WAGES AND RECRUITMENT: EVIDENCE FROM EXTERNAL WAGE CHANGES

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In this article, the author estimates the causal effect of the wage level on the recruitment rate in establishments. During the 1990s, the wage setting for certified teachers in Norway was completely centralized, with a state-paid wage premium of about 10% at some schools with severe recruitment problems. The empirical approach exploits within-school variation in wage-premium eligibility and that actual teacher supply is empirically observed at schools with excess demand for teachers. In a difference-in-differences framework, the wage premium increases the recruitment rate by 6 to 7 percentage points. This finding is robust to model specification and indicates that the recruitment elasticity to the wage is equal to the separation elasticity in absolute terms. The implied short-run labor-supply elasticity for individual establishments is about 1.4. It is also evidence of a diminishing return to scale in recruitment activity, a central assumption in search-theoretic models of imperfect competition in the labor market.

Whereas the theoretical literature on search and matching in the labor market mainly models the recruitment process, the microeconometric evidence on wage responses is almost exclusively about separations. The recruitment elasticity to the wage is important for the wage-setting power and the recruitment activity of establishments. In this article, I estimate the recruitment elasticity in a labor market that has shortages of labor and exogenous wage differentials.

Manning (2003, 2011) argues that separation and recruitment elasticities are identical in absolute terms. Because a separation from one firm is a recruitment in another firm in steady-state, the recruitment elasticity, and consequently also the labor-supply elasticity, can simply be inferred by the

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separation elasticity. However, credible separation elasticities are estimated on specific labor markets, for which the steady-state assumption does not hold. Engineers might take nontechnology jobs, teachers might take non-teaching jobs, and so on, which implies that a separation from a firm in a specific labor market often does not end up as a recruitment in a firm in the same labor market. Thus, whether the recruitment elasticity is equal to the separation elasticity in specific labor markets is an empirical question. The empirical literature might have leaned too heavily on the assumption of the equality of the absolute values of the separation and recruitment elasticities to infer the labor-supply elasticity at the establishment level,[1] which ultimately is the main indicator of the degree of imperfection in the labor market.

In this article, I investigate whether the separation and recruitment elasticities are similar to one another by exploiting a quasi-experimental setting in the Norwegian teacher labor market. For a period of 10 years, a state-paid wage premium of about 10% was offered to certified teachers in some Norwegian schools in a market in which, otherwise, the same wage was paid at each school. The majority of newly appointed teachers in the present sample did not teach during the previous school year, and thus we might question the steady-state assumption. In a difference-in-differences framework for this market, in which supply is the binding constraint, I estimate to what extent the wage premium increased recruitment. I compare the estimated recruitment elasticity to the previously estimated quit elasticity in the same labor market (Falch 2011). If the elasticities are different in absolute value, future work must use more sophisticated methods than used currently to infer about the labor-supply elasticity at the establishment level.[2]

Search-based models of imperfect competition assume diminishing returns to scale in the technology of recruiting new workers.[3] The lack of evidence on scale effects is a main criticism made by Kuhn (2004) of monopsony models. Although the within-school variation in size is much smaller than the variation in the wage premium in the present sample, I find evidence for diminishing returns to scale: The wage effect on recruitment seems to decline as school size increases.

The empirical literature on recruitment and wages includes only Krueger (1988), Holzer, Katz, and Krueger (1991), and Bó, Finan, and Rossi (2013). These articles estimated how the number of job applicants is related to the wage. In addition, Bó et al. analyzed the wage effects on the qualification of the application pool and on the acceptance rate of job offers using an experiment with exogenous wage variation for some public-sector positions in Mexico. One argument for empirically analyzing separations in the labor

[1]This literature includes Manning (2003); Hirsch, Schank, and Schnabel (2010); Ransom and Oaxaca (2010); Ransom and Sims (2010); Falch (2011); Hotchkiss and Quispe-Agnoli (2012); Depew and Sorensen (2013).

[2]I also compare the estimates to the results for the overall supply elasticity on the level of individual schools in Falch (2010), which provide a static analysis of school-level data for a short time period.

[3]See, for example, Rogerson, Shimer, and Wright (2005) for a survey of search-theoretic models.

market instead of recruitment is that separations are less affected by employer behavior.[4] In the recruitment process, employers generally have several policy instruments in addition to the wage. This is a major identification concern in the present study, but casual evidence indicates that this is not important in the specific labor market investigated here. Results from robustness analyses also suggest that the estimated recruitment elasticity is not biased by endogenous recruitment activity.

I first present the institutions, data, and empirical strategy. Next, I present the main results, several robustness and heterogeneity analyses, and the estimated scale effects. Finally, I discuss the findings in a labor-supply framework and show that the implied recruitment elasticity is in accordance with previous estimated separation elasticities on average, but not within subgroups of teachers.

The Quasi-Natural Experiment

Until 2001 the wage of an individual teacher in Norway was fully determined by collective bargaining between the teachers' union and the central government. The agreement determined by collective bargaining was binding for all schools. The wage varied across teachers only with respect to formal education level and teaching experience. Some limited local wage flexibility was introduced in fall 2001, when the central bargaining set aside a small amount to be used to increase the wage for some teachers selected in a local bargaining session at the school-district level. The wage setting was further decentralized starting in 2004. The present article uses data only for the period without any local discretion.

The wage-premium experiment took place in Norwegian compulsory public schools (1st to 10th grades) during the school years 1993/1994 to 2002/2003. Within the completely centralized system, the state determined that certified teachers who had at least a 50% appointment in schools with particular recruitment problems and located in one of the 3 northernmost counties (out of a total of 19 counties)—Nordland, Troms, and Finnmark—were eligible for a wage premium of about 10%, paid by the state.[5] The school districts had no influence on a school's eligibility, and eligibility had no financial implications for them. The wage premium was a fixed nominal amount independent of a teacher's education and experience. The percentage size of the premium changed only in 1994 and 1998.

The eligibility criteria were linked to specific appointment rules for teachers. Teachers are employed by the school district, but the district cannot move a teacher to another school unless a major downsizing of the school occurs or explicit approval is given by the particular teacher. According to the school law, an individual who is not certified as a teacher can be

[4]See, for example, Manning (2011) for a survey of the literature on separation elasticities.

[5]The average percentage of the wage premium was lowest in 1993/1994 (about 7.5%) and highest in 1998/1999 (about 12.0%).

employed only if no certified teacher applies for a vacant teacher position and the noncertified teachers can be hired only for up to one school year. In the next school year, the school has to again publicize the vacant position and search for certified teachers. Representatives of the teachers' trade union take part in every hiring decision and thus closely monitor that the schools behave in accordance with the law; this has been one of the cornerstones in the union's policy. These institutions imply that the observed teacher shortages, in terms of the employment of noncertified teachers, reflect the state of the local teacher labor market in any year.

In the school years 1993/1994 to 1995/1996, teachers in schools that had more than a 20% teacher shortage during the previous school year and were located in one of the three relevant counties were eligible for the wage premium.[6] *Teacher shortages* are defined as the share of man-years of noncertified teachers. The eligibility criterion became stricter for the school years 1996/1997 to 1997/1998. In those years, only teachers in schools that had more than a 30% teacher shortage during the previous school year received the higher wage. Under the last set of rules and continuing to the school year 2002/2003, the eligibility criterion required a teacher shortage of more than 20% on average during the previous four school years.

The classification of the schools was done by the relevant county governor's office. The county governor is appointed by the state and has mainly inspection duties related to local public services. Because the wage-premium criterion was the percentage of the previous year's teacher shortages, which schools were eligible was always well known in advance of the school year. Starting in 1998/1999, the instructions from the central government explicitly stated that schools eligible for wage premium in the next school year should be informed before March 1 and that, for any new teaching positions publicized before this date, the school districts had to pay the wage premium without compensation from the state.

Several schools switched between paying the wage premium and not paying it during the period under study. One system-induced explanation for this is that the eligibility criterion varied over time. Another possible explanation is the expected wage effect on recruitment and quits. Because paying a wage premium is expected to increase the teacher supply, schools with teacher shortages marginally above the criterion level for the wage premium would most likely increase the number of teachers they employed, so the schools would not be eligible for the wage premium the next school year.[7]

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[6]For schools with a 20 to 30% teacher shortage during the previous school year, the rules in 1993/1994 to 1995/1996 differed across school districts in the relevant counties. In schools located in one of the northeastern school districts, the teacher wage premium was about 10% when the shortage the previous year exceeded 30% but only about 5% when the shortage ranged between 20 and 30%. In the other school districts in the relevant counties, the wage premium was about 10% in all eligible schools.

[7]If a school lost eligibility because of better recruitment, the incumbent teachers at the school kept their wage premium as long as the specific rules were in place, but the new hires did not get a wage premium. The rules changed after the school years 1995/1996 and 1997/1998, as previously explained.

Because eligibility for the wage premium was based on lagged information, gaming the system was, in principle, possible. For example, if a school replaced quitting teachers with noncertified teachers, even though certified teachers were interested in the positions, the incumbent teachers might get a wage premium the next school year.[8] For several reasons, such gaming arguably did not occur. When I collected the data, the county governor offices reported that they did not believe the system had been manipulated because of the strict appointment rules presented above. In addition, the two changes in the eligibility criterion for the wage premium during the empirical period were determined after the registration of teacher shortages. Both the change in 1996 and 1998 were decided in December of the previous years, and the eligibility criterion was based on teacher shortages earlier in the fall. Finally, because the wage-premium criterion was based on four-year averages from 1998 onward, manipulating eligibility became even harder, in the sense that changes in a few teacher positions in one single year had a smaller impact on the distance to the eligibility threshold.

The treatment schools (the schools that were eligible for the wage premium for at least one year during the empirical period) are located in the three northernmost counties in Norway, which contain 90 school districts and about 550 schools.[9] The mode is five schools per school district.[10] Typically, the commuting distance between schools in the same district is feasible, but traveling from one district to another takes longer because the district borders are most often in uninhabited areas. According to the 1990 census, only 15% of the workers in the relevant counties worked in a district other than the one in which they resided.

Data

Information about the wage premium was provided by state representatives in the relevant counties. Table 1, panel A, presents the number of schools and teachers with wage premiums by school year. Few schools were eligible for wage premiums during the relatively restrictive rules in 1996/1997 and 1997/1998, whereas about three times as many schools had wage premiums in the preceding and following school years. The changes in the eligibility criterion over time imply that most schools had a wage premium for only a short period. Table 1, panel B, shows that, of the 158 treatment schools, in

[8]In principle, strategic behavior on the part of the individual teachers might also be possible. A teacher might choose to delay an application one year to contribute to the wage-premium eligibility. This is, however, a highly uncertain strategy because a single teacher cannot know whether other teachers (e.g., newly graduated teachers) might apply for the open position.

[9]Private schools were not eligible for the wage premium; however, only about 0.5% of the students in the relevant counties were enrolled at private schools.

[10]Four school districts in the relevant counties have only 1 school, and the largest district has 39 schools, 5 of which are treatment schools.

Table 1. Observations with Wage Premium

A. Yearly observations of wage premium

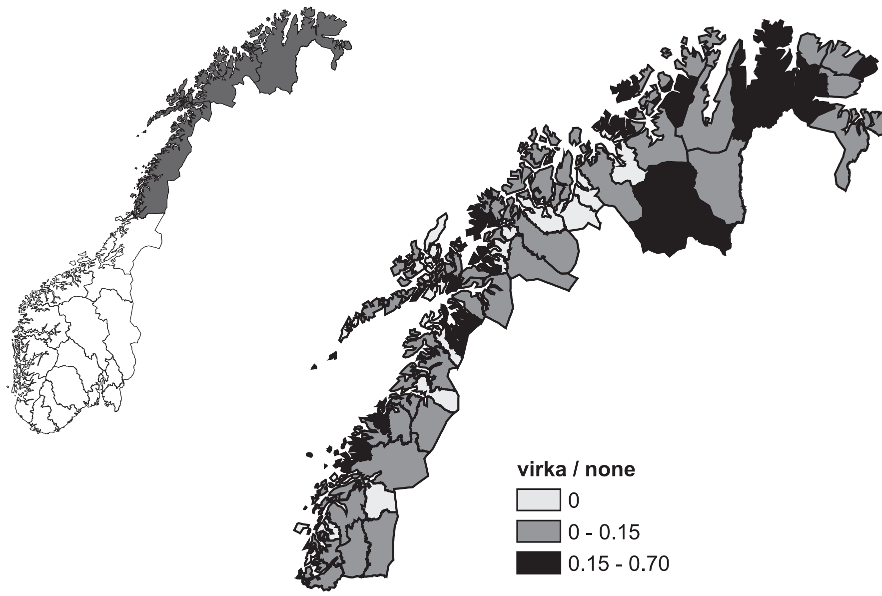
	1993/1994	*1994/1995*	*1995/1996*	*1996/1997*	*1997/1998*	*1998/1999*	*1999/2000*	*2000/2001*	*At least one year*
Schools	69	45	33	22	16	62	72	72	158
Teachers	405	223	163	66	51	297	362	325	1,202

B. Number of years with wage premium for the treatment schools[a]

	Years								
	0	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*
Wage premium for new hires	0	66	34	22	12	12	5	3	4
Not wage premium for new hires	8	8	7	15	13	22	30	55	0

Note: School and teacher observations in which only incumbent teachers are eligible for the wage premium are not included.
[a]Because of school mergers and closures, panel B is not symmetric.

Figure 1. The Average Extent of Wage Premiums at the Teacher Level in the Different School Districts in Norway, 1993/1994 to 2000/2001



66 schools new hires received wage premiums in only one year and in 36 schools new hires received wage premiums in more than three years.

Data on individual teachers and school identifiers were provided by Statistics Norway. The estimation sample is restricted to certified teachers in at least 50% positions because noncertified teachers and teachers with shorter working times were not eligible for the wage premium.[11] For the relevant counties, the average share of teachers in the empirical period who received a wage premium is shown in Figure 1. The school districts in which teachers received the wage premium are spread all over the region; however, receipt of the wage premium was most common in the northernmost county. In the three counties, 77% of the school districts include at least one treatment school during the empirical period and 13% have an average share of teachers who received a wage premium exceeding 15%.

Descriptive statistics are presented in Table 2. The data consist of 79,135 teachers, of whom 10,868 worked in one of the counties with treatment schools and 2,034 worked at a treatment school during the empirical period. On average, 15.5% of the certified teachers were new hires.[12] The

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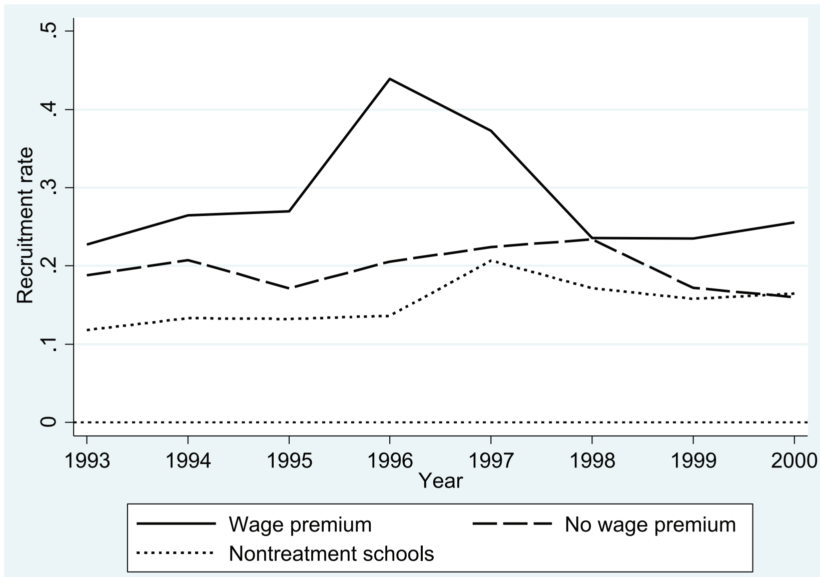
[11]The share of noneligible teachers in the original data is 11.4%. Data from the first year of new schools in the data are also excluded from the analysis because all the teachers were new hires (0.5% of the observations).

[12]This recruitment rate is calculated as the number of teachers registered at the school in the present year but not registered at the school in the previous year relative to the number of teachers in the present year. The rate is calculated using individual data, which implies that the schools are weighted by their teacher employment.

Figure 2. Yearly Recruitment Rates for Teachers



recruitment rate is about the same in the three relevant counties and in the whole country, but it is clearly higher in the treatment schools. With the wage premium, the average recruitment rate is 25.4%, but without the wage premium, the recruitment rate in these schools is 19.7%. The difference of 5.7 percentage points (pp) is significant at conventional levels.

Figure 2 presents the yearly recruitment rates. With a wage premium, the recruitment rate is close to 25% each year, except for the school years 1996/1997 and 1997/1998. Few schools were eligible for the wage premium during these years, as shown in Table 1. Every year, the recruitment rate is lower in the treatment schools without the wage premium than with the wage premium. For the schools that never paid a wage premium (nontreatment schools), the recruitment rate is slightly increasing in the empirical period. This is related to a reduction in the age of starting school from seven to six in 1997, which increased the demand for teachers by about 10%.[13]

Table 2 also presents descriptive statistics for some teacher characteristics. Both the average age, the share of female teachers, the share of married teachers, and the share of teachers with a permanent teacher position during the previous year are lower in the treatment schools than in other schools. In addition, the schools in the relevant counties are smaller than in

[13]To meet the increased demand for teachers, a change in the certification rules for teachers was implemented at the same time as the reform. A teaching certification for grades 1 to 4 was given to preschool teachers who took some additional college courses. This increased the supply of teachers.

Table 2. Descriptive Statistics

Variable	All schools	Schools in the relevant counties	Treatment schools: Wage premium for new hires	Treatment schools: No wage premium for new hires	Treatment schools: Estimated difference
A. All teachers					
Recently hired teachers	0.155	0.159	0.254	0.197	0.057* (0.011)
Age	44.3 [10.3]	43.1 [10.2]	40.0 [10.7]	40.8 [10.4]	−0.766* (0.283)
Women	0.663	0.623	0.613	0.616	−0.003 (0.013)
Children 6–18 years of age	0.409	0.429	0.398	0.418	−0.020 (0.013)
Married	0.696	0.637	0.522	0.559	−0.037* (0.013)
Permanent teacher position last year	0.790	0.786	0.730	0.684	−0.046* (0.012)
Number of students	245 [134]	191 [132]	58.7 [57.9]	68.4 [53.1]	−9.64* (1.47)
Number of observations	401,483	51,313	1,892	5,073	6,965
Number of teachers	79,135	10,868	1,202	1,714	2,034
Number of schools	3,313	567	158	150	158
B. Recently hired teachers					
Age	36.0 [10.1]	35.6 [9.80]	34.6 [10.2]	34.9 [9.90]	−0.334 (0.555)
Women	0.719	0.658	0.605	0.622	−0.017 (0.027)
Children 6–18 years of age	0.341	0.338	0.268	0.305	−0.037 (0.025)
Married	0.495	0.409	0.310	0.353	−0.043 (0.026)
Permanent teacher position last year	0.229	0.224	0.212	0.182	0.030 (0.022)
Number of students	245 [141]	180 [134]	50.0 [54.6]	65.3 [51.9]	−15.3* (2.93)
Number of observations	62,319	8,180	481	1,000	1,481
Number of teachers	45,188	6,198	470	942	1,343
Number of schools	3,258	546	129	135	153

Notes: Mean values. Standard deviations appear in brackets, and standard errors appear in parentheses. *Denotes significance at 5% level.

the rest of the country, and this is particularly the case for the treatment schools.[14]

Table 2, panel B, presents the descriptive statistics for the observations of the recently hired teachers. Compared to all teachers (panel A), they are younger, less likely to be married, and less likely to have had a permanent teacher position during the previous school year. The last column in the table indicates that the wage premium has a negative effect on the probability that recently hired teachers are old, married, and have children, but the differences are not significant.

Table 3 provides information about the teachers' previous position and distinguishes six origins: 1) incumbent, 2) did not teach in the previous year, 3) taught in another school in the district with a wage premium, 4) taught in another school in the district without a wage premium, 5) taught in a school in another district with a wage premium, and 6) taught in a school in another district without a wage premium. Across all schools (column (1)), 84.5% of the teachers were incumbent teachers. A majority of the new hires, including novice teachers in their first job, did not work in compulsory education or in noncompulsory upper secondary education during the previous school year (9.9% of all teachers). About the same share of teachers was recruited from schools in the same school district as from schools in other districts.

The recruitment pattern for the schools in the three relevant counties (Table 3, column (2)) is almost exactly the same as for the rest of the country. The interesting pattern, however, is that the share of newly hired teachers from all origins is larger when the treatment schools paid wage premiums than when they did not. Table 3, column (5) shows that the difference is significant at the 5% level for four of the six origins (including incumbents).[15] Thus, the larger recruitment rate with wage premiums does not reflect a larger recruitment from specific origins.

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[14]The schools are most often either a primary school (1st to 7th grade) or a lower secondary school (8th to 10th grade). Typically, several primary schools feed into one lower secondary school. The average size of the treatment schools in the data is 66 students, and wage premiums are slightly more common in the smallest treatment schools than in the larger treatment schools, as is evident from Table 2. On average, the treatment schools employ 10.0 teachers, of which 2.1 are recently hired. About 20% of the observations of the treatment schools are of teachers at schools with fewer than 25 students and more than 100 students. Schools of these sizes have, on average, 1.0 and 3.9 recently hired teachers, respectively. This implies that some of the treatment schools with teacher shortages close to 20% searched for teachers to fill positions that were less than full-time; however, the kind of position (full-time or part-time) is typically a result of negotiations.

[15]This essentially tests for equality of proportions of teachers from the different origins pre- and post-treatment because the standard errors are clustered at the school level. Table 3 presents tests weighted by the number of teachers at the school level. Unweighted tests using shares at the school level give qualitatively similar results.

Table 3. Origins of the Teachers (%)

Variable	All schools (1)	Schools in the relevant counties (2)	Treatment schools: Wage premium for new hires (3)	Treatment schools: No wage premium for new hires (4)	Estimated difference (5)
Incumbent	84.5	84.1	74.6	80.3	−5.71*
					(1.12)
Did not teach last year	9.89	10.1	17.0	13.7	3.27*
					(1.06)
Worked at another school last year					
With wage premium, in the same school district	0.02	0.17	0.79	0.20	0.60*
					(0.22)
With wage premium, in another school district	0.05	0.19	0.37	0.24	0.13
					(0.15)
Without wage premium, in the same school district	3.14	3.19	2.96	2.66	0.30
					(0.54)
Without wage premium, in another school district	2.42	2.35	4.33	2.92	1.42*
					(0.58)
Number of observations	401,483	51,313	1,892	5,073	6,965

Notes: Standard errors appear in parentheses.
*Denotes significance at 5% level.

Empirical Strategy

The first main identifying assumption is that all schools in the sample have an excess labor demand for certified teachers. The schools have teacher shortages, and the teacher supply is always a binding constraint. Because any certified applicant must be hired before any noncertified applicant, when this assumption holds the number of certified applicants in the previous year is given by the number of certified teachers hired this year. The second main identifying assumption is exogeneous recruitment activity. I will present results from different model specifications and several robustness checks in order to consider whether these assumptions are reasonable.

The Main Model

The main model relates the probability that a teacher is recently hired to the wage premium and estimate variants of the following model.[16]

[16]Separation and recruitment decisions are, in general, not separable. But analyzing the matching process of teachers and schools is complicated by the fact that we observe only the actual matches and not all the potential matches. To estimate a structural model including both the quit and the matching decisions, we need information on the searching behavior of teachers. Boyd, Lankford, Loeb, and Wyckoff (2013) estimated a matching model for the initial sorting of newly educated teachers to schools. By focusing on the initial matching, they reduced the number of potential matches and avoided modeling quit decisions. For the present article, the whole labor market for certified teachers is relevant; that is, each individual with a teacher certificate, employed at a school or not, is relevant for vacant teacher posts.

$$\text{(1)} \qquad r_{ijt} = \kappa P_{jt} + \phi X_{jt} + \alpha_j + \alpha_t * \alpha_d + \beta_t * T_j + \beta_j * t + \varepsilon_{ijt}$$

where r_{ijt} is a dummy variable for whether teacher i at school j is recruited in year t, and P is an indicator for wage premium. The terms α_j, α_t, and α_d are the school, year, and school-district fixed effects, respectively, T is an indicator for a treatment school, X includes school characteristics, and ε is the error term. Standard errors are clustered by school.

The model could, alternatively, be estimated using data aggregated to the school level. The difference, however, is simply the weighting of schools. Large schools have more hires and receive a larger weight in an individual-level regression than in an unweighted school-level regression. The main advantage of using the individual-level regression is that it facilitates the inclusion of individual characteristics. I estimate a linear probability model, as suggested by Angrist and Pischke (2009), which facilitates the inclusion of fixed effects and makes the results directly comparable to the school-level regressions presented later in the article.

The inclusion of school fixed effects implies that the model amounts to a difference-in-differences specification. Controlling for time might be important because, for example, the extent of the wage premium varies across the years. The interaction between the year fixed effects and the school-district fixed effects captures all the common yearly elements in the teachers' choice set at the school-district level. The model allows for different time trends in the treatment schools than in the other schools, modeled flexibly as the interaction between year fixed effects (β_t) and the indicator for treatment school (T_j). In addition, the model includes school-specific time trends ($\beta_j * t$). Finally, the time-varying school characteristics included in the model are the logarithm of the number of students and the change in the logarithm of the number of students. These variables control more credibly for demand shocks than is typically possible for individual establishments, although this is not expected to be important in the present case because changes in demand are not expected to be related to the wage premium.

The model does not include individual characteristics. From the point of view of the labor-supply elasticity faced by the establishment, the interesting question is how a higher wage will affect the number of applicants who have a given productivity, without regard for the other characteristics of these applicants. It is expected that new applicants have characteristics that in general make them mobile without necessarily being related to productivity, such as being young, unmarried, and without a current permanent position. The wage response in models that are conditional on such individual characteristics is therefore expected to be underestimated. An important caveat, however, is that the productivity of the workers employed can increase in the wage, which makes the concept of labor-supply elasticity to an individual firm fragile, as argued by Kuhn (2004). If the wage increases both recruitment and worker quality, models without quality information will underestimate the pure wage effect. In robustness analyses later in the article, I include models with observed and unobserved teacher characteristics.

The model uses a dummy variable specification for the wage premium. An alternative specification is to use the actual wage premium to estimate the wage elasticity more directly. Such a model specification would materialize into an alternative specification only if the percentage wage premium is included and not the nominal wage premium; this is because the nominal wage premium is equal for all teachers each year and the variation over time is saturated by the year fixed effects. In that case, however, individual characteristics would be embedded in the model because the base wage is a strict function of the teachers' experience and education.[17]

The parameter of interest κ expresses the effect of the wage premium in percentage points. This can be transformed to the average recruitment elasticity by assuming that employment is independent of the wage, which is reasonable in the present case because the wage premium is paid by the state. Then the average recruitment elasticity is given by

$$\varepsilon_{rw} = (\partial r/\partial P)(\partial P/\partial \ln W)(1/r) = (\kappa/r)(\partial P/\partial \ln W) \tag{2}$$

where W is the actual wage. For the treatment schools, the weighted average recruitment rate is 0.212. The average wage premium during the empirical period is 9.1%, which amounts to 0.087 log points. Thus, $\varepsilon_{rw} \simeq 55 * \kappa$.

Threats to Identification

I address four related identification concerns in the analysis. First, several of the control schools might be very different from the treatment schools. Most important, the control schools are less likely to be supply constrained than the treatment schools. In addition, they are, to a large extent, located in other parts of the country, and they tend to be larger than the treatment schools. This concern is not necessarily an important issue because the model includes school fixed effects, which implies that time-invariant differences across schools do not contribute to the identification. Nevertheless, the coefficients of the control variables might be biased, and through them, the coefficient of interest. To consider the robustness of the results, I estimate the model on several different subsamples (e.g., samples that include only control schools with observed teacher shortages or only treatment schools). The estimated wage effect turns out to be insensitive to sample used, probably because the identifying variation is within the schools.

Second, some observations of the treatment schools might not have excess demand, which can be considered to be a truncation issue. We cannot rule out that sometimes teachers are interested in working at such schools without getting a job offer; that is, we observe the demand and not the supply. If the wage has a strong effect on teachers' mobility decisions, we would expect the wage premium to be positively related to absence of

[17]The qualitative results for models using the actual wage premium in percentage terms are the same as in the models presented here.

excess demand. In this case, the estimated effect of the wage premium would be downward biased. I investigate the relevance of the truncation issue by estimating models that restrict the sample to schools with vacant teacher positions.

The assumption that the schools are supply constrained implies that the number of quits should have no effect on the number of hires. If some schools are not supply constrained or if schools respond to a high quit rate by increasing their recruitment efforts, the effect of quits on hires will be positive. In contrast, if shocks on school attractiveness from the teachers' point of view are frequent, the effect will be negative. It is not possible to estimate a causal effect of quits on recruitment, but a significant positive correlation between the quit rate and the recruitment rate is present in the data.[18] Thus, if some schools become demand constrained because of the wage premium, as the positive correlation suggests might be the case, the recruitment elasticity will be underestimated by the lack of open positions. I consider this concern by including the quit rate in the model in some of the robustness checks.

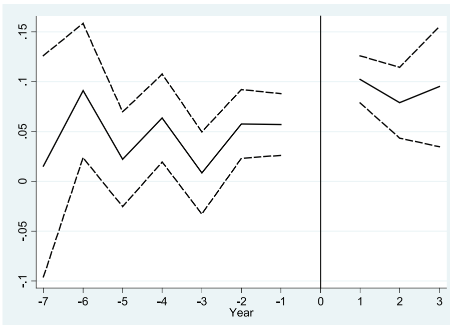
Third, a negative shock in supply might make the school eligible for the wage premium, and with mean reversion in the data, the estimated effect of the wage premium will be upward biased. To investigate the relevance of mean reversion, I estimate models that are conditioned on previous teacher shortages in various ways. I also perform a falsification test using the fact that eligibility for the wage premium was restricted to only some counties.

Fourth, according to Manning's (2003, 2011) model, the optimal behavior of firms facing an increased wage is to reduce their recruitment efforts. In that case, the estimate will be a combined effect of the wage premium and reduced recruitment costs, yielding a smaller estimate than the pure wage effect. In the specific labor market studied here, however, there is causal evidence for minor variations in recruitment activity. One reason for this is that all vacant positions had to be publicized in specific magazines. Potential teachers clearly knew where to find information on vacant positions, and information costs did not depend on the wage premium in this sense. If, nevertheless, schools and school districts changed their recruitment activity strategically, we would expect that the optimal behavior would also depend on circumstances other than the wage. I investigate whether the wage effect depends on the level of the teacher shortages in the previous school year and on the change in the number of students. Note, however, that this is not a direct test of whether the recruitment activity of the schools is endogenous.

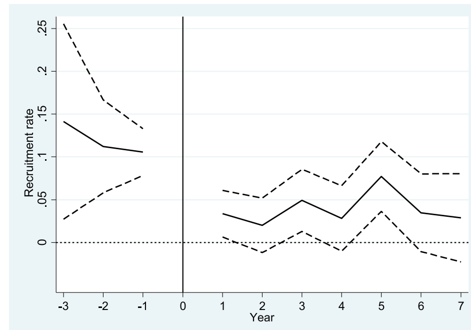
Some descriptive evidence can shed light on whether the two identification assumptions hold. Figure 3 investigates the assumption about parallel

[18]The within-school correlation coefficient between the share of certified teachers in permanent positions at the school who quit from year $t-1$ to year t and the share of certified teachers recruited in year t is 0.44 for the treatment schools.

Figure 3. Trends in the Pretreatment Relative Recruitment Rate with 95% Confidence Interval. Wage premium change in year 0



A. Wage premium introduced



B. Wage premium removed

trends. For the treatment schools, Figure 3A presents the dynamic pattern in recruitment in relation to the wage premium being introduced, and Figure 3B presents the pattern related to the wage premium being removed. Both changes contribute to the identification of the treatment effect.

The treatment occurred in different years at the different schools. Figure 3 handles unobserved year-specific factors by presenting the relative recruitment rate. The recruitment rate around treatment is presented relative to the recruitment rate at the nontreatment schools in the same year for each individual observation. The constant relative recruitment rate prior to the treatment supports the familiar parallel-trends assumption.[19]

Figure 3A shows that some variation—without any trend—is present in the relative recruitment rate prior to the wage premium and that the relative recruitment rate increased after the wage premium was introduced. Figure 3B indicates that a weak negative trend exists in the relative recruitment rate before the wage premium was removed but that a sharp decline occurs afterward.[20] The weighted difference between treatment and

[19]I use all the available information about the treatment schools when constructing Figure 3A. This includes schools in which the wage premium was introduced in 1993 and 1994, years when observing pretreatment recruitment in the data is not possible. Restricting the sample to schools with at least two years of observation prior to the wage premium, however, gives the same pattern. In Figure 3B, schools with wage premiums at the end of the empirical period cannot be included because the year the wage premium was removed is unknown. Restricting the sample to schools with at least two years of observations before the wage change also gives the same pattern.

[20]The estimates presented in Figure 3 are restricted to cases with at least 40 observations. Because most schools had a wage premium in only one to three years of the empirical period (see Table 1), few observations are available of teachers working in schools that had a wage premium in four subsequent years. Notice that the schools with observations seven years from the wage change are schools that were eligible for the wage premium in 2000 but observed in 1993 (Figure 3A) and schools that were eligible only in 1993 and observed in 2000 (Figure 3B).

nontreatment is 0.046 for the introduction of the wage premium (Figure 3A) and -0.069 for the removal of the wage premium (Figure 3B).

In principle, the regression discontinuity design is an alternative identification approach. Unfortunately, the information on teacher shortages used to define wage-premium eligibility is not available for the whole empirical period. Some official information is available from 1996, but Falch (2010) showed that the match between the actual eligibility and a classification based on public information on teacher-years is weak. The available data can be used to calculate an indicator for teacher shortages, and thus wage-premium eligibility, because they include information on teacher certification. This indicator is, however, only weakly related to actual wage premium for two reasons. First, the data were collected at different times, and noncertified teachers can be hired on very short-term contracts. Up to 1997/1998, eligibility was based on information collected only for this purpose at the start of the school year (August), whereas the present data were collected in October. Second, the official classification of shortages is based on the teacher's certification for the grade that the teacher is teaching, which is not included in the present data. Combined with relatively few schools being close to the eligibility threshold, this makes the regression discontinuity design infeasible.

Scale Effects

The model formulation in Equation (1) implies that the recruitment rate is linearly related to the wage premium. The wage premium yields more new teachers in large schools than in small schools. Relating this model formulation to the discussion of returns to scale in recruiting is useful. The theoretical models of imperfect competition assume a declining return to scale in recruitment activity (see in particular Manning 2003, 2006), but empirical evidence on this assumption is basically absent (see the discussion in Kuhn 2004). The question is whether the cost per worker of keeping employment constant depends on the number of workers. With diminishing returns, the costs of recruiting new hires increase with employment.

Consider the case in which institutional constraints force schools to have the same recruitment activity per teacher. Then the assumption of diseconomies of scale implies that, for a given wage, fewer teachers will be hired in large schools than in small schools. Likewise, a wage rise will increase employment more in small schools than in large schools. The present quasi-natural experiment is not well suited to estimating the nonlinear effects of the wage because the treatment schools tend to be small and, at the outside, exhibit characteristics that make them unattractive. Thus, the identification rests on within-school variation, which absorbs most of the variation in size. Nevertheless, I will present some evidence on the nonlinear effects of the wage using count models on data aggregated to the school level.

Table 4. Wage Effect

	Model 1	*Model 2*	*Model 3*	*Model 4*	*Model 5*
Wage premium	0.098*	0.073*	0.065*	0.064*	0.070*
	(0.013)	(0.014)	(0.014)	(0.015)	(0.018)
Treatment school * Time FE	No	Yes	Yes	Yes	Yes
School FE	No	No	Yes	Yes	Yes
Time FE * School-district FE	No	No	No	Yes	Yes
School-specific trends	No	No	No	No	Yes
Standard error of equation	0.361	0.360	0.356	0.355	0.354
Number of observations	401,305	401,305	401,305	401,305	401,305

Notes: The dependent variable is "Recently hired." The models are estimated using the linear probability model. Standard errors clustered at the school level are reported in parentheses. All models include the logarithm of the number of students and the change in the logarithm of the number of students. FE = fixed effects.
*Denotes significance at 5% level.

Main Results

Table 4, model 1, shows that the correlation between recruitment and the wage premium is relatively large. The probability that a teacher is recently hired is 9.8 pp higher when the wage premium is about 10%.[21] Model 2 includes two sets of year-specific effects, one set for the treatment schools and another set for the control schools. Then the correlation is reduced to 7.3 pp.

The main identification in this article is based on within-school variation. The result for the most simple difference-in-differences specification is presented in Table 4, model 3, with an effect of 6.5 pp. It follows from Equation (2) that this implies an average recruitment elasticity equal to 3.5. Including Year * School-district fixed effects to control for conditions in the choice set of teachers (model 4) does not alter the wage effect, while including School-specific trends (model 5) increases the estimate slightly to 7.0 pp (elasticity of 3.8). The finding that the estimated wage effect is robust to various ways of conditioning on unobserved variables indicates that features of the teachers' choice set beyond school characteristics are not related to the wage.

All the models in Table 4 include the level and the change in the logarithm of the number of students. Excluding these two variables from the model does not alter the results; for example, the effect of the wage premium for model 3 is reduced from 0.0647 to 0.0643 when they are excluded. This clearly indicates that variation in teacher demand does not drive the results.

[21]The estimated effect is larger than the difference in Table 2 because the sample includes the population of teachers, not just the observations for the treatment schools.

Robustness and Heterogeneity Analyses

Control Schools

The results might be sensitive to the choice of sample and the choice of control schools. The school districts in the counties with wage premium are relatively small. The first subsample used in Table 5 includes only school districts with fewer than 3,000 students, on average, during the empirical period. The treatment schools also tend to be small. The second subsample excludes schools with 175 students and above, on average, during the empirical period.

Most of the control schools do not have any challenges related to teacher recruitment. As such, they are not ideal comparison schools in the present context. Even though information on the size of teacher shortages used to define eligibility for the wage premium is not available (as previously discussed), the present data can be used to calculate an indicator for teacher shortages because they include information on certification. The third subsample used in Table 5 includes, in addition to the treatment schools, only schools with observed persistent teacher shortages located in counties other than the treatment schools. *Persistent teacher shortages* are shortages exceeding 10% on average during the empirical period, as measured by the indicator available in the present data. Because all these control schools are located in other counties, they were not eligible for the wage premium.

The fourth subsample includes only the counties with treatment schools. The last two subsamples include only teachers who worked at a treatment school during the empirical period; the fifth subsample includes all these teacher observations, while the sixth subsample is restricted to the treatment schools.

Table 5 shows that the estimated wage effect is stable across the subsamples. In model 1, the specification with only School fixed effects, the wage effect varies from 0.062 to 0.066. Including Year * School-district fixed effects (model 2) increases the variation in the estimate to the range 0.051 to 0.071, and for model 3, which includes School-specific trends, the estimate varies from 0.064 to 0.086. The results clearly show that the estimate is driven by the within-treatment school variation.

Omitted Variables

If schools became supply constrained by the wage premium, the recruitment elasticity will be underestimated by the lack of open positions. Truncation might be relevant because all observations do not have excess demand. In 21% of the observations of treatment schools, noncertified teachers were not employed, according to the present data. If the absence of excess demand and the wage premium are positively related, truncation yields a negative bias in the wage effect. The correlation between the wage premium and absence of excess demand is very low, however, indicating

Table 5. Wage Effect, Using Different Subsamples

Subsample	*Model 1*	*Model 2*	*Model 3*
1. Only school districts with fewer than 3,000 students on average during the empirical period[a]	0.066*	0.065*	0.074*
	(0.015)	(0.015)	(0.018)
Number of observations	252,870	252,870	252,870
2. Only schools with fewer than 175 students on average during the empirical period[b]	0.062*	0.071*	0.076*
	(0.015)	(0.015)	(0.018)
Number of observations	134,813	134,813	134,813
3. Control schools restricted to schools located outside the relevant counties and with teacher shortages of at least 10% on average during the empirical period[c]	0.063*	0.071*	0.086*
	(0.014)	(0.016)	(0.020)
Number of observations	20,691	20,691	20,691
4. Only counties with treatment schools[d]	0.064*	0.063*	0.067*
	(0.014)	(0.015)	(0.018)
Number of observations	51,275	51,275	51,275
5. Only teachers who worked at a treatment school during the empirical period[e]	0.063*	0.062*	0.075*
	(0.014)	(0.016)	(0.021)
Number of observations	10,473	10,473	10,473
6. Only treatment schools	0.062*	0.068*	0.082*
	(0.015)	(0.016)	(0.021)
Number of observations	6,959	6,959	6,959
School FE	Yes	Yes	Yes
Time FE * School-district FE	No	Yes	Yes
School-specific trends	No	No	Yes

Notes: The dependent variable is "Recently hired." The model specifications are the same as in Table 4, models 3 to 5. Standard errors clustered at the school level are reported in parentheses. FE = fixed effects.
[a]This restriction excludes the two most populous school districts in the relevant counties (both of which had treatment schools) and 35 school districts in other parts of the country, leaving a total of 407 school districts and 2,497 schools in the sample.
[b]This restriction excludes the largest treatment school and 1,263 control schools, leaving a total of 424 school districts and 2,050 schools in the sample.
[c]This restriction reduces the sample by 94.8%. The sample includes 94 school districts and 185 schools outside the three counties with treatment schools. None of the treatment schools is excluded from the sample, although the average teacher shortages, as measured by the available information in the present data, are less than 10% in 25% of the treatment schools. Excluding these schools from the sample does not affect the results.
[d]This restriction reduces the sample by 87.2%, leaving a total of 292 school districts and 953 schools in the sample.
[e]This restriction reduces the sample by 98.3%, leaving a total of 69 school districts and 158 schools in the sample.
*Denotes significance at 5% level.

that the bias is small.[22] The first part of Table 6 (models 1 and 2) restricts the sample to schools with observed excess demand. Contrary to

[22]The within-school correlation coefficient between the wage premium and absence of excess demand is 0.01 for all schools and 0.03 for the treatment schools, which are both insignificant at conventional levels. Notice that the wage premium and lagged excess demand are correlated in the treatment schools, not the wage premium and present excess demand. In addition, the correlation should be reduced by the change in eligibility rules over time.

expectations, the estimated wage effect is lower than for the models including all schools in Table 4, but the differences are small (0.056 compared to 0.064 in the model with Year * School-district fixed effects).

Because a low level of shortages might be related to teacher positions with very low workloads, Table 6, models 3 and 4 include only school observations with at least 15% teacher shortages. In this case, the number of observations drops considerably, but the estimated wage effect does not change much. Overall, these results indicate that truncation is not an important issue.

In general, more quits can induce an intensified recruitment effort. In Table 6, models 5 and 6, I address this concern by including the quit rate at the school level in the model. More quits yields more open positions. On average, including all schools, a positive association exists between quits and hires. This indicates that the recruitment elasticity is underestimated by the lack of open positions. Although the estimated recruitment elasticity increases by the inclusion of the quit rate, the difference compared to the models in Table 4 is modest. The difference is 0.5 to 0.7 standard errors.

Stochastic shocks in teacher shortages might drive the results because eligibility for the wage premium is related to previous shortages. In Table 6, models 7 and 8, I address this issue by including three lags in the share of noncertified teachers at the present school, as measured by the available data starting in 1992/1993. Including the previous teacher shortages reduces the wage effect by about one standard error.[23] Models 9 and 10 include, in addition, indicators for whether teacher shortages were increasing or declining, with an additional small reduction in the wage effect (0.046 compared to 0.048). Although the wage effect is smaller in these models, it is not significantly different from the estimates in Table 4; however, it is still significantly different from 0 at the 5% level.

The bias related to potential mean reversion should be smaller for the school years 1998/1999 and onward because the eligibility criterion changed from teacher shortages in the previous school year to average shortages over the previous four school years. In Table 6, models 11 and 12, I restrict the sample to the school years 1998/1999 to 2000/2001, which yields estimates that are very similar to the models taking lagged teacher shortages into account.

The relevance of mean reversion can also be considered by investigating patterns in the counties without wage-premium schools. A falsification test is not straightforward because information on the size of the teacher shortages used to define wage-premium eligibility is not available, as previously discussed. Nevertheless, I have created a dummy variable equal to unity if the teacher shortages at the school as measured by the present individual data were at least 20% in the previous school year and the average shortages during the empirical period were at least 10%. In a sample excluding the counties with treatment schools, this fictitious wage premium

[23]The estimate is 0.067 when using the sample in Table 6, model 8, on the main model specification, compared to 0.048 in Table 6, model 8, when lags in the teacher shortages are included.

Table 6. Wage Effect and Sensitivity to Omitted Characteristics

Sample	*Teacher vacancies present year*		*Teacher vacancies >15% present year*		*All observations*		*Time period: 1995/1996 to 2000/2001*				*Time period: 1998/1999 to 2000/2001*		*Only counties without treatment schools*	
	Model 1	*Model 2*	*Model 3*	*Model 4*	*Model 5*	*Model 6*	*Model 7*	*Model 8*	*Model 9*	*Model 10*	*Model 11*	*Model 12*	*Model 13*	*Model 14*
Wage premium	0.059*	0.056*	0.070*	0.047	0.072*	0.074*	0.049*	0.048*	0.047*	0.046*	0.049*	0.050	—	—
	(0.018)	(0.019)	(0.030)	(0.037)	(0.013)	(0.014)	(0.018)	(0.019)	(0.018)	(0.019)	(0.024)	(0.027)		
Fictitious wage premium	—	—	—	—	—	—	—	—	—	—	—	—	−0.014	−0.0002
													(0.029)	(0.026)
Quit rate from permanent positions at the school	—	—	—	—	0.448*	0.437*	—	—	—	—	—	—	—	—
					(0.018)	(0.018)								
Share of vacant teacher positions ($t-1$)	—	—	—	—	—	—	0.243*	0.234*	0.262*	0.262*	—	—	0.239*	0.226*
							(0.051)	(0.045)	(0.076)	(0.070)			(0.046)	(0.042)
Share of vacant teacher positions ($t-2$)	—	—	—	—	—	—	−0.033	−0.013	−0.028	−0.018	—	—	−0.044	−0.019
							(0.024)	(0.020)	(0.028)	(0.025)			(0.030)	(0.025)
Share of vacant teacher positions ($t-3$)	—	—	—	—	—	—	−0.045	−0.034	−0.047	−0.038	—	—	−0.047	−0.031
							(0.032)	(0.030)	(0.020)	(0.003)			(0.035)	(0.033)
Indicators for change in teacher shortages[a]	No	No	No	No	No	No	No	No	Yes	Yes	No	No	No	No
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE * School-district FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Standard error of equation	0.362	0.361	0.396	0.396	0.354	0.353	0.363	0.362	0.363	0.362	0.364	0.363	0.363	0.362
Number of observations	223,241		14,967		399,403		306,594				165,210		267,677	

Notes: The dependent variable is "Recently hired." The model specifications are the same as Table 4, models 3 and 4, except as indicated. Standard errors clustered at the school level are reported in parentheses. FE = fixed effects.

[a]Four dummy variables are included; indicators for increasing or decreasing share of vacant teacher position from $t-1$ to t and from $t-2$ to $t-1$ (No change is the reference category).

*Denotes significance at 5% level.

occurs for 3,372 teachers and 443 school observations, which are larger numbers than for the actual wage premium but smaller in terms of percentages. In Table 6, models 13 and 14, I present the results. The estimated effect of the fictitious wage premium has the wrong sign, but it is close to 0 and clearly insignificant.

In general, employers also have policy instruments other than the wage, as emphasized by Manning (2003, 2011). A wage raise can be combined with reduced hiring costs, in which case the pure wage effect is underestimated. When a school becomes eligible for the wage premium, and if hiring activity is endogenous, reasonable changes in hiring activity depend on circumstances. For example, if recruitment is most important with large teacher shortages, hiring activity can be expected to decrease when shortages decline. In this case, the estimated wage effect is increasing in teacher shortages. The same logic applies for changes in teacher demand, which are measured by the change in the number of students.

Figure 4 presents the results from a semi-parametric model specification with Year * School-district fixed effects included. Figure 4A shows that the effect of the wage premium is not systematically related to previous teacher shortages. The regression line, estimated on the point estimates in the figure using the inverse of the standard errors as weight, is only slightly upward sloping. Figure 4B shows that the wage effect is also not systematically related to the change in school size.[24]

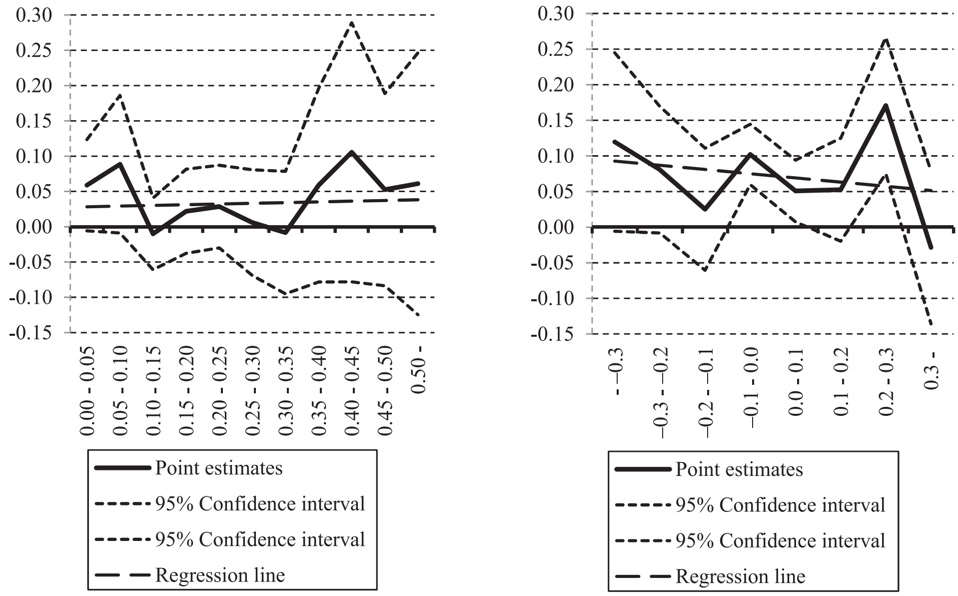
Individual Characteristics

The estimates so far can be interpreted as the wage effect from the point of view of the establishment, without regard to what type of workers that were hired, including worker productivity. Although no direct measure of the teachers' productivity or ability is available, we can condition on unobserved teacher characteristics using the present longitudinal data.

First, I investigate whether the characteristics of the recently hired teachers are related to the wage. I estimate the same model specification as before, using different characteristics of the recently hired teachers as the dependent variable. Table 7 shows that the wage is not related to whether the hire was a woman, was married, had school-age children, or had a permanent teacher position during the previous year. But the wage premium does increase the probability that the hire was young. Some evidence indicates that teacher quality increases with experience (Rivkin, Hanushek, and Kain 2005), which suggests that the wage might be negatively related to productivity rather than the opposite as we would expect (Kuhn 2004). This suggests that including individual characteristics in the empirical model will underestimate the wage effect on recruitment at the establishment level.

[24]The average effect of the wage premium is smaller in Figure 4A than in Figure 4B. Because Figure 4A allows for different wage effects at different levels of lagged teacher shortages, the model effectively conditioned on lagged shortages. In that case, the wage effect is smaller, as shown in Table 8.

Figure 4. Nonlinear Wage Effects on Recruitment



A. Degree of teacher shortages B. Change in log number of students

The wage seems not to be positively related to teacher quality, but using conditional models we can infer on variation among less mobile workers.

Table 8 presents estimates for recruitment conditional on individual characteristics. The full models are presented in the Appendix and show that young teachers, divorced teachers, and teachers with school-age children were more likely to be recently hired than other teachers. The effect of age is strong and nonlinear. For example, for a teacher at age 30 the marginal effect is −3.6 pp. Some of these characteristics, and in particular age, are correlated with the wage premium simply because the share of recently hired teachers was larger at schools with the wage premium. Thus, when observable individual characteristics are included in Table 8, model 1, the estimated effect of the wage premium declines from 7.0 pp (in Table 4, model 5) to 5.1 pp. The decline, however, is not statistically significant at conventional levels.

Including teacher fixed effects (Table 8, model 2) and an indicator for whether the teacher had a permanent teacher position during the previous year (model 3) reduce the wage effect even more. In the former model, it is significant only at the 10% level.[25] The indicator for permanent position

[25]Because the dependent variable is a dummy variable, it varies only for individuals who de facto moved during the sample period in the within-individual framework. The weight on mobile individuals is higher than in the other models, which should work in the direction of a larger wage effect. Excluding teachers who were not recently hired during the empirical period (33,947 teachers and 212,585 observations), the wage effect in a model specification similar to Table 4, model 3, increases from 0.065 to 0.092.

Table 7. Wage Effect on Characteristics of Recently Hired Teachers

	Dependent variable					
	Women	*Age younger than 38*	*Age*	*Married*	*With children ages 6–18*	*Permanent teacher position last year*
	Model 1	*Model 2*	*Model 3*	*Model 4*	*Model 5*	*Model 6*
Wage premium	0.029	0.072*	−1.587*	−0.003	−0.031	0.034
	(0.031)	(0.029)	(0.658)	(0.042)	(0.034)	(0.030)
Treatment school* Time FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Standard error of equation	0.442	0.480	9.800	0.487	0.466	0.411
Number of observations	62,158	62,158	62,158	62,158	62,158	62,158
Mean of dependent variable	0.719	0.589	36.02	0.495	0.341	0.229

Notes: The sample is recently hired teachers. The models are estimated by the linear probability model. Standard errors clustered at the school level are reported in parentheses. All models include the logarithm of the number of students and the change in the logarithm of the number of students.
FE = fixed effects.
*Denotes significance at 5% level.

is highly significant, clearly confirming that the mobility of teachers in permanent positions is much lower than of other teachers.[26] Without tenure, the teacher can be forced to move, and the probability of being observed as recently hired is larger.

Individual Heterogeneity

The literature on labor supply has found that women's working hours are more responsive to the wage than men's working hours (see Killingsworth and Heckman 1986; Pencavel 1998). In addition, Pencavel found that the wage elasticity is highest for married women and young women. Translating these supply elasticities at the market level into mobility responsiveness is not straightforward. We could expect that people who respond strongly in terms of working hours also respond strongly in terms of mobility. The usual interpretation, however, is that women are more responsive in terms of hours because an attractive alternative for many is to stay at home. But then they become less geographically mobile, working in the direction of less mobility responsiveness. At the firm level using individual data, Manning (2003) found similar elasticities for both genders, and Hirsch et

[26]Teachers who had a permanent position in the previous school year change schools in 4.5% of the observations, whereas teachers who had a temporary teacher position in the previous year change schools in 19% of the observations. In 9.9% of the observations, the teacher did not have a teacher position in the previous year (see Table 3) and are thus by definition recently hired.

Table 8. Wage Effect, including Individual Characteristics

	Model 1	*Model 2*	*Model 3*
Wage premium	0.051*	0.030	0.030*
	(0.017)	(0.017)	(0.015)
Permanent teacher position last year	—	—	−0.432*
			(0.005)
School FE	Yes	Yes	Yes
Time FE * School-district FE	Yes	Yes	Yes
School-specific trends	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes
Individual fixed effects	No	Yes	Yes
Standard error of equation	0.325	0.268	0.246
Number of observations	401,152	401,152	401,152

Notes: The dependent variable is "Recently hired." The model specification is the same as in Table 4, model 5, except as indicated. Standard errors clustered at the school level are reported in parentheses. The individual characteristics included are a cubic in age; gender; dummy variables for marital status (unmarried, married, divorced, and widow/widower); dummy variables for children younger than 6 years of age, children 6 to 18 years of age, children older than 18 years of age, and no children; dummy variable for whether the teacher is on leave; dummy variable for reduced working hours; dummy variables for years of education; dummy variable for leader position; and dummy variable for whether the teacher works in the same region as born. All coefficients for models 1 and 3 are reported in the Appendix. FE = fixed effects.
*Denotes significance at 5% level.

al. (2010) and Ransom and Oaxaca (2010) found that women's labor supply is less elastic than men's.

Table 9 presents the heterogeneous effects related to the socioeconomic characteristics of the teachers. The wage effect seems to be largest for female teachers, married teachers, young teachers, and teachers without school-age children. Except for age, however, the wage effects are not statistically different across groups. The table also reports the results for models (9 and 10) in which the wage effect is allowed to differ for teachers who had a permanent teacher position in the previous school year. If these teachers are observed as being recently hired, they certainly preferred the present school over the previous school. I find that they seem to have a lower wage response than other teachers, but again the difference in the wage effect across the groups is not significant at the 5% level.

Stability and Dynamic Effects

Even though the wage response depends on individual characteristics, we would expect the same effect across time and regions to the extent that the composition of the teachers does not change. Table 10, models 1 and 2, allows for different wage effects over time. The point estimates indicate that the effect was largest in 1996/1997 to 1997/1998, when the eligibility criteria was strictest, and lowest under the latest eligibility criterion. For model 2, the point estimates are 0.153 and 0.051, respectively, but the difference is insignificant at conventional levels.

Three counties were included in the system. Table 10, models 3 and 4, indicates that the wage effect was largest in the northernmost county (Finnmark, effect of 0.099 in model 4) and lowest in the southernmost of the three counties (Nordland, effect of 0.035), but again the differences are not significant.

The model formulation assumes that the wage response is static in the sense that only the present wage matters for the individual decisions. The last model formulations in Table 10 allow for different wage effects the first and the last years of wage premium. If which schools that would pay a wage premium was well known in advance, as intended, the wage effect should not be smaller in the first year a school was eligible for the wage premium than in the consecutive years. For the last year with wage premium, the effect is expected to be lower if teachers have forward-looking behavior. In the model with only school fixed effects (model 5), the wage effect is clearly independent of the timing. In the model with Year * School-district fixed effects (model 6), the point estimates indicate that the effect is largest in the last year with the wage premium (0.076) and smallest in the intermediate years (0.033). The differences are, however, not statistically significant.

Scale Effects

The model specification I have used so far assumes that the wage premium yields more hires in large schools than in small schools, the opposite of what follows from diminishing returns in the recruiting technology. The models presented in Table 11 have interaction terms between school size and the wage using data aggregated to the school level. The models mainly do not include school fixed effects to maintain variation in school size, which makes the identification of the wage effect weaker than in the previous models.

To compare the school-level models to the previous results, in Table 11, I use the share of new hires at the school level as the dependent variable in model 1. The effect is about 1 standard error larger than in the corresponding model in Table 4 (model 2). This is fully related to the implicit weighting when using individual data. Small schools have relatively fewer observations in the individual-level models in Table 4 than in the school-level models in Table 11. This effect of weighting indicates that the wage response is largest in small schools. Model 2 in Table 11 estimates this heterogeneity directly by allowing the effect of the wage premium to depend on the number of students. The interaction term is negative and significant at the 10% level. The estimated coefficients imply that the effect of the wage premium is positive only for schools up to about 500 students.

Models 3 to 6 in Table 11 use the number of new hires as the dependent variable and estimate count models. The results in model 3 imply that the wage premium increases the number of new hires by 0.72 teacher on

Table 9. Heterogeneous Wage Effects across Characteristics of Teachers

	Sample									
	Female	*Male*	*Married each year*	*Not married each year*	*Average age younger than 38*	*Average age older than 38*	*No children ages 6–18 any year*	*Children ages 6–18 at least one year*	*Permanent position last year*	*No permanent position last year*
	Model 1	*Model 2*	*Model 3*	*Model 4*	*Model 5*	*Model 6*	*Model 7*	*Model 8*	*Model 9*	*Model 10*
Wage premium	0.067*	0.046	0.053*	0.037	0.114*	0.022	0.081*	0.045*	0.036*	0.060
	(0.019)	(0.024)	(0.018)	(0.029)	(0.028)	(0.015)	(0.024)	(0.020)	(0.010)	(0.035)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE * School-district FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Standard error of equation	0.367	0.326	0.302	0.424	0.460	0.276	0.381	0.325	0.203	0.476
Number of observations	265,868	135,352	254,633	100,051	109,188	292,117	185,680	215,625	317,286	84,019

Notes: The dependent variable is "Recently hired." The model specification is the same as in Table 4, model 4, except for the sample. Standard errors clustered at the school level are reported in parentheses. FE = fixed effects.
*Denotes significance at 5% level.

Table 10. Stability and Dynamic Effects

Variable	*Model 1*	*Model 2*	*Model 3*	*Model 4*	*Model 5*	*Model 6*
Wage premium	0.079* (0.025)	0.059* (0.024)	0.086* (0.021)	0.099* (0.25)	0.071* (0.026)	0.033 (0.024)
Wage premium * School years 1996/1997 to 1997/1998	0.065 (0.059)	0.094 (0.058)	—	—	—	—
Wage premium * School years 1998/1999 to 2000/2001	−0.036 (0.034)	−0.008 (0.031)	—	—	—	—
Wage premium * County of Nordland	—	—	−0.051 (0.030)	−0.064 (0.033)	—	—
Wage premium * County of Troms	—	—	−0.022 (0.031)	−0.025 (0.036)	—	—
Wage premium * No wage premium last year	—	—	—	—	−0.007 (0.024)	0.015 (0.023)
Wage premium * No wage premium next year	—	—	—	—	−0.002 (0.024)	0.043 (0.025)
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE * School-district FE	No	Yes	No	Yes	No	Yes
Standard error of equation	0.356	0.355	0.356	0.355	0.356	0.355
Number of observations	401,305	401.305	401,305	401,305	401,305	401,305

Notes: The dependent variable is "Recently hired." The model specifications are the same as in Table 4, models 3 and 4, except as indicated. Standard errors clustered at the school level are reported in parentheses. FE = fixed effects.
*Denotes significance at 5% level.

average.[27] However, the wage effect significantly declines as school size increases. The coefficients in Table 11, model 4, imply that the effect of the wage premium is positive only for schools with fewer than 165 students. The qualitative result holds in model 5, which includes school fixed effects, although the precision declines.

An alternative measure of school size is the number of incumbent teachers, which is included in Table 11, model 6.[28] The wage effect also declines as the number of teachers increases.

These results indicate diminishing returns to scale. The results must, however, be interpreted with caution because the identification is weak in models without School fixed effects. In addition, potential variation in the recruitment efforts of the schools is not observed.

[27]The average number of new hires is 2.5 teachers, which implies that the estimated coefficient in model 3 in Table 11 amounts to 0.72 new hires. For treatment schools, the average number of new hires is 1.2 teachers. Fewer hires in the treatment school are attributable to the schools' being smaller (see Table 2).

[28]The correlation coefficient between the number of incumbent teachers and the logarithm of the number of students is 0.84.

Table 11. Scale Effects in School-Level Analyses

	Dependent variable					
	Share of new hires		*Number of new hires*			
	Model 1	*Model 2*	*Model 3*	*Model 4*	*Model 5*	*Model 6*
Estimation method	*Ordinary least squares*		*Negative binomial regression model*			
Wage premium	0.087*	0.161*	0.288*	0.785*	0.633*	0.498*
	(0.018)	(0.052)	(0.068)	(0.221)	(0.280)	(0.103)
Wage premium * log(Number of students)	—	−0.026	—	−0.154*	−0.082	—
		(0.015)		(0.062)	(0.077)	
Wage premium * Number of incumbent teachers	—	—	—	—	—	−0.043*
						(0.018)
log(Number of students)	−0.016*	−0.016*	0.695*	0.696*	0.411*	0.783*
	(0.002)	(0.002)	(0.015)	(0.015)	(0.031)	(0.041)
Change in log(Number of students)	0.178*	0.178*	1.093*	1.093*	1.146*	1.003*
	(0.012)	(0.012)	(0.084)	(0.084)	(0.043)	(0.075)
Number of incumbent teachers	—	—	—	—	—	−0.009*
						(0.003)
Treatment school * Time FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	No	No	No	No	Yes	No
Number of observations	25,089	25,089	25,089	25,089	25,089	25,089

Notes: The models use data that are aggregated to the school level. Standard errors clustered at the school level are reported in parentheses. FE = fixed effects.
*Denotes significance at 5% level.

A Labor-Supply Interpretation

The labor supply S faced by an establishment can be decomposed into the current incumbent workers I, new hires R, and workers who want to work at the establishment but who are not hired D.

$$S_{jt} = I_{jt} + R_{jt} + D_{jt} = (1 - q_{jt})N_{jt-1} + R_{jt} + D_{jt} \tag{3}$$

where N is employment, and the quit rate q is a flow variable. Krueger (1988), Holzer et al. (1991), and Bó et al. (2013) estimated the elasticity of job applicants with respect to the wage, which can be seen as the wage response of $(R + D)$.

The labor market analyzed in the present article is characterized by supply-constrained employment; that is, $D_{jt} = 0$, which implies that $N_{jt} = S_{jt}$. Denoting the log(Wage) by w, it follows from Equation (3) that the short-run labor supply elasticity can be written as:

$$(4) \qquad \varepsilon_{SW} = \frac{\partial S_{jt}}{\partial w_{jt}}\frac{1}{S_{jt}} = \left(\frac{\partial R_{jt}}{\partial w_{jt}} - \frac{\partial q_{jt}}{\partial w_{jt}}S_{jt\text{-}1}\right)\frac{1}{S_{jt}} = \varepsilon_{RW}\frac{S_{jt} - (1-q_{jt})S_{jt\text{-}1}}{S_{jt}} - \varepsilon_{qW}\frac{q_{jt}S_{jt\text{-}1}}{S_{jt}}$$
$$= (1-(1-q_{jt})\gamma_{jt})\varepsilon_{RW} - \gamma_{jt}q_{jt}\varepsilon_{qW}$$

where ε_{RW} and ε_{qW} are the recruitment and quit elasticities, respectively, and $\gamma_{jt} = S_{jt-1}/S_{jt}$ is the inverse of the growth in labor supply.[29]

When separations to and recruitment from nonemployment are insensitive to the wage, it follows from Manning (2003) that:

$$(5) \qquad \varepsilon_{qW} = -\frac{(1-(1-q_{jt})\gamma_{jt})}{q_{jt}}\varepsilon_{RW}$$

A fixed relationship exists between the quit and recruitment elasticities.[30] Several studies assumed that $\gamma_{jt} = 0$ and used this relationship to infer the labor-supply elasticity based on estimates of the separation elasticity (e.g., Ransom and Oaxaca 2010). Based on the results presented here and in Falch (2011), we can empirically consider the appropriateness of the relationship in Equation (5).

In Falch (2011), I estimated the quit behavior of teachers in permanent positions exploiting the same wage-premium experiment as in the present article, and I found that $\varepsilon_{qW} = -3.5$.[31] To consider the appropriateness of Equation (5), we also need estimates of q and γ. The average quit rate at the treatment schools was about 0.18 (Falch 2011). In the present sample, the number of observations increased by 3.2% per year on average, and the average growth in the number of students was 2.7%. The large growth in the number of students is mainly attributable to the reduced starting age of compulsory education from seven to six years of age during the empirical period. Thus, γ is about 0.97, and from Equation (5), ε_{RW} should be equal to 3.1.

The estimates in Table 4, models 3 to 5, imply an average recruitment elasticity of about 3.5 to 3.8. This value is reasonably close to expectations and slightly larger than the finding by Bó et al. (2013) for new public-sector positions in Mexico. Using Equation (4), we find that the implied

[29]Bó et al. (2013) considered a situation in which a new job is created. In that case, the supply elasticity is related only to recruitment. They decompose the recruitment effect into the effects on the applicant pool and the conversion rate of selected candidates into the vacancies filled.

[30]If we relax the assumption that separations to and recruitment from nonemployment are insensitive to the wage, Manning's (2003) model implies that the relationship in Equation (5) still holds in steady state under the following assumptions: 1) The elasticities for separations to employment and to nonemployment are equal, and 2) the share of recruits from nonemployment is insensitive to the wage. The evidence in Manning (2003), Hirsch et al. (2010), and Hotschkiss and Quispe-Agnoli (2012) indicated that the estimated supply elasticity is not very sensitive to such corrections. In fact, the positive effect of the wage on the share of recruits from employment found by Manning and Hirsch et al. implied that the supply is less elastic than what follows from Equations (4) and (5).

[31]This result is similar to findings by Clotfelter, Glennie, Ladd, and Vigdor (2008). They exploited a three-year-long bonus program in North Carolina public schools that served low-income or low-performing students and found a quit elasticity on the order of −3 to −4.

supply elasticity is 1.33 to 1.39. Notice that this is a short-run elasticity, which makes sense in the present case because the policy intervention was short term in nature. This is almost identical to the findings in Falch (2010), in which the same experiment was analyzed for the period 1995/1996 to 2000/2001 using a static model and school-level data on employment.[32]

In addition, I find that the wage effect to some extent depends on individual characteristics, and not always in the same way for the quit and recruitment decisions. The results for quit behavior (Falch 2011) and recruitment behavior (Table 7) are summarized in Table 12. With regard to quits, male teachers and old teachers seem to respond more strongly to the wage than do female teachers and young teachers. But the opposite seems to be the case with regard to recruitment. The age effect might differ because retirement decisions may be important for the observed quits, whereas the observed recruitment behavior might be driven by teachers who do not currently have permanent positions. Thus, even though the quit and recruitment elasticities seem to be similar for the particular labor market considered here, they clearly differ across genders and age groups in this market.[33]

Conclusion

Causal evidence of wage effects on recruitment has remained limited despite the fact that theoretical models with frictions in the labor market have mainly focused on the recruitment process. The main empirical challenges are that the wage offered is endogenous and that additional recruitment instruments are in the hand of employers. In the present article, I exploit a wage-premium system in a completely centralized wage-setting institution for teachers in Norway using a difference-in-differences framework.

The identification relies on excess demand for teachers at the school level and on other recruitment policies of the employers being unrelated to the wage premium. The particular institutional setting in the specific labor market under study arguably indicates that these assumptions are reasonable. The finding that the wage effect is robust to several robustness checks also suggests that the assumptions hold. An important caveat, however, is

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[32]Matsudaira (2014) exploited a legislation that increased the minimum number of nurses in the long-term-care industry in California. This is a different quasi-natural experiment than I exploit in the present article because it increased the employment and not the wage. Matsudaira found evidence of a very elastic supply curve and could not reject the neoclassical model.

[33]Table 12 provides effects in percentage points and not elasticities. In addition, the findings for quit decisions in Falch (2011) are estimated on a smaller sample than the sample in the present article. Thus, I do not present formal tests of equality of the quit and recruitment elasticities for the different subsamples. Using the formulas and the mean values already presented, the point estimates in the recruitment model ($\hat{\kappa}$) should be about 25% higher than the point estimates of the quit model ($\hat{\varphi}$) if Equation (5) holds. From Table 11, $\hat{\varphi}$ is not within the 95% confidence interval of $\hat{\kappa} * 1.25$ in the subsamples for females, males, young, and old.

Table 12. Heterogeneous Wage Effects on Recruitment and Quit

	Female	*Male*	*Married each year*	*Not married each year*	*Average age younger than 38*	*Average age older than 38*	*No children ages 6–18 any year*	*Children ages 6–18 at least one year*
Wage effect on	0.067*	0.046	0.053*	0.037	0.114*	0.022	0.081*	0.045*
recruitment	(0.019)	(0.024)	(0.018)	(0.029)	(0.028)	(0.015)	(0.024)	(0.020)
Wage effect	−0.040	−0.110*	−0.092*	−0.028	−0.045	−0.073*	−0.086*	−0.067*
on quits	(0.026)	(0.034)	(0.026)	(0.031)	(0.042)	(0.023)	(0.037)	(0.028)

Note: Results are as reported in Table 8 and in Falch (2011: table 3).

that I am unable to directly test whether schools' recruitment activity depended on the wage premium.

The wage premium of almost 10% is found to increase recruitment by about 30%, which implies a short-run recruitment elasticity of about 3.5. This elasticity is similar in absolute terms to previous published findings for separations. The wage responsiveness is significant but not massive. Interpreted in a labor-supply framework, the results imply a short-run labor-supply elasticity of about 1.4. The establishments face an upward-sloping supply curve of labor in the short run.

Economic theory predicts that the separation and recruitment elasticities with respect to the wage are equal in absolute terms when the flows of workers to and from nonemployment are insensitive to the wage. Despite the fact that 70% of new hires were recruited from outside the specific labor market in this study, the empirical results are in accordance with the theory. Nevertheless, this does not hold for subgroups of teachers. The findings indicate that recruitment is more responsive to the wage for young teachers than for old teachers and that the opposite is the case for quit decisions. Likewise, the wage responsiveness of female teachers is stronger with respect to recruitment than with respect to quits, whereas the opposite is the case for male teachers.

Search-theoretic models of imperfect competition in the labor market assume diminishing returns to scale in recruitment activity. The cost of recruiting increases with employment, and a given wage attracts fewer workers in large establishments than in small establishments. The evidence presented in this article supports this assumption. The effect of the wage on the number of new hires seems to decline as establishment size increases.

Appendix

Full Models

Table A.1. Full Models

		Coefficient	
Variable	*Mean*	*Table 7, Model 1*	*Table 7, Model 3*
Wage premium	0.004	0.051*	0.030*
		(0.014)	(0.015)
log(Number of students)	5.29	−0.005	−0.101*
	[0.76]	(0.018)	(0.014)
Change in log(Number of students)	0.027	0.206*	0.184*
	[0.108]	(0.017)	(0.014)
Women	0.663	0.0002	—
		(0.0013)	
Married at present	0.696	−0.028*	−0.035*
		(0.003)	(0.008)
Divorced	0.092	0.025*	−0.029*
		(0.003)	(0.009)
Widow/widower	0.014	−0.003	−0.015
		(0.005)	(0.011)
Unmarried	0.198	Ref.	Ref.
Children younger than 6 years of age	0.169	−0.012*	−0.005
		(0.002)	(0.003)
Children 6–18 years of age	0.409	0.019*	0.020*
		(0.002)	(0.002)
Children older than 18 years of age	0.501	0.004*	−0.003
		(0.002)	(0.002)
No children	0.182	0.004	−0.035*
		(0.003)	(0.008)
On leave	0.012	−0.099*	−0.004
		(0.004)	(0.005)
Reduced working hours	0.256	0.060*	0.028*
		(0.002)	(0.002)
4 years of higher education and nonleader position	0.411	−0.010*	−0.011*
		(0.002)	(0.004)
5 years of higher education and nonleader position	0.209	−0.001	−0.029*
		(0.002)	(0.006)
More than 5 years of higher education and nonleader position	0.024	0.063*	−0.029
		(0.005)	(0.016)
Years of higher education missing and nonleader position	0.009	−0.070*	−0.036*
		(0.005)	(0.008)
Leader position	0.101	0.021*	−0.004
		(0.002)	(0.006)
3 years of higher education and nonleader position	0.255	Ref.	Ref.
Work in the same region as born	0.352	−0.017*	0.129
		(0.001)	(0.117)
Region of birth missing	0.137	−0.008*	—
		(0.002)	
Working in another region than born in	0.511	Ref.	Ref.
Age	44.3	−0.216*	−0.436*
	[10.3]	(0.003)	(0.009)

(continued)

Table A.1. Continued

Variable	Mean	Coefficient	
		Table 7, Model 1	*Table 7, Model 3*
$Age^2/10$	207	0.043*	0.085*
	[89.5]	(0.001)	(0.002)
$Age^3/1,000$	101	−0.029*	−0.055*
	[61.5]	(0.001)	(0.001)
Permanent position last year	0.791	—	−0.432*
			(0.005)
Time FE * Treatment school; number of FE	—	Yes; 14	Yes; 14
School FE; number of FE	—	Yes; 3,313	Yes; 3,313
Time FE * School-district FE; number of FE	—	Yes; 3,036	Yes; 3,036
School-specific trends; number of trends		Yes; 3,313	Yes; 3,313
Individual fixed effects; number of FE	—	No	Yes; 79,062
Standard error of equation	—	0.325	0.246
Number of observations	401,152	401,152	401,152

Notes: Estimated using the linear probability model. Standard errors clustered at the school level are reported in parentheses. Standard deviations of the means appear in brackets. FE = fixed effects; Ref. = reference.
*Denotes significance at 5% level.

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