Future	Future	Nontested	Nontested
	Score (σ)	Score (σ)	Score (σ)	Score (Perc)
Years Waiting t	-0.337***	-0.211		
	(0.0874)	(0.214)		
Ln Class Size	0.144	0.268	0.0861***	2.162***
	(0.218)	(0.320)	(0.0191)	(0.501)
Years Waiting t+1		0.0962		
		(0.132)		
Years Waiting			-0.0158	0.555
			(0.0861)	(2.354)
Per Class t	-0.0675	-0.0422		
Per Class t+1		0.0192		
Per Class			-0.0025	0.0880
Mean DV	-0.102	-0.102	-0.0173	49.65
Test p-val		0.0031		
N	133	133	281	281
District FE	Yes	Yes	Yes	Yes
Reg-Yr FE	Yes	Yes	Yes	Yes
Risk Set	Yes	Yes	Yes	Yes

Notes: The table includes IV regressions that test for placebo effects. The "Future" columns test whether future assignments affect current scores. The sample is teachers in tested subjects and district-years with assignments the following year. The "Nontested" columns test whether teachers in nontested subjects affect scores in tested subjects. The sample in the last two columns is all teachers in subjects that are not included on the twelfth grade exams. An observation is a district-year and the outcome is the mean score on the national twelfth grade exams (in student standard deviation units or percentiles). 'Years Waiting" is the deputy teacher's years without formal employment and the instrument is the waitlist position, in percentiles, of the deputy teachers when they were on the fresh graduates list. Both variables are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. "Per Class" rows indicate the per-class effect, which is the main coefficient divided by 5 for tested subjects and 6.3 for nontested subjects. "Test p-val" tests whether the coefficients on "Years Waiting t" and "Years Waiting t+1" are equal. "Reg-Yr FE" are region-year fixed effects.$^{*}p < .1$, ** $p < .05$, *** $p < .01$.

Table 8: Effect of Years without Formal Employment on Test Scores – Controlling for Experience

	Individual	Individual	District	District
	Score (σ)	Score (Perc)	Score (σ)	Score (Perc)
Years Waiting	-0.323	-10.33	-0.218**	-7.037***
	(0.553)	(17.72)	(0.1000)	(2.565)
Deputy	0.0175	0.551		
	(0.207)	(6.049)		
Prior Year GPA	0.737***	21.90***		
	(0.00878)	(0.224)		
Ln Class Size			0.151*	5.838***
			(0.0835)	(1.611)
Per Class			-0.0436	-1.4074
Mean DV	0.0355	51.01	-0.0440	49.07
Clusters	383	383		
N	54851	54851	390	390
District FE			Yes	Yes
Reg-Yr FE			Yes	Yes
Risk Set	Incl Exp	Incl Exp	Incl Exp	Incl Exp

Notes: This table shows IV regressions of test score outcomes on years waiting without formal employment, while controlling for experience. “Years Waiting” is the deputy teacher’s years without formal employment and the instrument is the waitlist position. Both variables are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year and with the same prior years of experience. In the “Individual” columns, the sample is the set of students and teachers in our micro data set, with a student-subject-year as an observation. In the “District” columns, the sample is all school districts in Greece, with a district-year as an observation. Outcomes are subject-specific test scores (expressed in student standard deviation units or percentiles) in the “Individual” model and Panhellenic test scores in the “District” model. In the “Individual” model, standard errors are clustered by teacher. “Per Class” indicates the per-class effect in the district model, which is the main coefficient divided by 5 for the 5 classes twelfth graders take in tested subjects. “Reg-Yr FE” are region-year fixed effects. District-model standard errors are heteroskedasticity-robust. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

ONLINE APPENDIX: NOT FOR PUBLICATION

A Data Appendix

A.1 Waitlists

A.1.1 Waitlist Types

The first component of the dataset is teachers' waitlists from Greece. These waitlists rank teachers who are waiting for a school assignment as a deputy teacher (*thesi anapliroti*). They can be found on the website of the Ministry of Education under e-aitisi.sch.gr, for data starting in the 2003-04 school year. On the Ministry of Education website, there is a list of several waitlists. Two types of waitlists are relevant for our empirical analysis. These are:

1. Main lists for experienced high school deputy teachers

2. Main lists for inexperienced high school deputy teachers

The Greek translation for each one of those is "*Pinakes katataxis anapliroton deuterovathmias ekpaideusis*" and "*Pinakes katataxis anapliroton deuterovathmias ekpaideusis midenikis proipiresias*" respectively.

A.1.2 Waitlist Content

There are up to fifty subject waitlists. In the main analysis, we restrict attention to five subject categorizations that are tested on the national exams (Panhellenic). These are: PE02, PE03, PE04, PE09, and PE19. Subject PE02 is for Greek/History/Ancient Greek teachers (*filologoi*), subject PE03 is for mathematicians (including Algebra, Statistics, and Geometry), PE04 is for science teachers, subject PE09 is for Economics teachers, and subject PE19 is for Computer Science teachers. For PE04, we include four subcategorizations: PE04.01 (physics), PE04.02 (chemistry), PE04.04 (biology), and PE04.05 (geology). Usually, there is no PE04.03 list.

In each waitlist, we have a list of teachers with information on the teacher's identity (first name, last name, father's name, mother's name, date of birth), the teacher's degree (degree mark, degree date, subject of specialization), whether the teacher has received any other specialized training (Braille, foreign languages, etc.), and a teachers' past experiences, such as whether obligatory military conscription was completed, that affect eligibility. The waitlists also contain information on *moria*, which are points that teachers collect in order to obtain future assignments. Moria can be collected through various ways: academic qualifications, professional experience, social criteria, and other criteria like knowing foreign languages. We also observe *moria* experience, which refers to the number of *moria* credits accrued through professional teaching at a *moria*-eligible school after initial assignments.[39]

A.1.3 Ranking Teachers

Waitlist observations are ranked by sum of *moria* on the waitlists. This variable perfectly predicts rank on the waitlist. The only waitlist for which the sum of collected *moria* does not predict a teacher's position on the list is the main waitlist for inexperienced teachers. This is the waitlist for fresh graduates, who are ranked based on the following lexicographic ordering: (oldest) degree year, (oldest) degree month, (oldest) degree day, (highest) degree mark. Due to bureaucratic hurdles and subject-specific exam days, the degree date offers enough variation that tie-breaking with the degree mark is relatively uncommon.

Regulations for teacher assignments are governed by law 1268/1982 (25, par.12) of the Constitution, according to which: "A student is automatically announced a degree holder (thereby ceases to have a student status) following the end of the exam period during which they fulfilled the requirements of their degree completion. According to Law 1268/1982 (clause 25, paragraph 12) and the decision of the Council of State (Decision 366/1994), as well as the ensuing explanation on the relevant document from the Ministry of Education (17-5-2004, 5/45340/B3), the date of degree conferral of the degree holder is the date of announcement of the grade of the last exam by the member of teaching faculty."

[39]By definition, this information is only available for the experienced teacher lists; inexperienced teachers have yet to accrue experience.

A.1.4 Eligibility

For someone to be included in these waiting lists, the following conditions must be met: a) the applicants should be either Greek or from North-Epirus or ethnic Greeks from Constantinople and from the islands of Imbros and Tenedos (Law No. 3832 / 1958) or European Union citizens (Law No. 2431/1996); b) male applicants should 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 should submit a birth certificate and a certificate to the Ministry certifying that they are Greeks. There is no age restriction.

A.2 Assignments

The second component of our dataset is assignments. Assignment data include information on teacher identity (first name, last name, father's name and, sometimes, mother's name), the taught subject (PE02, PE03, etc.), and the teacher's assignment unit. The assignment is typically at the district level and is given by a letter and a region, e.g., A Evrou, B Evrou, A Artas, B Artas, etc. The prefix/letter refers to the regional office of the school authority of the relevant region. For a minority of teachers, the assignment data includes the specific school the teacher was assigned to.

A.3 Test Scores

We have test score data from the Ministry of Education. For the national sample, we have the composite score built from the following subjects: Greek Language, History, Mathematics, Physics, Biology, special modules, and Economics. The test score data is at the individual level, which we aggregate to the school- or district-level in our district-level model.

A.4 Individual Level Data

There are 23 schools for which we have individual test scores and other outcomes for the 10th, 11th, and 12th grade. These schools maintained an electronic archive, with data at the student-teacher-class level, which we then hand-collected. The student-level data are available for the students of all three grades for these 23 schools. The teacher-level data provides us with a teacher id for each grade, class, year, school and subject combination. The years available varies by school.

B Teacher Survey

We conducted a survey of current and former teachers between December, 2019 and January, 2020. The survey was conducted using Qualtrics, and the sample was drawn using advertisements in Facebook groups for Greek teachers, as well as internet forums for Greek teachers. The survey asked respondents about their perceptions and experiences with the waitlist system and process, as well as the activities they engaged in while waiting and once assigned. Respondents were offered a small Amazon gift card for participation, though take-up of the gift card was very low.

We attach the survey, plus an English translation, to this appendix. Table A.1 shows summary statistics for selected survey variables. Approximately three-quarters of teachers are aware of their waitlist positions and understand the assignment process, but approximately just one-third of teachers understood the assignment process when they decided to become a teacher. Only 12% of teachers ever rejected an assignment. Figure A.6 shows the distribution of the number of years on waitlists.

The survey examined what teachers did while on the waitlists, and if they continued any activities once assigned. Table A.2 shows the activities in which individuals participated while waiting. Approximately one-half of teachers gave private lessons. Nearly 40% of teachers worked in a non-education position while 19% worked in the education sector but in a non-teaching position. One-third of teachers continued with studies and 16% started a family. Many teachers reported engaging in multiple activities while waiting.

Table A.3 explores jobs teachers engaged in while working as a deputy teacher, and whether these activities had started when the teacher was on the waitlist. The top-row shows that around 30% of teachers engage in part-time work, while teaching in the public system. Among these teachers, the most common form of part-time work is offering private lessons, though small fractions of teachers report working in other industries. The fraction of teachers continuing an activity they started while waiting is quite similar. Conditional on continued activities, private lessons is by far the most common.

Table A.4 regresses whether a teacher participated in an activity or continued part time work on a teacher's years waiting for her first assignment. In the left column the dependent variable is an indicator variable for whether the teacher participated in an extra activity, and in the right column the dependent variable is an indicator variable for whether the teacher participated in an extra activity, while working as a public school teacher, that had been started while the teacher was waiting for an assignment. The results indicate that there is no statistically significant relationship between years waiting and either outcome. The point estimates are quite small as are the robust standard errors, so we can rule out even a small correlation between years waiting and participating in an activity or continuing an activity once assigned.

Default Question Block

The University of Chicago, Electronic Description and Participation Agreement in the survey
Number of the Survey: 19-1614
Title of the Survey: Human Capital Depreciation
Researchers: Michael Dinerstein, Rigissa Megalokonomou, Constantine Yanellis

Description: We are academics at the University of Chicago and Queensland conducting research on Greek teachers. We want to ask you some questions about your experience, and what you did while waiting as a teacher. This will help us with our research. The survey should only take 10 minutes, your responses are completely anonymous.
Reward: If you finish the survey, we will offer you a $5 Amazon gift card that will be sent to the email address that you will provide us with.

Contact and Questions: You can only take the survey once. Please answer all the questions. If you have any question about the survey, please email us: spyridon.kypraios@chicagobooth.edu

We really appreciate your input!

Participation Agreement

- ○ Yes, I agree to participate in the survey
- ○ No, I don't agree to participate in the survey

Section 1: Background Info

Note:
At the end of the multiple-choice questions exist a text in order to write anything you want further

1.1 Did you start from the first day as a permanent or deputy teacher?

- Yes, I have started as permanent teacher from the 1st day
- No, I have started as deputy teacher from the 1st day
- I haven't started to work in a school yet
- None of the above

1.2 The position which you have is a full time, part-time or hourly paying position?

- Full time
- Part time
- Hourly paying
- None of the above

1.3 In what year did you get your teaching degree?

1.4 What is/was your teaching categorization (e.g. ΠΕ70, ΠΕ03, ΠΕ02 etc)?

1.5 If you have started to teach in a school, in what year did you receive your first deputy assignment?

1.6 If you ever left teaching, how many years post-degree did you?

1.7 What was the main reason you became a teacher?

Section 2: Understanding and Expectations of the System

2.1 If you were in the system with the waitlists. Did you know your waitlist position?

- Yes, I knew it
- No, I didn't know it
- None of the above

2.2 Did you understand the assignment process?

- Yes, I did
- No, I didn't
- None of the above

2.3 How long did you expect to wait for an assignment?

2.4 Were you aware of the waitlist and assignment systems when you chose to become a teacher?

- Yes, I was
- No, I wasn't
- None of the above

2.5 When you were on the waitlists, did ASEP performance affect position?

- Yes, it did
- No, it didn't
- None of the above ______

Section 3: Time Spent Waiting

3.1 While waiting for assignments, which of the following did you do? Check all that apply.

- Teaching in private lessons
- Occupation in another field of a none education sector
- Further studies
- Teaching in private school
- Create a family
- Occupied is non-teaching position related to education sector
- Other ______

3.2 What was your main motivation in choosing how to spend time waiting?

- Economic factors
- Improving skills
- Other ______

3.3 How did you support yourself financially? Check all that apply.

- Support by the family
- Support by spousal income
- Income from part time job
- Income from occupation in another field of a none education sector
- Other ______

3.4 Did you take any measures to improve your waitlist position (e.g., take an exam)?

Section 4: Attrition and Rejecting Assignments

4.1 Did you consider and/or end up leaving the system?

- ○ Yes, I did
- ○ No, I didn't
- ○ None of the above

4.2 (If you left) How long did you consider leaving the system before you did?

- ○ Before I have started to work in the school
- ○ While you were waiting in the waitlist
- ○ While you were working in school as a deputy teacher
- ○ While you were working in school as a permanent teacher
- ○ None of the above

4.3 What were the relevant considerations in making this decision? (more than one choice)

- ☐ Economic reasons
- ☐ Family reasons
- ☐ Physiological reasons
- ☐ Distance from the residency
- ☐ Uncertainty
- ☐ None of the above

4.4 Did you consider rejecting an assignment?

- ○ Yes, I had rejected

- No, I hadn't rejected
- None of the above

4.5 What were the relevant considerations in making this decision?

- Economic reasons
- Family reasons
- Physiological reasons
- Distance from the residency
- Uncertainty
- None of the above

Section 5: Job Characteristics Once Assigned

(Answer for your first assignment)

5.1 Did you choose the district you ended up at?

5.2 Did you choose the school(s) you worked at?

5.3 Did you choose the classes(e.g, A1, A2 or A3 etc) you taught?

5.4 Did you choose the grades (e.g. A’, B’ or Γ’ of Primary school or A’, B’ or Γ’ of High school) you taught?

5.5 What categorization did you teach? Was this subject on the national exam?

5.6 How many schools did you work in?

5.7 What were the main skills you developed while teaching (if any)?

5.8 While you were working in a school, simultaneously you were working somewhere else?

- Yes,
Please, define the kind of occupation
- No

5.9 Did you continue an unofficial work-- which you had started while you were waiting in the waitlist-- when you took a permanent position in a school?

- Yes,
Please, define the kind of occupation
- No

Section 6: Assessment of skills most affected

6.1 While waiting, did you feel like you lost skills?

- Yes, I think this
- No, I don't think this
- None of the above

6.2 If so, which types of skills?

6.3 Did you take any steps to maintain skills? (e.g., attend workshops)

- Yes, I did

Please, define the kind of steps

- No, I didn't
- None of the above

Section 7: Miscellaneous and Open-ended Questions

7.1 Do you think teaching quality matters for national exam scores?

7.2 Do you consider the end of year tests important?

7.3 What are the incentives to do well as a teacher?

7.4 Anything else you want us to know about the system?

Είσαι Έλληνας/νίδα Εκπαιδευτικός; Κάνε κλικ ώστε να λάβεις μέρος στην έρευνα κα

Πανεπιστήμιο του Σικάγο, Ηλεκτρονικό Έντυπο Περιγραφής και Συμμετοχής στην Έρευνα

Αριθμός Έρευνας:

Τίτλος Έρευνας: Η Απαξίωση του Ανθρώπινου Κεφαλαίου

Ερευνητής/τές: Μίκαελ Ντίνεστέϊν Ρήγισσα Μεγαλοκονόμου Κωνσταντίνος Γιαννέλης

Περιγραφή: Είμαστε Ακαδημαϊκοί από το Πανεπιστήμιο του Σικάγο (The University of Chicago) και το Πανεπιστήμιο του Κουίνσλαντ (The University of Queensland, Australia). Διεξάγουμε μια έρευνα για τους Έλληνες-Ελληνίδες Καθηγητές/τριες και Δασκάλους/λες. Θα θέλαμε να συμπληρώσετε κάποιες ερωτήσεις σχετικά με την εμπειρία σας και το τι κάνατε ή κάνετε από την αποφοίτηση σας από το Πανεπιστήμιο μέχρι να ξεκινήσετε να εργάζεστε σε σχολείο για πρώτη φορά. Η διάρκεια του ερωτηματολογίου δεν θα ξεπεράσει τα 10 λεπτά, και η συμπλήρωση του ερωτηματολογίου γίνεται **τελείως ανώνυμα.**

Ανταμοιβή: Αν πληροίτε τα κριτήρια και συμπληρώσετε την έρευνα θα σας δοθεί δωροεπιταγή Amazon αξίας $5 ($5 Amazon gift card). Λαμβάνοντας μέρος σε αυτή την έρευνα, ίσως να μη σας ωφελήσει προσωπικά, αλλά εμείς θα πάρουμε χρήσιμες πληροφορίες που στο μέλλον θα βοηθήσουν άλλους εκπαιδευτικούς. Οι απαντήσεις σας είναι εμπιστευτικές και αφότου σας αποστείλουμε τη δωροεπιταγή, θα διαγράψουμε κάθε προσωπική σας πληροφορία.

Επικοινωνία & Ερωτήσεις: Μπορείτε να συμπληρώσετε την έρευνα μόνο μια φορά. Παρακαλώ να απαντήσετε σε όλες τις ερωτήσεις. Αν έχετε κάποια ερώτηση σχετικά με την έρευνα, παρακαλώ να μας στείλετε ηλεκτρονικό μήνυμα (email) : spyridon.kypraios@chicagobooth.edu

Συγκατάθεση Συμμετοχής:

○ Συμφωνώ να συμμετάσχω στην έρευνα

- Δεν συμφωνώ να συμμετάσχω στην έρευνα

Μέρος 1: **Βασικές Πληροφορίες**

Επισήμανση:
Τα πλαίσια στο τέλος των ερωτήσεων επιλογής υπάρχουν ώστε να συμπληρώσετε αν θέλετε κάτι παραπάνω

1.1 Ξεκινήσατε απο την πρώτη μέρα να εργάζεστε ως μόνιμος εκπαιδευτικός ή ως αναπληρωτής;

- Ναι, ξεκίνησα να εργάζομαι ως **ΜΟΝΙΜΟΣ** εκπαιδευτικός απο την 1η μέρα
- Όχι, ξεκίνησα να εργάζομαι ως **ΑΝΑΠΛΗΡΩΤΗΣ** εκπαιδευτικός απο την 1η μέρα
- Δεν έχω ξεκινήσει να εργάζομαι προς το παρόν
- Κανένα από τα παραπάνω

1.2 Η θέση που κατέχετε είναι πλήρης, μερικής ή ωρομίσθιας απασχόλησης;

- Πλήρης απασχόλησης
- Μερικής απασχόλησης
- Ωρομίσθιας απασχόλησης
- Κανένα απο τα παραπάνω

1.3 Ποιο έτος λάβατε το πτυχίο σας από το πανεπιστήμιο;

▾

1.4 Ποιά είναι η ειδίκευση σας όπως προσδιορίζεται από τον κωδικό σας (για παράδειγμα ΠΕ70, ΠΕ03, ΠΕ02 κλπ);

1.5 Αν έχετε ήδη εργαστεί σε σχολείο, ποιο έτος εργαστήκατε για πρώτη φορά ως αναπληρωτής/τριά καθηγητής/τριά;

▼

1.6 Εαν κάνατε σπουδές στα παιδαγωγικά, αλλά αργότερα αποφασίσατε να αλλάξετε κλάδο, πόσα χρόνια αφού πήρατε το πτυχίο σας πήρατε αυτή την απόφαση;

1.7 Ποιος είναι ο κύριος λόγος που ασχοληθήκατε με το λειτούργημα του δασκάλου/ας;

Μερος 2: Κατανόηση και προσδοκίες από το σύστημα εκπαίδευσης

2.1 Εάν ήσασταν στο σύστημα με τις λίστες αναμονής για τη πρόσληψη αναπληρωτών, γνωρίζατε την θέση που κατείχατε στον πίνακα κατάταξης/αναμονής για τοποθέτηση σε σχολείο μετά από κάθε περίοδο νέων προσλήψεων;

- Ναι, γνώριζα
- Όχι, δεν γνώριζα
- Κανένα απο τα παραπάνω

2.2 Θεωρείται ότι είχατε κατανοήσει την διαδικασία τοποθέτησης σε σχολείο;

- Ναι, την είχα κατανοήσει
- Όχι, δεν την είχα κατανοήσει
- Κανένα από τα παραπάνω

2.3 Πόσο χρόνο περιμένατε στον πίνακα κατάταξης/αναμονής αναπληρωτών μηδενικής προϋπηρεσίας για να αρχίσετε να εργάζεστε σε σχολείο για πρώτη φορά;

▼

2.4 Θεωρείται ότι ήσασταν ενημερωμένος/η για τη διαδικασία με τις λίστες αναμονής και το σύστημα διορισμού όταν επιλέξατε να γίνεται δάσκαλος/α;

- Ναι, ήμουν ενημερωμένος/η
- Όχι, δεν ήμουν ενημερωμένος/η
- Κανένα από τα παραπάνω

2.5 Όταν ήσασταν στον πίνακα κατάταξης/αναμονής αναπληρωτών **μηδενικής προυπηρεσίας**, επηρέασαν τα μόρια του ΑΣΕΠ (εάν είχατε) την θέση σας στον πίνακα;

- Ναι, την επηρέασαν
- Όχι, δεν την επηρέασαν
- Κανένα από τα παραπάνω

Μέρος 3: Χρόνος Αναμονής

3.1 Όσο χρόνο περιμένατε ώστε να εργαστείτε για πρώτη φορά ως αναπληρωτής/τρια, τι από τα παρακάτω κάνατε; (Μπορείτε να σημειώσετε περισσότερες από μια απαντήσεις)

- Διδασκαλία σε κάποιο ιδιωτικό φροντιστήριο
- Παραδίδατε ιδιαίτερα μαθήματα
- Διευρύνατε τις σπουδές σας με κάποιο νέο τίτλο σπουδών
- Ξεκινήσατε μια οικογένεια
- Εργαστήκατε σε κάτι άλλο σχετικό με τον τομέα της εκπαίδευσης
- Εργαστήκατε σε κάτι άλλο, ΜΗ σχετικό με τον τομέα της εκπαίδευσης

- ☐ Άλλο (Να το αναγράψετε): ______

3.2 Ποιο ήταν το βασικό κίνητρο σας ώστε να επιλέξετε αυτό που κάνατε όσο χρόνο περιμένατε;

- ○ Οι οικονομικές απολαβές
- ○ Διεύρυνση των δεξιοτήτων
- ○ Άλλο (Να το αναγράψετε): ______

3.3 Πως υποστηρίζατε τον εαυτό σας οικονομικά όσο χρόνο περιμένατε; (Μπορείτε να σημειώσετε περισσότερες από μια απαντήσεις)

- ☐ Με την υποστήριξη της οικογένειας
- ☐ Με την υποστήριξη του εισοδήματος του/της συζύγου
- ☐ Με το μισθό από εργασία μερικής απασχόλησης
- ☐ Με το μισθό από εργασία μη σχετική με την εκπαίδευση
- ☐ Άλλο (Να το αναγράψετε): ______

Κάνατε κάποια ενέργεια ώστε να βελτιώσετε τη θέση σας στη λίστα αναμονής και να αυξήσετε τα μόρια σας (για παράδειγμα: δώσατε κάποιες εξετάσεις) ;

______

Μέρος 4: Αποχώρηση ή απόρριψη θέσης

4.1 Σκεφτήκατε να αλλάξετε κλάδο εργασίας και να βρείτε δουλειά σε άλλο κλάδο;

- ○ Ναι, το σκέφτηκα
- ○ Όχι, δεν το σκέφτηκα
- ○ Κανένα από τα παραπάνω ______

4.2 Αν ναι, σε ποιο στάδιο της διαδικασίας;

- Πριν αρχίσω να εργάζομαι σε σχολείο
- Όσο χρόνο περίμενα στη λίστα αναμονής
- Κατά τη διάρκεια εργασίας μου σε σχολείο ως αναπληρωτής/τρια
- Κατά τη διάρκεια εργασίας μου σε σχολείο ως μόνιμος/η
- Κανένα από τα παραπάνω

4.3 Ποια στοιχεία σας οδήγησαν σε αυτή την απόφαση;

- Οικονομικοί λόγοι
- Οικογενειακοί λόγοι
- Ψυχολογικοί λόγοι
- Απόσταση από την κατοικία
- Αβεβαιότητα
- Άλλοι λόγοι

4.4 Είχατε απορρίψει κάποια προσφορά θέσης αναπληρωτή/τριάς;

- Ναι, είχα απορρίψει
- Όχι, δεν είχα απορρίψει
- Κανένα από τα παραπάνω

4.5 Ποια στοιχεία σας οδήγησαν σε αυτή την απόφαση;

- Οικονομικοί λόγοι
- Οικογενειακοί λόγοι
- Ψυχολογικοί λόγοι
- Απόσταση από την κατοικία
- Αβεβαιότητα
- Άλλοι λόγοι

Μέρος 5: Χαρακτηριστικά της εργασίας ως καθηγητής/τριά

(Απαντήστε σύμφωνα με τις συνθήκες της πρώτης φοράς που εργασθήκατε)

5.1 Είχατε επιλέξει την περιοχή που σας προτάθηκε η θέση εργασίας;

5.2 Είχατε επιλέξει το/τα σχολείο/α που σας προτάθηκε η θέση εργασίας;

5.3 Είχατε επιλέξει τα τμήματα (Α1, Α2 ή Α3) που σας προτάθηκαν να διδάξετε;

5.4 Είχατε επιλέξει τις τάξεις (για παράδειγμα, Α', Β' ή Γ' Δημοτικού, ή Α', Β' ή Γ' Λυκείου) που σας προτάθηκαν να διδάξετε;

5.5 Τι μάθημα/ματα διδάξατε; Ήταν μάθημα/τα που εξετάζονταν στην διαδικασία των Πανελληνίων εξετάσεων;

5.6 Σε πόσα σχολεία εργαζόσασταν ταυτόχρονα;

5.7 Ποιες ήταν οι σημαντικότερες δεξιότητες που αποκτήσατε όσο διδάσκατε και γιατί;

5.8 Ενώ εργαζόσασταν σε κάποιο σχολείο, εργαζόσασταν ταυτόχρονα και κάπου αλλού;

- Ναι,
Παρακαλώ προσδιορίστε το είδος της δουλειάς:
- Όχι

5.9 Συνεχίσατε να κάνετε κάποια ανεπίσημη δουλειά--την οποία ξεκινήσατε να κάνετε όσο περιμένατε στη λίστα αναμονής--όταν τελικά διοριστήκατε σε κάποιο σχολείο;

- Ναι,
Παρακαλώ προσδιορίστε το είδος της δουλειάς:
- Όχι

Μέρος 6 : Αξιολόγηση των δεξιοτήτων

6.1 Όσο χρόνο περιμένατε στους πίνακες/λίστες αναμονής, θεωρείται ότι χάσατε κάποιες από τις δεξιότητες που είχατε αποκτήσει;

- Ναι, θεωρώ οτι έχασα
- Όχι, θεωρώ οτι δεν έχασα
- Κανένα από τα παραπάνω

6.2 Αν ναι, ποιο τύπο δεξιοτήτων;

6.3 Όσο χρόνο περιμένατε κάνατε κάποια ενέργεια ώστε να διατηρήσετε τις δεξιότητες σας; Αν ναί, τι κάνατε;

- Ναι, έκανα.
Παρακαλώ συμπληρώστε τι ενέργειες κάνατε :
- Όχι, δεν έκανα
- Κανένα από τα παραπάνω

Μέρος 7 : Διάφορες Ερωτήσεις Ανοιχτού Τύπου

7.1 Θεωρείτε ότι η ποιότητα της διδασκαλίας διαδραματίζει ρόλο στα αποτελέσματα των μαθητών στις Πανελλήνιες εξετάσεις;

7.2 Θεωρείτε σημαντικά τα διαγωνίσματα στο τέλος της χρονιάς;

7.3 Ποια ήταν τα κίνητρα που σας ώθησαν να αποδώσετε καλύτερα στον ρόλο σας ως εκπαιδευτικός;

7.4 Οτιδήποτε άλλο που θεωρείται σημαντικό για το σύστημα και θα θέλατε να γνωρίζουμε;

C Robustness Checks

In this appendix we provide a variety of robustness checks around our individual- and district-level estimates of the causal effect of a year without formal employment on student outcomes.

C.1 Individual-Level Estimates

C.1.1 Controls

In Table A.5 (column 2), we present our main results using demeaned lagged GPA where the teacher-year mean is removed. Because we do not observe students' lagged GPA for every teacher in the country, we cannot demean by risk set, as would be consistent with our model. But we find that demeaning by a finer level – the teacher-year mean – leaves our main point estimate essentially unchanged.

C.1.2 Functional Forms

As discussed in Section 4, the economics of education literature sometimes argues that returns to experience are declining at higher levels of experience. Further, test score units do not have a standard conversion rate to teacher human capital measures. We thus offer variations on our main specification where we include log test scores and log years without formal employment. We present the results in Table A.9. We find strong effects regardless of the functional form. One year waiting leads to a 4.4% drop in students' test scores (column 2), while a 10% increase in time waiting corresponds to a 0.13σ effect on students' test scores (column 3). For the log-log specification, we estimate an elasticity of student test scores with respect to years waiting of -0.66 (column 4).

C.1.3 Standard Errors

When we demean using risk sets, the mean has a sampling distribution. We do not account for this in our main estimates because risk sets are large enough that sampling variation in the mean is likely to be second order. Here we incorporate such sampling variation by bootstrapping our estimates. First, we sample from the full sample of teachers, to calculate risk set means. Then we run 500 wild clustered bootstrap iterations, where we sample in the instrumental variable analysis according to the Rademacher distribution and use the same draw for all observations in a cluster and for first stage and second stage residuals. We construct a bootstrapped estimate for our main estimate (corresponding to Table 4, column 4). The bootstrapped standard error is 0.025. The 95% bootstrapped confidence interval is $(-0.153, -0.051)$, compared to $(-0.117, -0.053)$ in Table 4.

C.1.4 Sample

We argue that within-month variation in degree conferral is orthogonal to teacher type and plotted the distribution in Figure 1. But the distribution is not uniform either, with a peak on the 30th of the month. We also see a peak in within-year degree conferral in July. We confirm that our results are not sensitive to these degree months and days by dropping teachers with degree conferrals on the 30th of the month and then by dropping teachers with degree conferrals in the month of July. We present the results in Table A.10.

C.1.5 Outcomes

The results are robust to different functional forms of our outcomes. In Table A.11 we show causal effects on unstandardized test scores, log test scores, and raw university score. We also include several variations in calculating an institution-program's selectivity. In the main analysis, we calculated selectivity based on enrollees' mean university scores. Here we show selectivity based on enrollees' mean national exams scores, which are a different weighting than the university admissions scores. In both cases, these selectivity measures are means across multiple years, including years in our sample. To avoid any concerns of our sample affecting the selectivity measures, we also include selectivity measures derived from 2003 admissions outcomes only.

C.2 District-Level Estimates

C.2.1 Controls

We present estimates that vary our use of controls in Table A.12. In the first column, we show estimates where we residualize all fixed effects by risk sets. The point estimate is very similar to our main result. The second and third columns add additional (demeaned) district-level controls. The point estimates are similar and move closer to our individual-model estimates.

C.2.2 Functional Forms

As with the individual-level analysis, we vary the functional forms and present the results in Table A.13.

C.2.3 Sample

In Table A.14, we present the results dropping teachers with degree conferrals on the 30th of the month and then dropping teachers with degree conferrals in the month of July.

C.2.4 Outcomes

In Table A.15, we show the causal effects on the other test or selectivity outcomes.

C.2.5 Scaling and Weighting

In the district-level model, we use an aggregation matrix, which included all deputy teachers, regardless of whether we observe them on an inexperienced list in our sample period. For these teachers, we consider them part of their own risk sets so that their years not working do not identify the causal estimates. In Table A.16, we explore the sensitivity of our results to including these additional deputy teachers. We find similar estimates that are slightly smaller in magnitude to our main estimates. Given all deputy teachers factor into a district's teaching, the attenuation toward zero is consistent with this specification including classical measurement error.

As the aggregation of an individual-level model, we estimate our district-level model weighting by the number of students in each district-year. In Table A.17, we show how the results change with different weightings. The alternate weightings lead to somewhat more negative point estimates.

D Construction of Instrument

In this appendix we provide more details on how we construct our instrument, as it varies across our individual- and district-level specifications. Even though it reverses the paper's order, here we start with the district-level instrument as it is more straight-forward.

For the district-level specification, we construct our instrument, z_j, as a teacher's actual waitlist position on the first *inexperienced* waitlist she appears on. For example, consider a teacher who earns her degree in 2003 and immediately enters the 2003 inexperienced waitlist. She does not get assigned in 2003 and thus moves up the inexperienced waitlist for 2004. She gets assigned in 2004, teaches for 10 months, and then enters the experienced waitlist for 2005, after which she waits a year and gets assigned in 2006. This teacher had different waitlist positions over the four years that spanned two different lists. We use her 2003 waitlist position – the earliest one on the inexperienced waitlist – as exogenous variation.

But position is a somewhat noisy measure of assignment probability given that the lists' churn rate varies across subjects. We thus normalize waitlist position by the length of the list the teacher is on. Suppose our example teacher's 2003 waitlist position is 250 on a list of length 500. Then we calculate her normalized waitlist position as 0.5. The teacher's risk set includes the teachers in the same subject whose degrees were conferred in the same year-month. These teachers will have normalized waitlist positions near 0.5 as well, such that the within risk-set range in normalized waitlist position may be considerably less than 1. Finally, new teachers appear on full-time and hourly lists. We take the minimum position across these lists as that is the position more likely to generate an assignment.

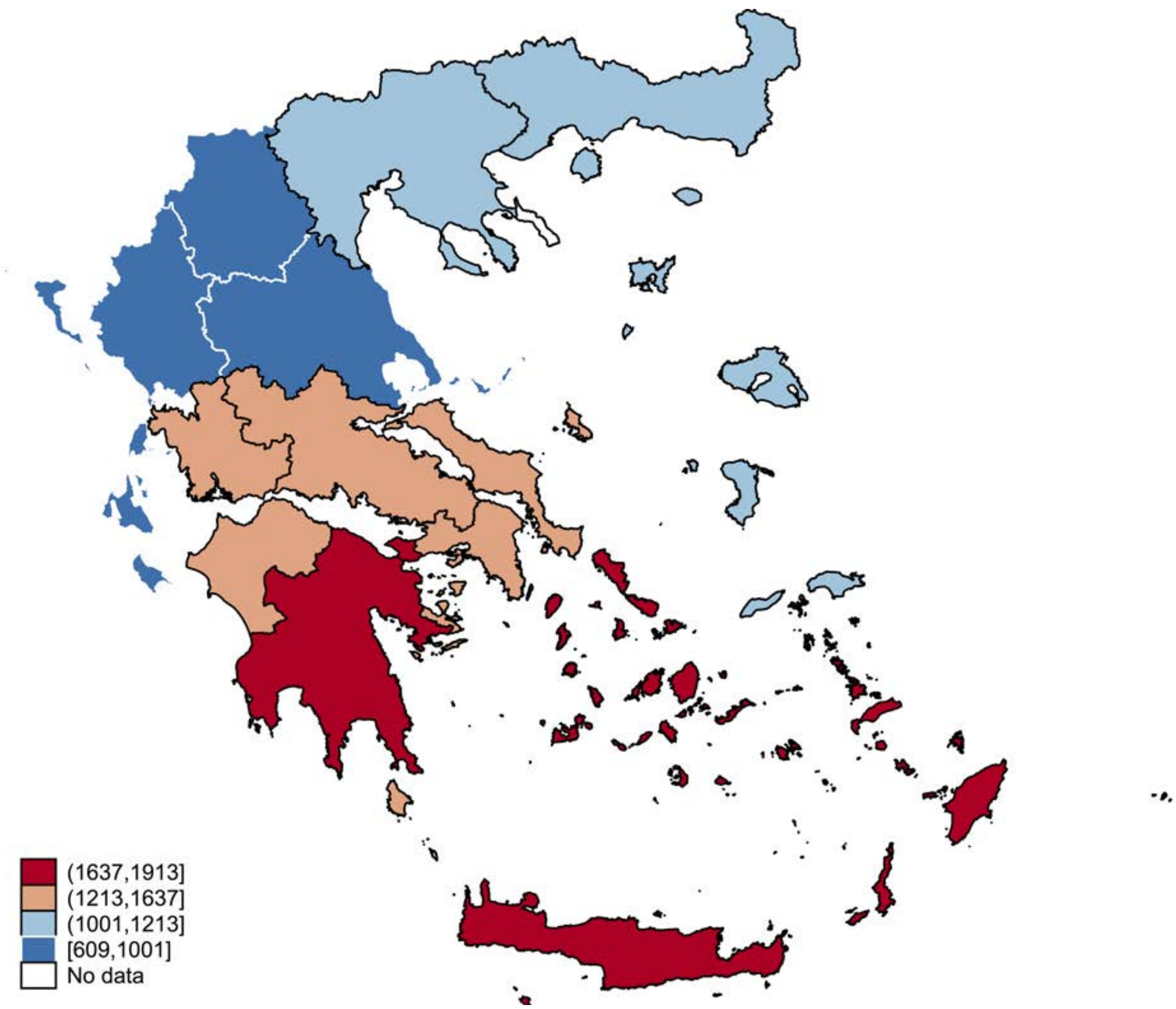
For our individual-level specification, we include experienced teachers – even those who have only appeared on the experienced lists since the beginning of our sample period – to gain statistical power. Because we do not observe these teachers on inexperienced lists during our sample, we cannot construct our instrument in the same way we do for the district-level analysis. Instead, we calculate an imputed inexperienced waitlist position. We take every teacher in our data who shares a risk set with the teacher in question. Then we order them according to degree day (within the same degree-month) and break ties randomly.[40] We then calculate

[40] We could also break ties using the degree mark, as the actual waitlists do. But in case such tie-breaking induces a correlation between position and teacher human capital, we avoid doing so.

normalized waitlist positions by dividing by the size of the risk set. Normalized waitlist positions in a single risk set thus span 0 to 1, which is a different scaling than in the instrument we use in the district-level analysis. But we are unable to normalize in the same way because we do not know how long the inexperienced waitlist was prior to our data sample period.[41]

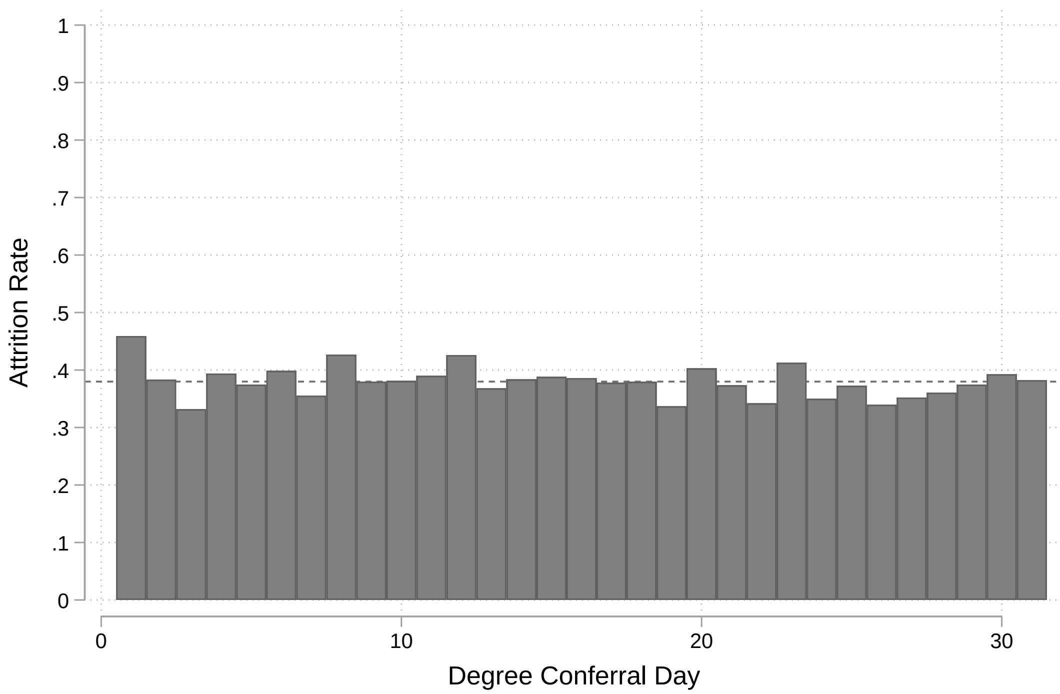
[41] Specifically, some teachers who were on the original list may have attrited from the system prior to 2003.

Figure A.1: Number of Deputy High-School Teachers Assigned



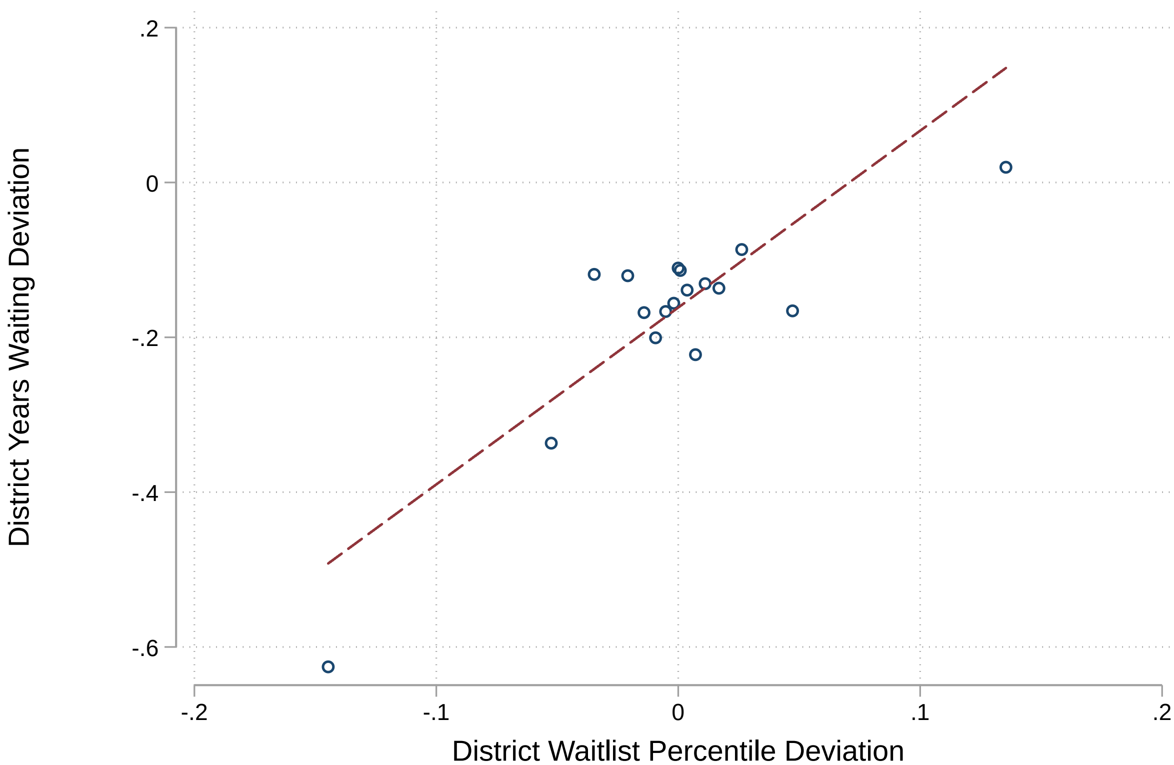
Notes: This figure shows the number of deputy high-school teachers assigned in each Greek province.

Figure A.2: Attrition and Degree Conferral Day



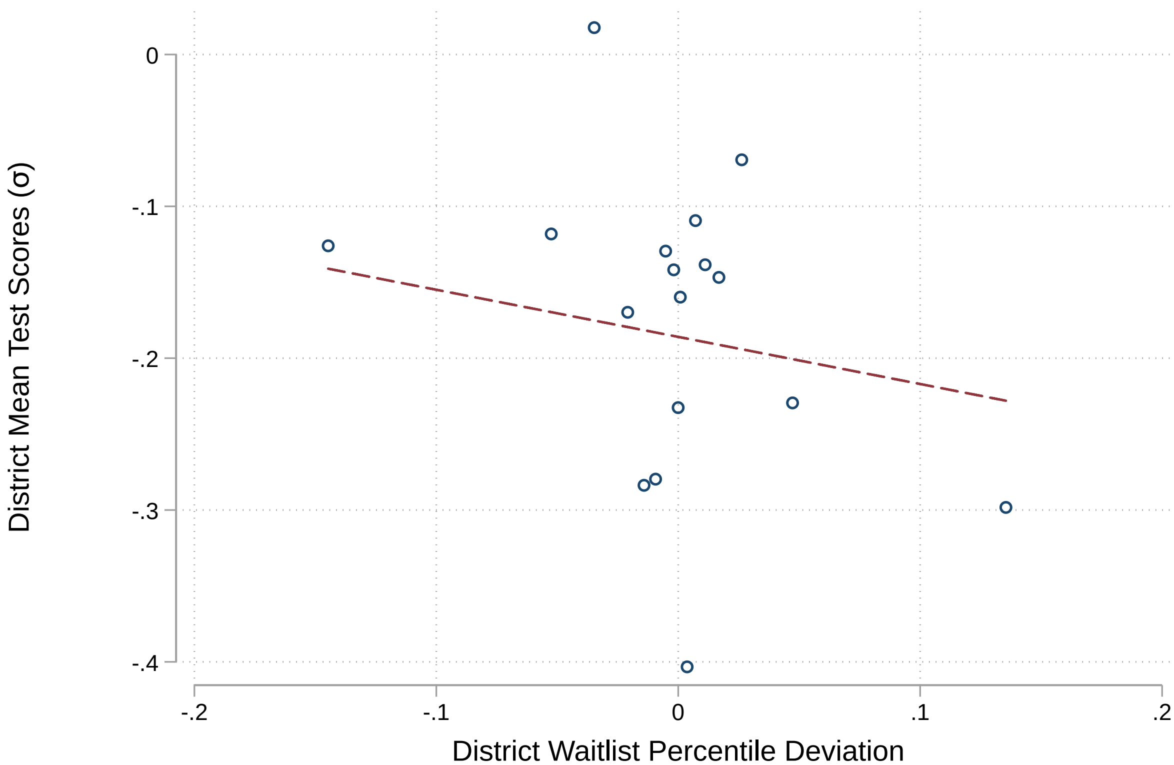
Notes: This figure shows attrition rates by the day of the month in which teachers' university degrees were conferred. This day of the month variation is our within risk-set timing variation that identifies our causal effects. Attrition is defined as leaving the waitlists before the end of our sample period and without ever having accrued experience.

Figure A.3: Years without Formal Employment and Waitlist Position – District Level



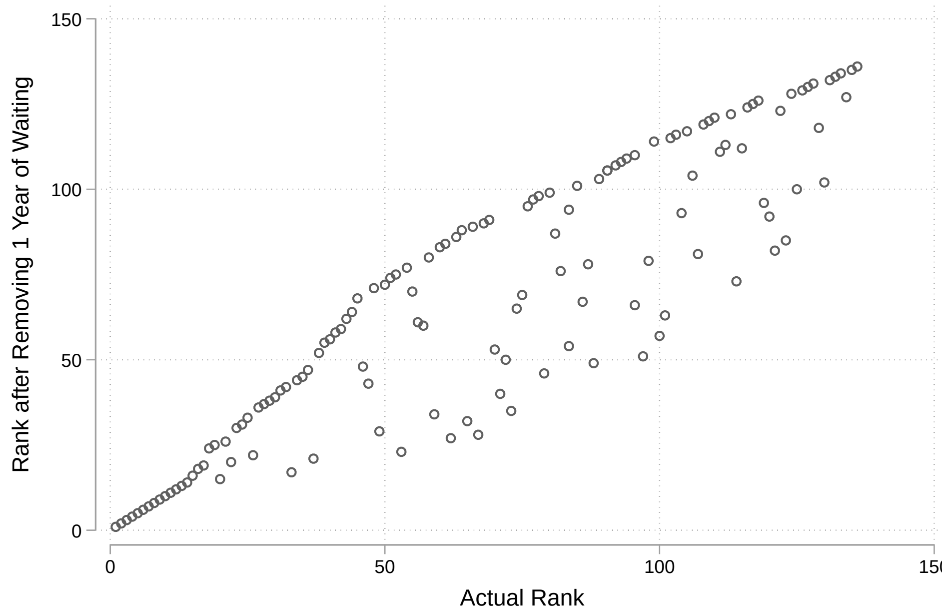
Notes: The binscatter figure shows the relationship, at the district-year level, between district demeaned waitlist percentile and district demeaned years without formal employment where the demeaning is done by risk set-year. A risk set is a degree conferral year-month and subject combination. The waitlist percentile is the initial position on the fresh graduates waitlist, normalized to vary from 0 to 1. The sample includes all teachers on a waitlist between 2003 and 2011 whose degree was conferred before 2006.

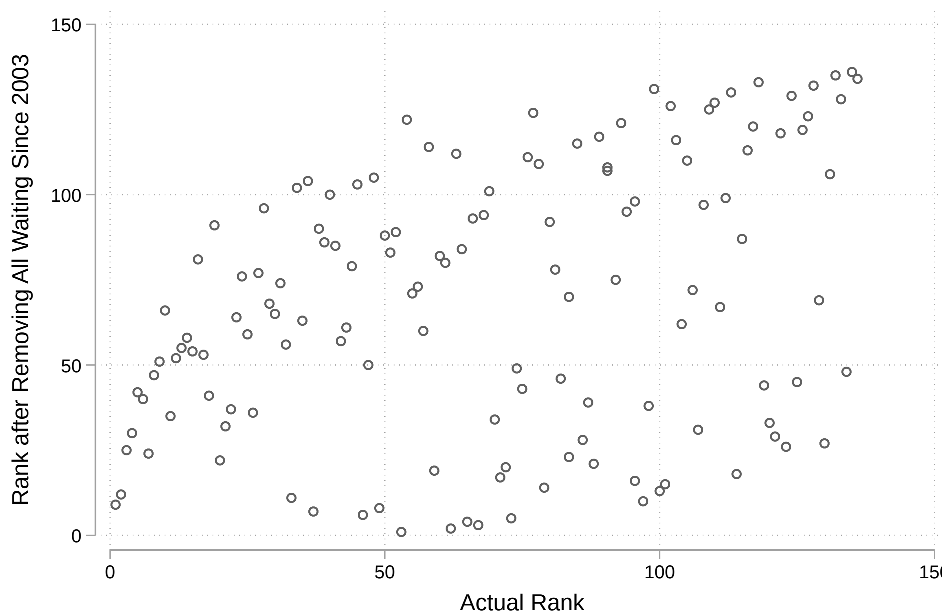
Figure A.4: Panhellenic Test Scores (σ) and Waitlist Position – District Level



Notes: The binscatter figure shows the relationship, at the district-year level, between district demeaned waitlist percentile and district Panhellenic test scores (in student standard deviation units) where the demeaning is done by risk set-year. A risk set is a degree conferral year-month and subject combination. The waitlist percentile is the initial position on the fresh graduates waitlist, normalized to vary from 0 to 1. The sample includes all teachers on a waitlist between 2003 and 2011 whose degree was conferred before 2006.

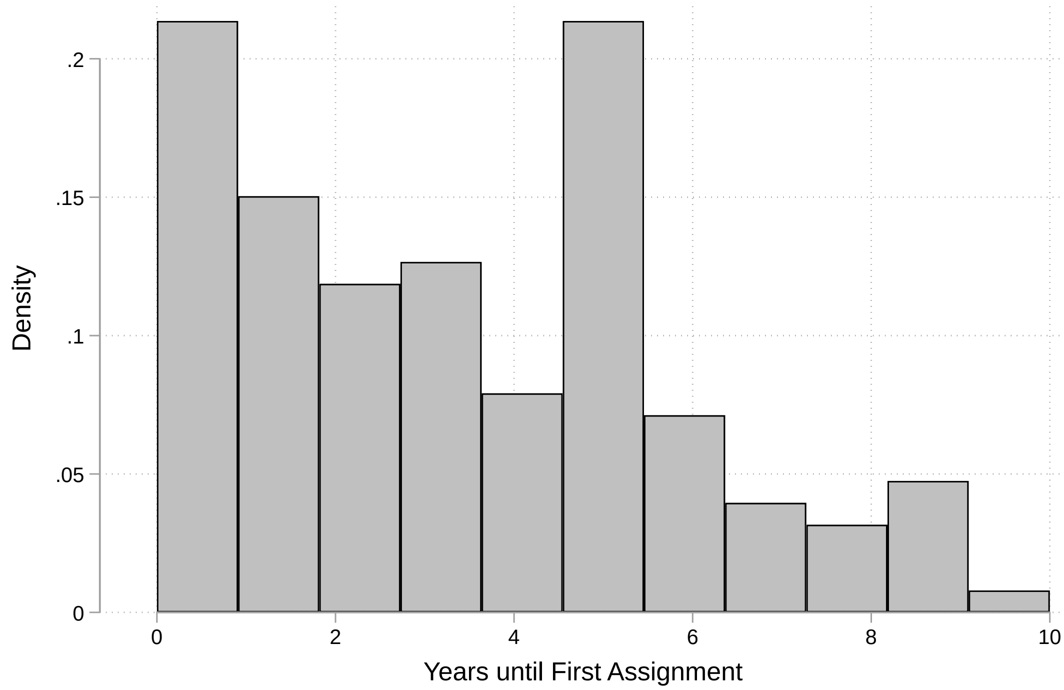
Figure A.5: Cross-District Effects of Eliminating Waiting





Notes: The figures show how changing the time out of formal employment affects districts' test score ranks. The top figure reduces the number of years deputy teachers wait without formal employment by 1 year; the bottom figure reduces the number of years deputy teachers wait to 0. We calculate each district's test score rank, under the actual scores and under counterfactuals, where the district with the lowest mean test scores has the rank 1. For each counterfactual, we take the absolute value of our point estimate from the district-level model, multiply by each district's heterogeneous exposure to the deputy assignment system, and then multiply by the number of years waiting reduced in the counterfactual.

Figure A.6: Years Waiting for First Assignment – Survey



Notes: This figure shows the distribution of the years spent waiting between degree conferral and first teaching assignment. The sample is the teachers who took our online survey. Responses with implied waiting times that are negative or more than 10 years have been excluded.

Table A.1: Survey Responses

	Mean	Std. Dev.	Obs
Degree Conferral Year	2007	8	187
Year of First Assignment	2010	9	159
Num Schools Worked in	1.42	0.87	93
Aware of System when Choosing Teaching	0.34	0.47	184
Understand the Assignment Process	0.76	0.43	184
Knew Waitlist Position	0.73	0.44	184
Considered Attriting	0.49	0.50	200
Rejected an Assignment	0.12	0.33	185
Has Left Teaching	0.20	0.40	200
Believes Skills Depreciated while Waiting	0.18	0.38	187
Invested in Skill Maintenance	0.46	0.50	200

Notes: This table shows summary statistics for selected responses to the online survey of teachers. “Num Schools Worked in” indicates the number of different schools a teacher worked in at the same time.

Table A.2: Activities while Waiting

	Fraction
Private Lessons	0.54
Further Studies	0.33
Started a Family	0.16
Non-Teaching Work in Education Sector	0.19
Work in Non-Education Sector	0.39
Other	0.19

Notes: This table shows the fraction of survey respondents in various activities during the time spent waiting for an assignment. Respondents in the online survey could choose multiple activities.

Table A.3: Teachers Working in Part-Time Jobs

	Part-Time Work while Teaching	Continued Part-Time Work while Teaching
Yes	0.28	0.33
Private Lessons	0.09	0.17
Private School	0.02	0.01
Tourism Industry	0.01	0.01
Work in Other Private Sector	0.04	0.03
Other	0.04	0.03
No	0.72	0.67
N	157	158

Notes: This table shows the part-time jobs teachers report having, while teaching, in the online survey. "Yes" indicates the teacher held a part-time job while teaching. "Part-Time Work while Teaching" is the distribution of part-time jobs while "Continued Part-Time Work while Teaching" is the distribution of part-time jobs that continue activities started while the teacher was waiting for an assignment. Respondents in the online survey could choose multiple responses.

Table A.4: Relationship between Years Waiting and Activities during Teaching

	Activity while Teaching	Continued Activity
Years until First Assignment	0.00706	0.0126
	(0.0143)	(0.0147)
Constant	0.249***	0.292***
	(0.0598)	(0.0608)
Mean DV	0.273	0.333
N	132	132

Notes: This table shows regressions of activity while teaching on years spent waiting until first assignment. Years spent waiting is calculated as the difference between the year of the first teaching assignment and the year of degree conferral. "Activity while Teaching" is a dummy variable for whether the teacher participated in an extra activity – e.g., private lessons – while working as a public school teacher. "Continued Activity" is a dummy variable for whether the teacher participated in an extra activity, while working as a public school teacher, that had been started while the teacher was waiting for an assignment. The sample is the teachers responding to our online survey. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

Table A.5: Prior Year Grade Point Average

	IV	IV
	Prior Year GPA	Score (σ)
Years Waiting	0.0143	-0.0723***
	(0.0131)	(0.0224)
Deputy	-0.0804	-0.231**
	(0.0500)	(0.103)
Demeaned Prior GPA		0.746***
		(0.0102)
Mean DV	0.0306	0.0355
Clusters	385	383
N	75340	54851
Risk Set	Yes	Yes

Notes: The table presents instrumental variable estimates. An observation is a student-subject-year. The dependent variable is the student's prior year grade point average (column 1) or the student's subject-specific test score, in standard deviation units (column 2). The instrument is the assigned teacher's imputed waitlist position (demeaned by risk set), normalized to run between 0 and 1 within a risk set. Risk sets are teachers in the same subject whose degrees were conferred in the same month-year. Demeaned prior GPA is demeaned at the teacher-year level. The sample includes students taught by deputy and permanent teachers. Permanent teachers each have their own risk set. Standard errors are clustered by teacher. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

Table A.6: Effect of Years without Formal Employment on Students' Tests by Subject

	IV	IV	IV
	Score (σ)	Score (σ)	Score (σ)
Years Waiting	-0.0853***	-0.136**	-3.585
	(0.0164)	(0.0581)	(18.51)
Deputy	-0.168*	-0.476**	-1.375
	(0.0992)	(0.228)	(6.971)
Prior Year GPA	0.737***	0.734***	0.739***
	(0.00877)	(0.0116)	(0.0174)
Mean DV	0.0355	0.0529	0.0241
Clusters	383	158	273
N	54851	21693	33158
Risk Set	Yes	Yes	Yes
Sample	All	STEM	Non-STEM

Notes: The table includes instrumental variable estimates with (demeaned) imputed waitlist position as the instrument. An observation is a student-subject-year. "Years Waiting" is the deputy teacher's years without formal employment, normalized by the risk set size to be in percentiles. "Years Waiting" and the instrument are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. The outcome is the student's average full-year score in student standard deviation units (σ). STEM fields are algebra, geometry, mathematics, biology, physics, technology, and science. Standard errors are clustered by teacher. $^{*}p < .1$, ** $p < .05$, *** $p < .01$.

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

	IV	IV	IV
	GPA (σ)	GPA	Log GPA
Years Waiting	-0.0519***	-0.150**	-0.00986*
	(0.0129)	(0.0713)	(0.00518)
Deputy	-0.0945	-0.00620	-0.000156
	(0.0579)	(0.248)	(0.0170)
Prior Year GPA	0.930***	2.589***	0.178***
	(0.00360)	(0.0146)	(0.00144)
Mean DV	-0.00226	14.94	2.686
Clusters	383	385	385
N	54541	74997	74941
Risk Set	Yes	Yes	Yes

Notes: The table includes instrumental variable estimates with (demeaned) imputed waitlist position as the instrument. An observation is a student-subject-year where the outcomes do not vary by subject but the teachers do. "Years Waiting" is the deputy teacher's years without formal employment, normalized by the risk set size to be in percentiles. "Years Waiting" and the instrument are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. The 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. $^{*}p < .1$, ** $p < .05$, *** $p < .01$.

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

	Mean $\mathbb{E}$[Years Waiting]	Mean $\mathbb{E}$[Years Waiting]
Mean Waitlist Perc	-0.207	-0.760
	(0.644)	(0.635)
Mean DV	3.556	3.556
N	394	394
District FE	No	Yes
Reg-Yr FE	No	Yes

Notes: This table tests the identifying assumption behind our aggregation to a district-level model. 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. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

Table A.9: Student-Level Analysis – Different Functional Forms

	IV	IV	IV	IV
	Score (σ)	Ln Score	Score (σ)	Ln Score
Years Waiting	-0.0853***	-0.0442**		
	(0.0164)	(0.0186)		
Deputy	-0.168*	-0.0766	-0.204	-0.0952
	(0.0992)	(0.0572)	(0.160)	(0.0750)
Prior Year GPA	0.737***	0.204***	0.737***	0.204***
	(0.00877)	(0.00450)	(0.00877)	(0.00451)
Ln Years Waiting			-1.277***	-0.662***
			(0.404)	(0.213)
Mean DV	0.0355	2.638	0.0355	2.638
Clusters	383	383	383	383
N	54851	54840	54851	54840
Risk Set	Yes	Yes	Yes	Yes

Notes: This table shows IV regressions with (demeaned) imputed waitlist position as the instrument. We vary the functional form of the test outcome (student standard deviation units or log scores) and the measure of years spent not working formally (levels or log). An observation is a student-subject-year. "Years Waiting" and the instrument are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. Standard errors are clustered by teacher. $^{*}p < .1$, ** $p < .05$, *** $p < .01$.

Table A.10: Student-Level Analysis – Different Samples

	IV	IV	IV
	Score (σ)	Score (σ)	Score (σ)
Years Waiting	-0.0853***	-0.0890***	-0.0955***
	(0.0164)	(0.0275)	(0.0293)
Deputy	-0.168*	-0.207	-0.196
	(0.0992)	(0.146)	(0.140)
Prior Year GPA	0.737***	0.737***	0.737***
	(0.00877)	(0.00880)	(0.00879)
Mean DV	0.0355	0.0360	0.0361
Clusters	383	377	380
N	54851	54529	54657
Risk Set	Yes	Yes	Yes
Sample	All	Month Not July	Day Not 30th

Notes: This table shows IV regressions with (demeaned) imputed waitlist position as the instrument. We vary the sample across columns to show robustness to excluding common degree conferral months (July) or days (the 30th). An observation is a student-subject-year. "Years Waiting" and the instrument are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. Standard errors are clustered by teacher. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

Table A.11: Student-Level Analysis – Different Outcome Definitions

	IV	IV	IV	IV	IV	IV	IV
	Score	Ln Score	Univ Score	Selec (Adm)	Selec (Natl)	Selec (2003 Adm)	Selec (2003 Natl)
Years Waiting	-0.502***	-0.0442**	-269.8***	-2.468***	-2.413***	-2.110***	-1.975***
	(0.194)	(0.0186)	(74.55)	(0.665)	(0.621)	(0.547)	(0.500)
Deputy	-0.895	-0.0766	-318.0	-3.327	-2.831	-1.920	-1.548
	(0.642)	(0.0572)	(302.3)	(2.544)	(2.408)	(2.356)	(2.182)
Prior Year GPA	2.738***	0.204***	3031.5***	24.51***	23.78***	22.44***	21.59***
	(0.0469)	(0.00450)	(34.97)	(0.247)	(0.252)	(0.301)	(0.300)
Mean DV	14.58	2.638	14315.1	49.33	49.17	52.56	52.15
Clusters	383	383	363	363	363	363	363
N	54851	54840	49175	49175	49175	35554	35554
Risk Set	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows IV regressions with (demeaned) imputed waitlist position as the instrument. We vary the functional form of the outcome across columns. An observation is a student-subject-year. "Years Waiting" and the instrument are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. "Score" is the student's average subject-specific test score during the year. "Univ Score" is the natural log of the student's university admissions score. For the selectivity measures ("Selec"), we order the university-programs according to their enrollees' mean statistic, defined below, and rank them from highest to lowest. The measure is the percentile of this ordering where 100 is the program whose admits have the highest mean score. "Adm" uses the university admissions score for ranking while "Natl" uses the national Panhellenica score, which is an alternate weighting. The "2003" measures use the 2003 cohort to construct the measures to avoid overlap with our analysis sample. Standard errors are clustered by teacher. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

Table A.12: District-Level Analysis – Different Controls

	Score (σ)	Score (σ)	Score (σ)
Years Waiting	-0.374**	-0.413***	-0.414***
	(0.160)	(0.157)	(0.158)
Ln Class Size	-0.712***	-0.810***	-0.806***
	(0.184)	(0.198)	(0.198)
Num Teachers		0.0369***	
		(0.0120)	
Num Students			0.00164***
			(0.000559)
Per Class	-0.0748	-0.0827	-0.0828
Mean DV	-0.0442	-0.0442	-0.0442
N	394	394	394
District FE	Resid	Resid	Resid
Reg-Yr FE	Resid	Resid	Resid
Risk Set	Yes	Yes	Yes

Notes: This table shows IV regressions with (demeaned) imputed wait-list position as the instrument. The columns vary our use of controls. All columns use (demeaned) fixed effects. The second and third columns include additional district-level controls. An observation is a district-year. All variables are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. Standard errors are heteroskedasticity-robust. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

Table A.13: District-Level Analysis – Different Functional Forms

	Score (σ)	Ln Score	Score (σ)	Ln Score
Years Waiting	-0.342***	-0.129***		
	(0.121)	(0.0478)		
Ln Class Size	0.239**	0.131***	0.270***	0.143***
	(0.0986)	(0.0353)	(0.0923)	(0.0335)
Ln Years Waiting			-0.711***	-0.268***
			(0.239)	(0.0992)
Per Class	-0.0684	-0.0258	-0.1423	-0.0537
Mean DV	-0.0440	2.504	-0.0440	2.504
N	390	390	390	390
District FE	Yes	Yes	Yes	Yes
Reg-Yr FE	Yes	Yes	Yes	Yes
Risk Set	Yes	Yes	Yes	Yes

Notes: This table shows IV regressions with (demeaned) imputed waitlist position as the instrument. We vary the functional form of the test outcome (student standard deviation units or log scores) and the measure of years spent not working formally (levels or log). An observation is a district-year. "Years Waiting" and the instrument are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. Standard errors are heteroskedasticity-robust. $^{*}p < .1$, ** $p < .05$, *** $p < .01$.

Table A.14: District-Level Analysis – Different Samples

	IV	IV	IV
	Score (σ)	Score (σ)	Score (σ)
Years Waiting	-0.339***	-0.191**	-0.295***
	(0.121)	(0.0846)	(0.104)
Ln Class Size	0.237**	0.249***	0.237**
	(0.0982)	(0.0850)	(0.0922)
Per Class	-0.0679	-0.0383	-0.0589
Mean DV	-0.0442	-0.0442	-0.0442
N	394	394	394
District FE	Yes	Yes	Yes
Reg-Yr FE	Yes	Yes	Yes
Risk Set	Yes	Yes	Yes
Sample	All	Month Not Jul	Day Not 30th

Notes: This table shows IV regressions with (demeaned) imputed waitlist position as the instrument. We vary the sample across columns to show robustness to excluding common degree conferral months (July) or days (the 30th). An observation is a district-year. "Years Waiting" and the instrument are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. Standard errors are heteroskedasticity-robust. $^{*}p < .1$, ** $p < .05$, *** $p < .01$.

Table A.15: District-Level Analysis – Different Outcome Definitions

	IV	IV	IV	IV	IV	IV	IV
	Score	Ln Score	Univ Score	Selec (Adm)	Selec (Natl)	Selec (2003 Adm)	Selec (2003 Natl)
Years Waiting	-1.351***	-0.129***	-1153.7**	-5.645**	-5.097*	-3.690	-2.828
	(0.453)	(0.0478)	(455.1)	(2.576)	(2.654)	(2.973)	(3.140)
Ln Class Size	1.346***	0.131***	1725.2***	9.139***	9.540***	7.794***	8.047***
	(0.331)	(0.0353)	(427.1)	(2.262)	(2.236)	(1.832)	(1.868)
Per Class	-0.2702	-0.0258	-230.7	-1.129	-1.019	-0.738	-0.566
Mean DV	13.10	2.504	14274.0	48.77	48.58	52.40	52.12
N	390	390	390	390	390	385	385
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg-Yr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Risk Set	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows IV regressions with (demeaned) imputed waitlist position as the instrument. We vary the functional form of the outcome across columns. An observation is a district-year. "Years Waiting" and the instrument are demeaned by risk set, where a risk set is the cohort of teachers in the same subject with degrees conferred in the same month-year. "Score" is the student's average subject-specific test score during the year. "Univ Score" is the natural log of the student's university admissions score. For the selectivity measures ("Selec"), we order the university-programs according to their enrollees' mean statistic, defined below, and rank them from highest to lowest. The measure is the percentile of this ordering where 100 is the program whose admits have the highest mean score. "Adm" uses the university admissions score for ranking while "Natl" uses the national Panhellenica score, which is an alternate weighting. The "2003" measures use the 2003 cohort to construct the measures to avoid overlap with our analysis sample. Standard errors are heteroskedasticity-robust. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

Table A.16: Effect of Years without Formal Employment on Districts' Panhellenic Exam Scores without Scaling

	Score (σ)	Score (Perc)	Ln Univ Score	Admitted
Years Waiting	-0.121**	-3.152**	-0.0579	-0.0442**
	(0.0603)	(1.495)	(0.0458)	(0.0222)
Ln Class Size	0.105**	2.175*	0.0231	0.0102
	(0.0535)	(1.293)	(0.0412)	(0.0166)
Per Class	-0.0242	-0.6304	-0.0116	-0.0088
Mean DV	-0.0440	49.07	9.518	0.818
N	390	390	390	390
District FE	Yes	Yes	Yes	Yes
Reg-Yr FE	Yes	Yes	Yes	Yes
Risk Set	Yes	Yes	Yes	Yes

Notes: The table include the main IV regressions without incorporating the deputies we lack inexperience waitlist positions for. An observation is a district-year and the IV outcomes are measures of student performance on the national twelfth grade exams, as mean level (in student standard deviation units) or mean percentile, and university admissions outcomes. Standard errors are heteroskedasticity-robust. $^{*}p < .1$, $^{**}\ p < .05$, $^{***}\ p < .01$.

Table A.17: Effect of Years without Formal Employment on Districts' Panhellenic Exam Scores with Alternate Weighting

	Score (σ)	Score (σ)	Score (σ)
Years Waiting	-0.342***	-0.452***	-0.466***
	(0.121)	(0.171)	(0.131)
Ln Class Size	0.239**	0.503***	0.599***
	(0.0986)	(0.117)	(0.0883)
Per Class	-0.0684	-0.0904	-0.0933
Mean DV	-0.0440	-0.150	-0.178
N	390	390	390
District FE	Yes	Yes	Yes
Reg-Yr FE	Yes	Yes	Yes
Risk Set	Yes	Yes	Yes
Weighting	Num Students	Num Deputies	None

Notes: The table includes the main IV regressions using alternate weighting. An observation is a district-year and the IV outcomes are measures of student performance on the national twelfth grade exams, in student standard deviation units. Weighting is shown in the last row. Standard errors are heteroskedasticity-robust. $^{*}p < .1$, ** $p < .05$, *** $p < .01$.