The Elasticity of Labor Supply at the Establishment Level

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Monopsonistic wage-setting power requires that the supply of labor directed toward individual establishments is upward sloping. This study utilizes institutional features to identify the supply curve. The elasticity of labor supply is estimated using data for the Norwegian teacher labor market in a period where the only variation in the wage level was determined centrally and with information on whether there is excess demand or not at the school level. In fixed-effects models, the supply elasticity faced by individual schools is estimated to about 1.4 and is in the range 1.0–1.9 in different model specification.

I. Introduction

Recently there has been increasing interest in models where firms have monopsonistic wage-setting power in the labor market. Several empirical labor market phenomena, for example, wage dispersion across identical

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workers and nonnegative employment effects of minimum wages, are hard to explain by perfectly competitive models but easily explained by frictions in the labor market. Imperfect information, mobility costs, and heterogeneous preferences over nonwage attributes that make employers imperfect substitutes are all plausible causes of imperfect competition. It may be argued that the conditions for perfect competition are violated to a greater extent in the labor market than in most product markets because it is relatively costly to change one's job.

Wage-setting power of firms requires that the labor supply is not perfectly elastic at the firm level. While several empirical regularities can be explained by imperfect competition, rejection of the hypothesis of horizontal labor supply is the direct test of the relevance of monopsony models. There are, however, inherent difficulties in estimating the structural parameters of the labor supply function because one typically does not have good instruments for wages at the firm level.

The lack of good instruments for wages makes it attractive to utilize specific institutional settings and experiments with exogenous wage changes to identify the supply curve. Some professional labor markets with centrally determined wages may have this property. Even though individual establishments cannot influence their wage under centralized wage setting, workers should react to wage differentials in the same way as under firm-level wage setting. Since supply is determined by individual behavior, empirical regularities from specific labor markets might be generalized.

The present study exploits specific characteristics of the Norwegian teacher labor market. During a period of more than 40 years, wages were solely determined by central wage bargaining. With one exception, the only sources of wage variation across teachers have been teaching experience and the amount of formal education. The exception, in which teachers in individual schools with large teacher shortages in the past received about a 10% higher wage, is exploited in a school fixed-effects framework. To identify labor supply it is essential to observe the supply in addition to the wage variation. When wages are centrally determined, one would expect excess demand in some establishments and excess supply in others. It is possible to observe excess demand in our data because of a clear-cut rule regarding the appointment of teachers. In Norway, individuals without a teaching credential can be employed only when it is impossible to recruit certified teachers to vacant teaching positions. Employment of individuals without a teaching credential reflects excess demand, and teacher supply is then given by the number of certified teachers.

The next section presents relevant empirical literature. Section III explores the institutional setting more closely, and data and empirical specification are discussed in Sections IV and V. The empirical findings are reported in Section VI. Several sensitivity tests are undertaken, and I also test whether the supply elasticity is heterogeneous across localities. Section VII provides concluding comments.

II. Previous Empirical Studies on Establishment-Level Labor Supply

The first study using data at the establishment level in order to estimate the labor supply elasticity appears to be Sullivan (1989). He estimates the supply elasticity of nurses directed toward individual hospitals to be in the range 1.3–3.8, identifying demand shifts by assuming an exogenous level of hospital caseload. Staiger, Spetz, and Phibbs (2010, in this issue), however, argue that the caseload was endogenous in the period studied by Sullivan (1989). They use a legislative increase in the wage for nurses at Veterans Administration (VA) hospitals. VA hospitals paid nurses based on a national scale until 1991, when a system in which wages are based on local surveys was introduced. The size of the wage increase was related to local relative wages. The wage rose in about two-thirds of the VA hospitals, while at the remaining hospitals, which paid at or above the wage level in the surrounding hospitals, the wage did not change. Their estimate of the supply elasticity is around 0.1 and insignificantly different from zero, which implies large monopsony power.

When wages are not instrumented it is essential not only to have exogenous wages but also to distinguish between labor demand and labor supply. Staiger et al. (2010, in this issue) argue that the employment of nurses is likely to be "supply constrained" because of nearly continuous reports of shortages of nurses since World War II. However, this is aggregated information, and variations in, for example, working conditions and workload may yield demand-constrained employment at hospitals initially paying a lower wage than surrounding hospitals. Their empirical result is, in fact, close to some studies of the effect of minimum wages (see, e.g., Card and Krueger 2000; Neumark and Wascher 2000). As Boal and Ransom (1997) and Dickens, Machin, and Manning (1999) clearly show, the effect of an increase in the minimum wage on employment is nonlinear because more firms become "demand constrained" as the minimum wage increases. Dickens et al. (1999) estimate an average elasticity of the minimum wage with respect to UK employment of about 0.3, but the elasticity seems to be above 2 in the industries with the lowest minimum wage.

Some older papers attempt to estimate firm-level labor supply on regional data. Nelson (1973) argues that a population density measure can be used for identification of labor supply and reports large elasticities for most U.S. states. Boal (1995) argues that it is important to have data with a panel structure in order to control for unobserved factors such as remote location and other potential sources of compensating differentials. Using data from U.S. coal mining in the first decades of the twentieth century, Boal (1995) finds the labor supply elasticity to be in the range 1.9–6.8 in the short run and infinite in the long run.

The labor supply elasticity must be related to how sensitive worker turnover is to the wage level. Regarding teachers, Ransom and Sims (2008) use this insight and calculate the labor supply elasticity in Missouri to 3.65 under specific steady state conditions. Holzer, Katz, and Krueger (1991) find evidence that the number of applicants to job openings is responsive to the wage. Another stream of literature has interpreted the observed positive relationship between wages and employer size as an indication of inelastic labor supply (see, e.g., Green, Machin, and Manning 1996). However, Manning (2003) shows that the quantitative relationship between employment and wages depends crucially on whether wages are regressed on employment or the other way around and indicates that the reason is measurement error. He concludes that even though it is reasonable to interpret this relationship as evidence of upward-sloping supply curves, such regressions "are just not very informative" on the supply elasticity (Manning 2003, 95).

III. The Quasi-natural Experiment

Wage determination of teachers is highly centralized, with basically the same wage at each school in several European countries such as France, Germany, Italy, and the United Kingdom. In Norway, the wage of an individual teacher was solely determined by central wage bargaining up to and including the school year 2000–2001.[1] With only one exception, the wage varied across teachers only with respect to their education level and teaching experience. The exception was teachers at schools located in one of the three northernmost counties (out of a total of 19 counties in the country) with particular recruitment problems. The school districts had no influence on which schools were eligible for a higher wage, and the selection had no financial implications for them.

The eligibility rules for the wage premium were determined by the national parliament in 1993, 1995, and 1997. The background in 1993 was particular high teacher shortages in the three northernmost counties, and the argument was that the policy should reduce shortages and strengthen the school system. The usual understanding for higher shortages in the relevant counties is few cities and scattered population, long distances to tertiary education institutions, and rough climate.

I use the term *experimental school* for schools at which teachers received a wage premium at least once during the empirical period of the present study. Three different systems to reduce teacher shortages have existed.

[1] Some very limited local wage flexibility was introduced in 2001, and wage setting was further decentralized from 2004.

Table 1
Possible States of the Experimental Schools

School Year	School A	School B
1993–94	IN	IN
1994–95	IN	SIN
1995–96	IN	SIN
1996–97	NIN	NIN
1997–98	NIN	NIN
1998–99	IN	NIN
1999–2000	IN	IN
2000–2001	SIN	SIN

Note.—IN denotes that all certified teachers at the school receive a wage premium, SIN denotes that only the incumbent certified teachers at the school receive a wage premium, and NIN denotes that none of the teachers receive a wage premium.

In 1993–94 to 1995–96, teachers at schools with teacher shortages in the preceding school year exceeding 20% received a wage premium in nominal terms, which implies that the percentage wage premium varied with the initial wage. For schools with at least 30% shortages, the mean wage premium was about 10%. For schools with 20%–30% shortages, the rules differed regionally. In the southern part of the relevant counties the wage premium was the same as for schools with above 30% teacher shortages, while in the northern part the wage premium was only half as large. The schools with the lowest wage premium are located in the local governments with the lowest national income tax rate.

The system changed slightly for the school years 1996–97 and 1997–98. Then, only teachers at schools with teacher shortages in the preceding school year exceeding 30% were eligible for a higher wage. Thus, fewer schools had a wage premium in this period, as illustrated by School A in table 1. School A was included (IN) in the first system but not included (NIN) in the second system. Within the last system, continuing to the school year 2002–3, teachers at schools with teacher shortages during the four previous school years exceeding 20% on average received a wage premium, and the wage premium was equal across all included schools.[2]

Since increased wages are expected to raise labor supply, schools with teacher shortages marginally above the criterion for paying higher wages

[2] Up to 1997–98, the teachers could choose between the wage premium described and 1 year of study at a college with full pay. In the latter case, they had to commit themselves to work at the school for the following 5 years. Those choosing college obviously considered the benefit from a year of study to be larger than the wage premium. Thus, using the wage premium as the benefit of being employed at an IN school may underestimate the true benefit.

are expected to increase employment of certified teachers such that the school is not eligible for a higher wage the next school year. This is illustrated by School B in table 1. School B was an IN school in 1993–94 but not in the following years. In this case, the teachers at the school in 1993–94 who stayed there kept their wage premium as long as the system was in place, while new teachers at the school did not get the wage premium. In such semi-included (SIN) schools, only quits and not hires are expected to differ from other schools. When a new system commences and the school is still ineligible to be included in the system, none of the teachers receive the wage premium. Thus, if there are no serious teacher shortages at School B in 1994–95 to 1997–98, the school will be a SIN school in 1994–95 to 1995–96 and a NIN school in 1996–97 to 1997–98.

In summary, the experimental schools can be in three different states in a particular year; all teachers receive a wage premium, only the incumbent teachers receive a wage premium, or none of the teachers receive a wage premium. Because factors explaining why some schools have consistently low teacher supply can be hard to observe, it is an attractive feature of the data that several schools changed status during the empirical period. It is therefore possible to analyze the within-school variation in teacher supply and wages in models in which fixed school effects capture major systematic factors important for low supply.

To identify the elasticity of labor supply, the supply must be observed. The appointment rule of teachers in Norway is crucial in this regard, as discussed by Bonesrønning, Falch, and Strøm (2005). First, the teachers are linked to the schools and not to the school districts (local governments). The local governments cannot force teachers in permanent positions to leave a specific school for another school unless in very specific cases, such as the closing of schools or a huge drop in the number of students. Yearly about 10% of the teachers quit voluntarily for jobs in other schools, the bureaucracy, or the private sector (Falch and Strøm 2005). Teachers are recruited from the same places as well as directly from teacher colleges.

Second, the local governments have to appoint the best-qualified applicant to a vacant teacher position. If at least one certified teacher wants the vacant position, a certified teacher will fill the post.[3] According to the school law, a person without the required qualifications to be certified as

[3] Teaching certification requires either a teacher college degree or a university degree with a certain amount of course work in pedagogy. With one of these types of education, the teacher is certified to teach in all subjects from first to tenth grade, throughout the entire compulsory schooling period. Even though the school principals would like to diversify the teacher stock with respect to their "major" and teaching interests, that is not possible except when there are many applicants for vacant positions, which is only the case under excess demand as defined in this study.

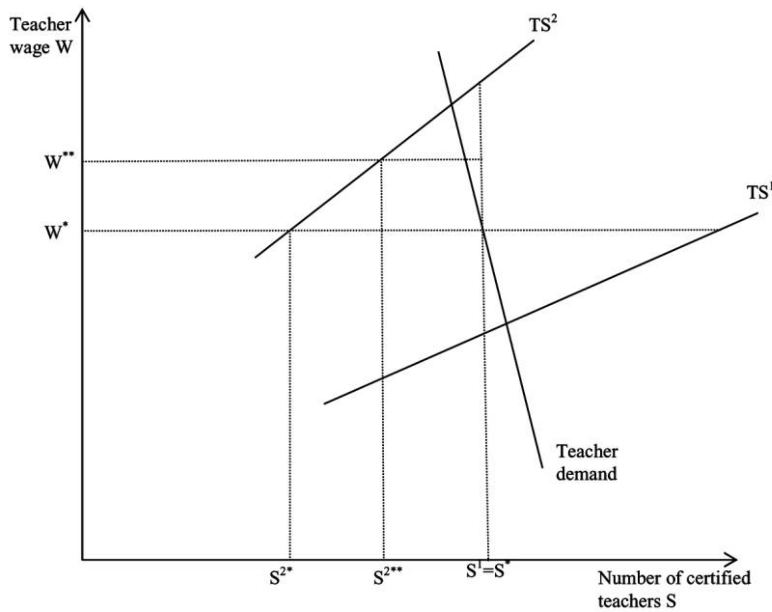

Fig. 1.—Identification of teacher supply

a teacher can only be employed if no certified teacher applies for the vacant teacher position. Furthermore, only certified teachers can be employed on permanent contracts. Noncertified teachers can only be hired on temporary contracts for up to one school year. In the subsequent year, the school must make the vacant position public again in order to get certified teachers to apply. According to national contracts, representatives of the teacher union must be informed prior to every hiring decision. In this way the union is able to closely monitor the schools' compliance with the law, which has been a cornerstone in the teacher trade union policy. The rule is crucial for the definition of the teaching profession. Thus, observed shortages of certified teachers in a particular year reflects the state of the teacher labor market in that particular year, and since one observes whether there is excess demand or not, one can also observe for which schools the actual adjustment is on the supply curve. In that case, the supply is given by the number of certified teachers.

The situation is illustrated in figure 1, which compares two schools with equal teacher demand, but different teacher supply. Given the centrally determined wage W^*, School 1 (described by the teacher supply function TS^1) has excess supply. The supply is greater than observed employment and cannot be empirically identified. The employment of certified teachers at School 2 is determined by the size of the teacher supply, S^{2*}. Since the school has to employ noncertified teachers, the supply is empirically identified by S^{2*}. If teachers at School 2 are eligible to receive a higher wage, illustrated by W^{**}, teacher shortages are reduced if the supply curve is upward sloping, as in the figure. Because the central government pays the wage premium, teacher shortages are reduced from $S^* - S^{2*}$ to $S^* - S^{2**}$. The excess demand can be eliminated if the wage rise is high or if the teacher supply is elastic.

The classification of IN schools was done by state representatives in the relevant counties. Up to 1997–98, the classification was based on information collected for this purpose at the start of the school year (September 1), and from 1998–99 the classification was based on the national data collected in October that are used in the present study. Because the criterion for a higher wage was previous teacher shortages, it has always been known in advance of the school year which schools will pay a wage premium. From 1998–99 it has been explicit in the instructions that the classification of IN schools for the following school year should be done before March 1. For new positions made public before this date, the school districts had to pay the wage premium without compensation from the central government. Otherwise, the wage premiums were remunerated by the central government based on lists over the individual teachers inspected by the state representatives in the relevant counties.

Since classification of IN schools is based on lagged information, there is no direct causal effect of current supply on whether there is a wage premium. However, if a school is able to reduce the number of certified teachers one year, the incumbent teachers may get a wage premium the next year. There are several reasons why this is not likely to be a serious problem in the present case. When I collected the data, the state representatives in the counties reported that they did not believe at all that the system was being manipulated. The appointment rules presented above are crucial in this regard. In addition, the criterion for IN schools varies over time and has been decided after the registration of teacher shortages. Both the changes in the system in 1995 and 1997 were decided in December, while the criterion is based on the shortages earlier in the fall. Thus, if there was any gaming of the system, the extent of the manipulation must be expected to vary over time. This issue will be considered in the empirical section of the paper by investigating the stability of the estimated supply elasticity.

IV. The Data

The experimental schools are located in three counties consisting of 89 local governments. All primary and lower secondary public sector schools

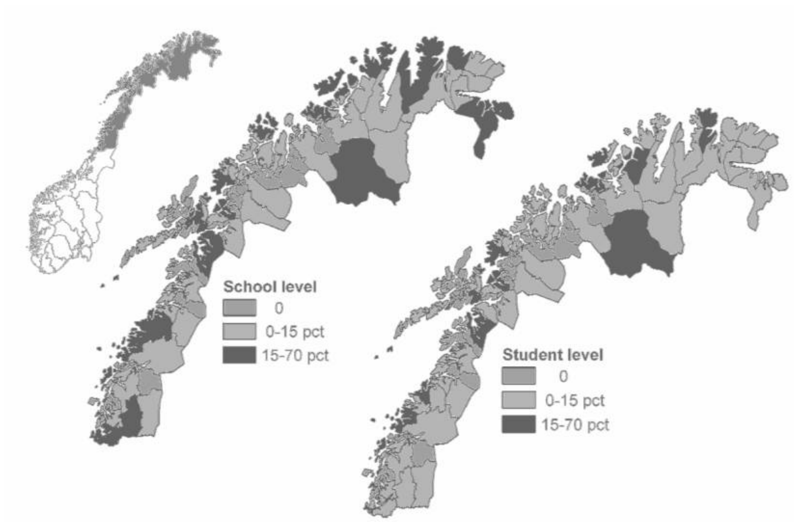

FIG. 2.—The extent of wage premiums in the different local governments in Norway

(first through tenth grade) in these counties are included in the data,[4] 565 schools followed over the 6 school years 1995–96 to 2000–2001.[5] Experimental schools exist in 73% of the local government districts and are therefore located in many different local labor markets. It is not straightforward to define the borders of local labor markets. However, in the relevant counties the population is to a large extent scattered, and relatively few workers reside in a local government district other than the district in which they work, which indicates that the local government districts are not too narrow measures of the local labor market.[6] For the relevant counties in the empirical period, the average percentage of the schools with a wage premium is shown in figure 2. Local governments in which more than 15% of the schools pay a wage premium are spread all over the region. The same is true for the local governments without any experimental school, except that all local governments in the northernmost county have an experimental school.

[4] Private schools exist only to a very small degree. In 1995, 0.5% of the students in the counties with experimental schools were enrolled at private schools.

[5] Employment data are not available before the school year 1995–96, and some wage flexibility in the hands of the local governments was introduced in the school year 2001–2.

[6] According to the 1990 census, 85% of the workers in the relevant counties worked in the local government district in which they resided. Based on the commuting statistics, Statistics Norway has classified 17 different labor markets areas in the relevant counties. There are experimental schools in all these areas, ranging from 4% to 50% of the schools.

Table 2
The Number of Experimental Schools in Different States

	1995–96	1996–97	1997–98	1998–99	1999–2000	2000–2001
IN	13	5	4	26	32	29
SIN	37	0	5	0	8	19
IN + SIN	50	5	9	26	40	48

Note.—IN denotes that all certified teachers at the school receive a wage premium, and SIN denotes that only the incumbent certified teachers at the school receive a wage premium.

Table 3
The Number of Experimental Schools in Years of Different States

	0 Years	1 Year	2 Years	3 Years	4 Years	5 Years	6 Years
IN	25	24	16	11	5	0	0
SIN	24	46	10	1	0	0	0
NIN	1	5	12	17	12	34	0
IN + SIN	0	36	13	17	11	3	1

Note.—IN denotes that all certified teachers at the school receive a wage premium, SIN denotes that only the incumbent certified teachers at the school receive a wage premium, and NIN denotes that none of the teachers receive a wage premium.

Experimental schools tend to be relatively small. The map on the right in figure 2 shows the average percentage of the students for which the teachers get a wage premium, which tends to be lower than the percentage of schools. In the main part of the paper, I include in the sample only schools with at least 20 students on average in the empirical period,[7] but I will also show that the results are insensitive to this choice.

Table 2 shows the number of schools with a wage premium in the different school years.[8] Few schools were eligible for a higher wage in the relatively restrictive system in 1996–97 and 1997–98, while about six times as many schools were included in the following years. The changes in the criteria to be included in the system imply that most schools only had a wage premium for a short period. Table 3 shows that out of the 81 experimental schools in the sample, teachers in 36 schools received a wage premium in only one year during the empirical period. Twenty-five schools have never been IN schools in the empirical period, but were all SIN schools in 1995–96. Only one school paid a wage premium in all 6 years of the empirical period.

Figure 3 illustrates how the classification of IN schools matches the data set available for the present study. Because the criterion is based on lagged teacher shortages, I cannot compare the lagged shortages criterion

[7] It is 112 schools that have fewer than 20 students on average, including 70 experimental schools.

[8] It is 18 schools with only one observation in the empirical period (new schools or closed schools) that are excluded from the analysis because they provide no information in fixed-effects models.

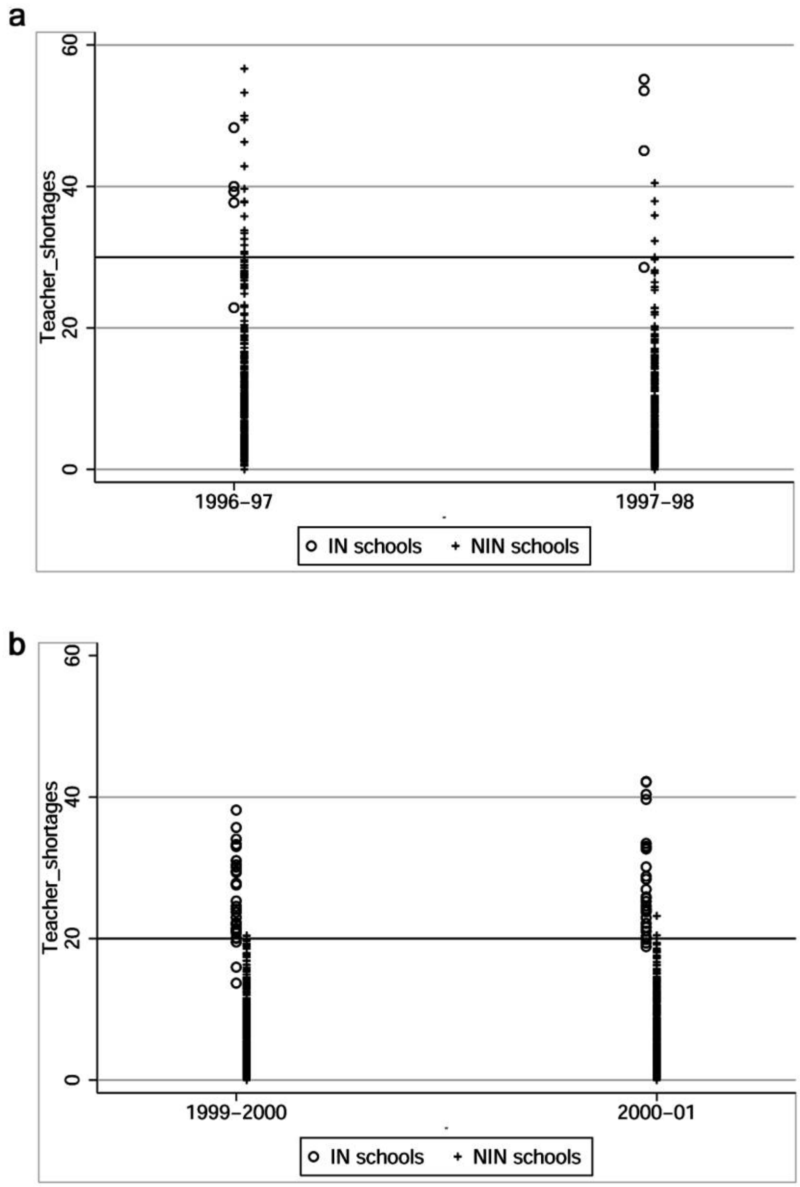

FIG. 3.—The criterion for being an IN school and observed teacher shortages. *a*, 1996–97 and 1997–98; *b*, 1999–2000 and 2000–2001. + shows observed teacher shortages in NIN schools, 0 shows observed teacher shortages in IN schools, and the horizontal line is the criterion to be an IN school.

Table 4
Descriptive Statistics, Mean Values (SD)

Sample	Nonexperimental Schools	Experimental Schools	IN and SIN Schools
Number of students	151.1	60.7	58.5
	(115.2)	(43.2)	(47.2)
Teacher supply in the cases of excess demand	14.7	7.4	7.4
	(9.5)	(4.7)	(5.1)
Population in school district	15,007	5,741	6,008
	(17,103)	(8,618)	(9,782)
IN	0	.229	.612
SIN	0	.145	.388
Percentage wage premium	0	.037	.098
		(.049)	(.018)
Observations	2,066	476	178
Percentage of noncensored observations	61.8	84.9	78.7

with the actual classification in the school year 1995–96, and I cannot calculate the 4-year average for 1998–99. For the last school years, the classification of IN schools is, with minor exceptions, in accordance with the data. The exceptions may be due to the fact that the state representatives should take errors in lagged data into account in their classification. For the first school years in the sample, in particular 1996–97, there are clearly divergences between the classification of IN schools and the present data. Note that in these years the classification is based on purpose-specific data collected 1–2 months before the present data. There is a possibility that this allows manipulation of the system by reporting low numbers of certified teachers. However, if anything, the purpose-specific data seem to underreport teacher shortages. There are several schools that seem to be eligible for the wage premium according to the present data, but which did not actually become IN schools. However, it is a concern that the divergence may reflect measurement errors in the present data. For this reason, and because the possibilities of manipulating the system may have changed over time, I will present estimation results for subperiods below.

Table 4 presents descriptive statistics. The experimental schools are relatively small. While the mean size of the experimental schools is 60 students, the mean size of the other schools in the relevant counties is 150 students. This is also reflected in lower teacher supply measured in full-time equivalent employment of certified teachers in the cases in which supply is identified. The supply is identified in 62% of the nonexperimental schools observations and 85% of the experimental schools observations.

In addition to the occurrence of a wage premium, the mean wage level varies across schools because mean teacher experience and amount of formal education vary. The percentage wage premium is calculated based on information of the mean wage level of certified teachers at each school. The average wage premium in the experimental schools is 3.7%. However, because only 38% of the observations of experimental schools are IN or SIN schools, the average wage premium for those schools is 9.8% (about 2,150 NOK per month, or about US$350). The wage premium ranges from 4.6% to 11.9%, with the lowest mean in 1995–96 (7.8%) and the highest mean in 1998–99 (11.3%).

Figure 4*a* plots the relationship between changes in the wage premium and changes in teacher supply. It is evident that supply tends to rise when a wage premium is introduced, while supply tends to decline when a wage premium is removed. Schools that continue to pay the wage premium have some small changes in the percentage wage premium, mainly because the premium is constant in nominal terms over some years.[9] Figure 4*b* presents the distributions of the changes in teacher supply when the wage premium is introduced and removed. The distribution of the former is clearly to the right of the latter.[10]

V. Empirical Specification

Central arguments for imperfect competition in the labor market are dynamic in nature. If a firm cuts the wage, all workers are not expected to leave the firm immediately, but the supply reduces over time as the quit rate increases and recruitment becomes harder. Frictions in the market yield sluggishness, for example, because there is a limited number of job offers at a specific point in time, as discussed, for example, in Mortensen (1986) and Burdett and Mortensen (1998).

Consider the following identity for the true supply S_{it} toward firm i at time t.

$$S_{it} = (1 - q_{it})N_{it-1} + H_{it} + E^S_{it} = N_{it} + E^S_{it} = D_{it} - E^D_{it}, \qquad (1)$$

where N_{it-1} is lagged employment, q_{it} is the quit rate, H_{it} is the number of hires since last period, E^S_{it} is excess supply, D_{it} is demand, and E^D_{it} is excess demand. With excess demand $E^D_{it} > 0$, current employment $N_{it} = (1 - q_{it})N_{it-1} + H_{it}$ is "supply constrained" and equal to the supply. The supply is observable since $E^S_{it} = 0$. With excess supply $E^S_{it} > 0$, employ-

[9] Changes in the composition of teachers will also affect the average wage premium. All observations with a decline of about 5% in the wage premium are from the school year 1996–97. Some schools paid only half of the ordinary wage premium in 1995–96 (see Sec. III).

[10] The average increases in log teacher supply when a wage premium is introduced and removed are 0.15 and −0.08, respectively. For experimental schools that do not have a wage premium 2 years in a row, the corresponding number is −0.03, while for schools that have a wage premium 2 years in a row the number is 0.03.

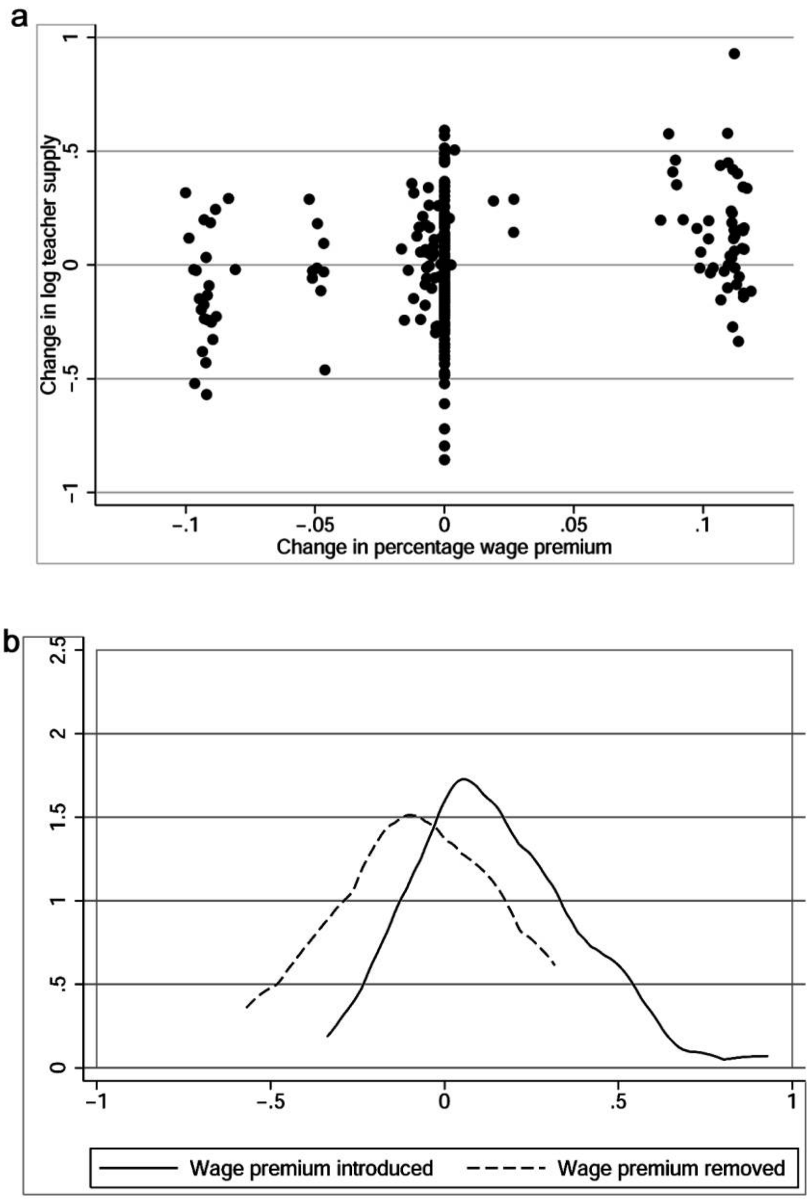

FIG. 4.—Teacher supply and wage premium, experimental schools. *a*, Changes in supply and wage premium; *b*, density of the change in log teacher supply.

ment is "demand constrained," and the supply is unobservable without information about job queues.

A large literature clearly indicates that quit rates and recruitment depend on the wage level W as well as other characteristics X of the enterprises (see Manning 2003).[11] On log-linear form, the supply can be written

$$s_{it} = \delta_i + \delta_t + \alpha n_{it-1} + \beta w_{it} + \gamma_x x_{it} + \eta^s_{it}, \quad (2)$$

where small letters denote logarithmic values and η^s is assumed to be a normally distributed independent and identically distributed error term. School-specific effects δ_i control for time-invariant school characteristics, such as, for example, location characteristics; time-specific effects δ_t capture macroeconomic factors, such as the outcome of the central wage bargaining; and the inclusion of lagged employment follows from equation (1).[12]

There are several potential problems in estimating equation (2) with ordinary least squares. First, it is not clear from theory which variables are included in the vector X. Teacher demand factors are theoretically not elements in X since the supply is aggregated teacher decisions. I will include population size in the local government in the baseline model formulation and extend the model with additional variables in sensitively analyses.[13] Given the endogeneity problems with lagged dependent variable, the baseline model I present is static. In this case the instantaneous supply elasticity is expected to be downward biased when $\alpha > 0$ since the wage and lagged employment are negatively correlated by the experiment exploited in this study.[14] The setup of the experiment is nevertheless temporal in nature, so the wage effect estimated can only be interpreted as a short-run elasticity. Last, selection issues may be relevant in the present case because teacher supply is observed only under excess demand. The fixed school effects capture some selection effects, partly because the

[11] For teachers, see, e.g., Dolton and Mavromaras (1994) and Falch and Strøm (2005).

[12] The supply directed toward an enterprise consists of the sum of the supply of individual workers. Using a random utility function approach and a specific distribution of the individual utility functions, a log-linear static upward-sloping supply curve with fixed effects is derived in Falch (2003). In that case the supply curve is upward sloping because the utility of working in different enterprises differs for some unobserved random reasons, for example, because of heterogeneous moving costs.

[13] One might prefer to include the population in the school catchment area in the model. This measure is, however, not available. In a sensitivity analysis I include the number of students at the school as a size variable. But since the borders of the school catchment area sometimes changes as a response to crowded schools, I cannot completely rule out the case that the borders also have reacted on teacher supply.

[14] The correlation coefficient in the present data is -0.25.

Table 5
Estimated Labor Supply Elasticity (Dependent Variable Is Log Teacher Supply)

	Within-School Estimator				First-Differenced Estimator			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wage premium at school (IN + SIN school)	.108 (.026)	.122 (.033)	. . .	. . .	.142 (.027)	.147 (.038)	. . .	. . .
IN school	. . .	−.023 (.032)	. . .	. . .	. . .	−.007 (.045)	. . .	. . .
Percentage wage premium	. . .	. . .	1.024 (.271)	1.449 (.387)	. . .	. . .	1.436 (.277)	1.408 (.327)
Log(Population)	1.300 (.356)	1.283 (.357)	1.337 (.356)	.838 (.987)	1.222 (.460)	1.220 (.462)	1.248 (.461)	1.674 (.940)
School-specific trend	No	No	No	Yes	No	No	No	Yes
Standard error of equation	.1746	.1747	.1749	.1482	.2008	.2009	.2008	.2186
Observations	1,680	1,680	1,680	1,680	1,091	1,091	1,091	1,091

Note.—Year-specific effects included. Standard errors corrected for within-school clustering in parentheses.

supply is observed more often in experimental schools than in other schools. However, there is also variation within schools that may bias naïve models that do not take censoring into account. Table 4 shows that in experimental schools the supply is less often observed when there is a wage premium than when there is no wage premium, working in the direction of underestimating the supply elasticity when selection is not taken into account.

VI. Estimation Results

The baseline model is the static version of equation (2) assuming linearity and random conditional selection. I show below that the conditional selection seems to be random and that the dynamic adjustment seems not particularly sluggish.

The baseline model can be estimated by the within-school estimator or the first-differenced estimator. Although both methods yield asymptotically consistent estimates for a given time dimension under standard assumptions, the former is exactly equal to equation (2) while the latter is not. Referring to School A in table 1, the within estimator identifies the effect of wages by comparing average supply in 1993–96 and 1998–2000 (IN school) with average supply in 1997–99 (NIN school). The first-differenced estimator identifies the effect on the switches in 1996–97 and 1998–99 and is more dynamic in this sense. On the other hand, there are "gaps" in the data of observed supply since there are observations of seemingly excess supply, and fewer observations are available for the first-differenced estimator since only observations with excess demand in two consecutive years can be utilized.

Table 5 includes the main results. Columns 1–4 present results for the within-school estimator while columns 5–8 present results for the first-differenced estimator. The main source of variation identifying the labor supply elasticity is whether there is a wage premium or not. The dummy variable for wage premium (IN + SIN school) is significant at the 1% level in columns 1 and 5, with the largest effect for the first-differenced estimator. The results imply that, at an average percentage wage premium, the elasticity of teacher supply is 1.10–1.45.[15]

Columns 2 and 6 in table 5 investigate whether the response to the wage premium differs between IN and SIN schools. Because only incumbent teachers receive a wage premium in SIN schools, it follows from equation (1) that the response should be largest in IN schools. The results show that the difference between IN and SIN schools is small and clearly insignificant.

The models in columns 3 and 7 in table 5 estimate the supply elasticity directly by replacing the dummy variable with the average percentage wage premium in the model. The estimated elasticity is again larger for the first-differenced estimator (1.44) than for the within estimator (1.02). Columns 4 and 8 indicate that the within estimator is biased downward because of missing variables at the school level. Including school-specific time trends, the elasticity increases to 1.43 for the within estimator, while the first-differenced estimator is hardly affected.

The elasticity of school district population size is about 1.2 and significant but clearly not significantly different from unity. A rising population implies more teachers in the district.

A. Sensitivity Analyses

Table 6 considers several different alternatives to the baseline model formulation, both in terms of new variables, sample, and estimation approach. Because the first-differenced estimator seems more robust across model specifications than the within-school estimator in table 5, results for the within estimator are not reported in table 6.[16]

The wage premium is correlated with school size as shown in table 4. The model in row 1 in table 6 includes the number of students in the model, but the estimated supply elasticity changes only marginally even

[15] Because the nominal wage premium is independent of the initial wage, the percentage wage premium differs across teachers. For newly educated teachers with the lowest qualifications (bachelor), the percentage wage premium is largest. Using the centrally decided wage frame, the models in cols. 1 and 5 imply that the supply elasticity is in the range 0.76–2.14.

[16] The results for the within-school estimator are qualitatively similar to the first-differenced estimator and are available on request.

Table 6
Sensitivity Analyses: Dependent Variable Is Log Teacher Supply

Specification Changes Compared to the Model in Column 7 in Table 5				
	Equation	Sample	Observa- tions	Estimated Elasticity (SE)
(1)	Log(Number of students) included	None	1,091	1.397 (.282)
(2)	The effect of log(Number of students) restricted to unity	None	1,091	1.368 (.294)
(3)	Including n_{it}, restricting the effect to unity	None	1,091	1.352 (.225)
(4)	The share of students from ethnic minorities and the share of students with special needs included	None	1,082	1.427 (.278)
(5)	Log(Teacher education hours) and log(Number of students) included at squared, cubic, and differenced form	None	1,085	1.368 (.280)
(6)	None	Only observations with at least one full-time equivalent noncertified teacher are regarded as noncensored	641	1.694 (.350)
(7)	Using $(s_{it} - n_{it-1})$ as dependent variable instead of $(s_{it} - s_{it-1})$	Increasing because more observations of the dependent variable	1,413	1.515 (.283)
(8)	None	Excluding schools for which IN classification is not in accordance with the present data at least 1 year	974	1.341 (.384)
(9)	None	Only the period 1999–2000 to 2000–2001	472	1.373 (.518)
(10)	OLS at level without fixed effects, including the variables mean teacher shortages and mean number of students during the empirical period	Increasing because more observations of the dependent variable	1,680	1.081 (.325)
(11)	OLS at level without fixed effects, including the variable mean number of students during the empirical period	Only schools with shortages $\pm 5\%$ points around the criterion for wage premium, 1999–2000 to 2000–2001	64	1.476 (.629)
(12)	None	All Norwegian schools	5,849	1.308 (.273)
(13)	None	Only experimental schools	298	1.817 (.304)
(14)	None	Including schools with fewer than 20 students on average	1,297	1.565 (.302)
(15)	None	Schools with average number of students <50	490	1.948 (.404)
(16)	None	Schools with average number of students ≥ 50	807	1.042 (.337)

Note.—All models estimated by the first-differenced estimator. Standard errors corrected for within-school clustering in parentheses.

though the effect of students (not shown) is strong.[17] The model in row 2 restricts the elasticity of the number of students to unity and thus uses the certified teacher-student ratio as the dependent variable. Row 3 normalizes directly on teacher demand, using the log of the share of certified teachers ($s_{it} - n_{it}$) as the dependent variable. None of the normalizations has quantitatively important effects on the estimated supply elasticity as expected since the experiment was designed such that the wage premium should not influence teacher demand.

Rows 4 and 5 in table 6 introduce some new variables into the model. Bonesrønning et al. (2005) show that both the share of ethnic minority students and the share of students with special needs influence teacher supply. They also show that the number of teacher education hours per student (a measure close to the teacher-student ratio), which may be important for teacher effort, has a strong effect on teacher supply. However, including measures of the student body composition or teacher density in the model has only a marginal effect on the estimated supply elasticity.

One reason why the demand side may affect the dependent variable is that noncertified teachers are utilized to a small degree in some schools. I have assumed observable teacher supply in such schools, but noise might perhaps give observations of noncertified teachers employed in part-time positions in some cases even without excess demand. Row 6 in table 6 assumes that the supply is observed only when noncertified teachers account for at least one full-time equivalent teacher. This change in sample increases the estimated supply elasticity to 1.69, which is about one standard error larger than in the baseline model 7 in table 5.

The present panel data have an unconventional structure since supply is observed in some years but not in other years. The existence of "gaps" in the data is of specific importance because the first-differenced estimator can only utilize information when there is excess demand in two consecutive years. To shed some light on the importance of gaps in the data, row 7 in table 6 increases the sample size by using the change in employment of certified teachers given observed supply in year t, that is ($s_{it} - n_{it-1}$), as the dependent variable. Then the estimated supply elasticity increases slightly to 1.51.

Another feature of the data is that the classification of IN schools is not always in accordance with observed teacher shortages in the present data, in particular for the years prior to 1998–99 (see fig. 3). In row 8 in table 6, schools for which the IN school classification is not in accordance

[17] The effect of the log number of students is about 0.6 with a standard error of 0.06. The result implies, however, that when the number of students increases by 10%, teacher supply *per student* is reduced by 4%. Even though there are some economies of scale, the probability of excess demand increases as schools expand.

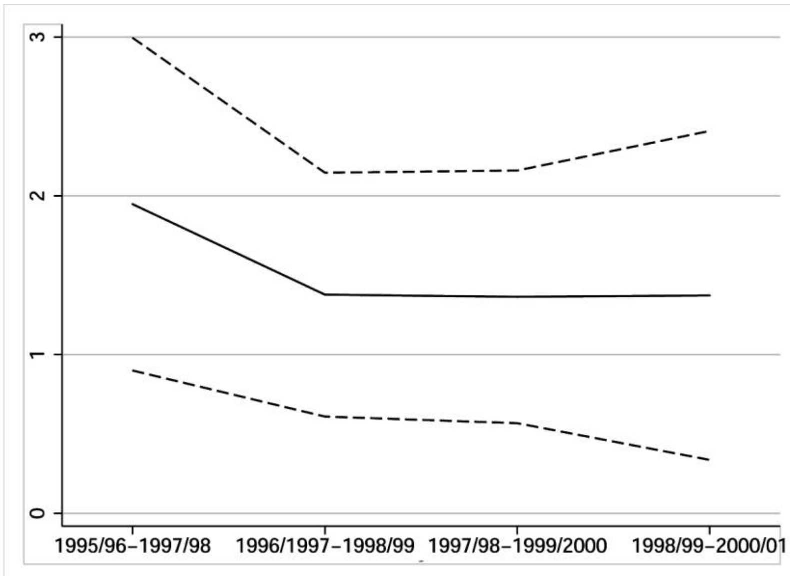

FIG. 5.—Time-varying teacher supply elasticities ±2 standard errors

with the present data in at least one year are excluded from the sample. This reduces the sample by 10% but does not affect the estimated supply elasticity. A related concern is that measurement error in the dependent variable may be an issue early in the empirical period. In addition, it was in principle easier to manipulate the system in the first years of the empirical period when only teacher shortages lagged one year was the criterion for wage premium. In row 9 the supply elasticity is only identified on the last experimental school system in place, reducing the sample by over 50%. In this sample, the within-school variation in the wage premium is markedly reduced, and the effect of the wage is identified only by schools introducing a wage premium. Nevertheless, the estimated supply elasticity does not change much and is significant at the 5% level.[18] Figure 5 presents estimates for a moving sample of 2 years, and they are all in the range 1.36–1.95. These results clearly indicate that gaming of the system is not a relevant issue since this possibility changed markedly during the empirical period.

In fixed-effects models, lagged residuals of the equation of interest are by construction in the residual of the estimated model. For example, for the first-differenced estimator of equation (2), the residual is $\eta^S_{it} - \eta^S_{it-1}$.

[18] The effect of wages seems to be symmetric. When the effects of introducing and removing a wage premium are allowed to differ in the baseline model formulation, the elasticity of the former is 1.41 and the elasticity of the latter is 1.48.

Thus, the effect of the wage premium might be biased upward because eligibility is due to lagged teacher shortages. In particular, mean reversion where a negative shock in supply that makes the school eligible for the wage premium is followed by a positive shock might be particularly problematic. However, the fact that the estimated supply elasticity for the last system in place where the wage premium criterion is related to average teacher shortages the last 4 years is similar to the estimates for the previous systems indicates that this is not a major problem for the present analysis. In order to do a more thorough investigation, I have estimated several models excluding the fixed effects.

A simple ordinary least squares (OLS) model at levels on the baseline sample, including the average number of students during the empirical period for normalization purposes, yields an insignificant supply elasticity of 0.51. This is likely to be a downward-biased estimate because the wage premium variable is likely to pick up school unattractiveness. We would like to have a continuous variable of the general attractiveness of the school, but such a variable is not easily constructed. In row 10 in table 6 the average share of noncertified teachers at the individual school during the empirical period is included in the OLS model since unattractive schools have high teacher shortages at mean. Then the estimated elasticity is 1.08.

An alternative is to use a discontinuity approach, comparing school observations barely eligible for the wage premium with school observations barely ineligible. This is challenging because I have a noisy measure of eligibility early in the empirical period. Row 11 in table 6 presents results for the years 1999–2000 and 2000–2001, including in the sample only school observations for which teacher shortages are 5 percentage points above or below the criterion for the wage premium.[19] In this case the estimated supply elasticity is 1.48 and significant at the 5% level, although more imprecisely estimated than in models with more observations.[20]

For the same time period, it is possible to construct a wage premium variable for the counties not affected by the experiment as if they did. Using the discontinuity approach described above, the estimated effect is clearly insignificant as expected and has a negative sign.[21] An alternative

[19] The sample includes 35 and 29 observations with and without a wage premium, respectively.

[20] The estimated elasticity is not particularly sensitive to the choice of bandwidth. If observations with teacher shortages ± 3 (7) percentage points around the criterion are included, that is 44 (107) observations, the elasticity is 1.55 (1.04), and it is significant at the 10% level.

[21] In this "falsification" test, there are 31 and 101 observations with and without the constructed wage premium, respectively. For the alternative bandwidths as reported in n. 20, the estimated effects are also negative.

procedure in order to maximize the potential for mean reversion effects and the number of observations is to create a dummy variable for teacher shortages above 20% in the preceding school year for the counties without wage premium, that is, the rest of Norway. For the within-school estimator, the effect of the dummy variable is close to zero and insignificant. For the first-differenced estimator the effect is positive and significant, which indicates a spurious wage effect, but the point estimate is below 50% of the estimate in table 5.[22]

The last rows of table 6 make other changes to the sample. The estimated elasticity is slightly lower when all Norwegian schools are included in the sample (elasticity of 1.30) despite the fact that it is always variation within schools that identifies the supply elasticity.[23] When only experimental schools are included, the estimated elasticity is higher (elasticity of 1.82). Rows 14–16 investigate the sensitivity to school size. Including schools with fewer than 20 students into the analysis increases the estimated supply elasticity, indicating that supply is more elastic in small schools than in large schools. The elasticity is clearly higher at schools with fewer than 50 students than at schools with more than 50 students, although the point estimates are not significantly different at the 5% level.[24]

B. Dynamics and Censoring

This section presents models with an explicit dynamic structure of teacher supply. In addition, the robustness of the results with regard to the fact that supply is observed only in about 80% of the observations of the experimental schools is investigated.

Column 1 in table 7 extends the baseline model in column 3 in table 5 with lagged employment in accordance with equation (2). In this model the supply elasticity is to a larger degree identified by the time variation

[22] In principle it would be interesting to test for spurious effects in the experimental region in periods without the experiment. Unfortunately, data prior to the experiment do not exist, and the experiment ended after some local flexibility in the wage setting was introduced.

[23] The time-specific effects clearly differ in the national sample compared to the sample in table 5, which has an impact on the estimated supply elasticity probably because the number of IN + SIN schools varies across years.

[24] It appears that the point estimate difference between schools with fewer and more than 50 students is partly a result of different year-specific effects, although they are not significantly different. Restricting the year effects to be equal across school size, the estimated supply elasticity is 1.76 for small schools and 1.15 for large schools (compared with 1.95 and 1.04, respectively, in table 6), and not statistically different at the 20% level.

in the wage premium. The effect of lagged employment is only 0.17,[25] indicating relatively rapid supply adjustment.[26] The estimated supply elasticity does not change. This is, however, partly due to the smaller sample. The supply elasticity is equal to 0.90 when estimating the baseline model on the same sample as in column 1 in table 7, which indicates that the instantaneous elasticity is slightly downward biased in the baseline model as expected.[27]

I will use three approaches to investigate whether selection is an important issue in the present case. First, following Wooldridge (2002, chap. 17.7), I test whether the lagged selection variable is significant when included in the model. Under the null hypothesis of no selection bias the residual at time t is uncorrelated with the selection variable for all years. The second approach builds on the fact that experimental schools have recruitment problems in general. In years without excess demand, it is likely that supply is close to demand. Thus, I simply use employment as the dependent variable, including all observations in the data in the regressions. The last approach models the selection process and estimates a type 2 tobit model (Amemiya 1984). Supply is observed under excess demand, and excess demand is determined by both supply and demand. Since the censoring point varies across observations, the traditional type 1 tobit model cannot be applied. However, selection, or the censoring point, can be identified utilizing that the demand behavior differs from the determination of supply.

[25] The effect of the lagged dependent variable is biased downward in OLS models (the Nickell bias), which does not seem important for the instantaneous effect of the wage premium in the present case. In a model without fixed effects, where the effect of lagged employment will be biased upward since the variable captures some time-invariant elements, the effect is 0.66. Restricting the effect of lagged employment to 0.5 and estimating a fixed effects model, the supply elasticity is 1.03.

[26] In the specific labor market studied here, it is relatively easy to get information on vacant positions because they are published in teacher magazines. Little uncertainty about job offers may explain the small extent of sluggishness. In studies of labor demand, it is common to find a larger effect of lagged employment. For example, Arellano and Bond (1991) find an effect of lagged employment of 0.5, and for the local public sector Bergström, Dahlberg, and Mörk (2004) find an effect of lagged employment of 0.4 for Swedish municipalities.

[27] Lagged employment that is included in table 7 is not exactly equal to the lagged dependent variable. However, employment is exactly equal to supply whenever supply is observed in the data. Using lagged supply instead of lagged employment in the model would thus simply reduce the number of observations. Replacing lagged employment with lagged supply in col. 1 in table 7, which is equivalent to dropping the observations in which lagged supply is not observed, the number of observations declines by 23% and the estimated elasticity is reduced by 21%. The change in the estimated elasticity is smaller in the other model specifications in table 7.

Table 7
Discontinuity, Dynamics, and Selection Models

	Dependent Variable, Log Of							
	Supply			Employment			Supply	
	Method: Within School			Method: Within School			Method: Tobit 2	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Percentage wage premium	1.032	1.027	1.052	1.020	1.194	1.632	1.025	1.069
	(.312)	(.272)	(.315)	(.294)	(.283)	(.261)	(.282)	(.306)
Log(Population)	.856	1.327	.814	1.313	1.106	. . .	−.030	−.008
	(.332)	(.356)	(.331)	(.271)	(.243)		(.025)	(.020)
Log(Employment, lagged)	.170	. . .	.179	. . .	.218	.228	. . .	.249
	(.038)		(.038)		(.035)	(.025)		(.047)
Lagged indicator for observed supply (excess demand)	. . .	.010	.025	. . .	. . .	. . .	. . .	. . .
		(.014)	(.014)					
Log(Number of students)	. . .	. . .	. . .	. . .	. . .	. . .	.670	.533
							(.061)	(.062)
Year − Local government interaction	No	No	No	No	No	Yes	No	No
Sample	Excess demand			All	All	All	All	All
Standard error of equation	.1638	.1749	.1636	.1743	.1583	.1525	.1538	.1403
Observations	1,413	1,680	1,413	2,542	2,106	2,106	2,542	2,106
Noncensored observations	1,413	1,680	1,413	2,542	2,106	2,106	1,680	1,413
F-test, joint significance of the instruments in the probit selection equation, *p*-value	. . .	. . .	. . .	. . .	. . .	. . .	.082	.221
Wald test, independence of the selection and supply equations, *p*-value	. . .	. . .	. . .	. . .	. . .	. . .	.600	.383

NOTE.—Year- and school-specific effects included. Standard errors corrected for within-school clustering in parentheses.

In Norway, compulsory education is the responsibility of multipurpose local governments providing additional local public services such as care of the elderly, day care, and different local utilities. They have discretion to set the number of teachers above what follows from the national maximum class size rule combined with the number of students. This is particularly relevant in the local governments in the included counties, which all are relatively rich.[28] Thus, variables at the local government level that

[28] For local governments the possibilities of influencing their income level are limited. All local governments in Norway have the same income tax rate in a system of income revenue sharing. The second most important revenue is lump sum grants from the central government. Lump sum grants are distributed based on objective criteria, among other things promoting an active regional policy. A lump sum grant is a particular high grant to the counties affected by the wage premium policy. During the empirical period of the present study the weight given to different criteria changed. I use the sum of income tax revenue and lump sum grants as exogenous local government income, as is traditionally done in Norwegian local public finance studies (see, e.g., Borge and Rattsø 1995).

influence teacher demand might be valid instruments to identify the selection. The instruments included in the model below are local government exogenous income per capita and the shares of preschool children and elderly in the population. An extensive literature shows that the demand for school expenditures depends on local income. In addition, different studies have found that the age composition of the population is important for the allocation of resources in multipurpose authorities (see, e.g., Borge and Rattsø 1995; Poterba 1997). The number of students is obviously also a demand factor. But because this variable has a strong effect on supply, probably because large schools have large catchment areas containing both a large number of students and teachers, it is included both in the supply and selection equation.

Note that the last approach has at least two shortcomings. Because the model includes school fixed effects, limited variation over time may make the selection instruments weak. Second, when estimating the selection equation, a nonlinear model must be used, while the incidental parameter problem derived by Neyman and Scott (1948) implies that the maximum likelihood estimator of nonlinear fixed-effects models in general is inconsistent. Greene (2004) argues, however, that the latter problem is of minor importance in tobit models.

Columns 2 and 3 in table 7 include a dummy variable for whether the supply is observed the previous year. The effect of the lagged selection variable is insignificant at the 5% level, which implies that the null of no selection bias cannot be rejected.[29] Columns 4 and 5 show that replacing supply by employment as the dependent variable, which increases the number of observations by about 50%, hardly affects the estimated coefficients. This also indicates that selection is not an important issue in the present case. To take account of all potential teacher demand factors at the decision-making level, column 6 includes interactions between year-specific effects and local government-specific effects. When all variation at the local government level is differenced out of the model, the estimated elasticity increases by almost 40% (1.5 standard errors). If this is because of demand factors, we would not expect to get the same change in the point estimate when using teacher supply as the dependent variable. However, in this case the estimated elasticity increases by the same amount (not shown). There seem to be some factors at the local government level that influence the supply elasticity.

The last two columns in table 7 show that the estimated supply elasticity

[29] Since the classification of IN schools was based on previous teacher shortages, including the level of shortages the previous school year in the model might be another way to investigate the importance of selection and mean reversion. When lagged teacher shortages are included in the models in cols. 1–3 in table 7, the estimated supply elasticity is reduced by only 7%–9%.

is similar for the full maximum likelihood fixed-effects tobit as for the baseline model. Notably, one cannot reject that the correlation between the errors in the selection model and the supply equation is equal to zero. Note, however, that the instruments of the selection equation are weak.[30] Strong instruments for selection in a fixed-effects approach on a relatively short panel are in general hard to come by, which, however, also is consistent with the case of random selection conditional on fixed effects.[31] Using the traditional tobit model (type 1 tobit) without instruments in the present case requires some kind of normalization. The dependent variable in row 3 in table 6 is $(s_{it} - n_{it})$, truncated at supply/demand = 1. Using the traditional tobit model extended with fixed effects on this specification, utilizing the whole sample, yields a supply elasticity of 1.42. This is almost identical to the results for the restricted sample in table 6.[32]

C. Variation in the Supply Elasticity across School Locations

Table 8 presents some crude tests on whether the teacher supply elasticity varies across local labor markets. I use local government boundaries to define the local labor markets. To what extent do increased wages result in new hires being recruited from other schools in the same local labor market? A simple examination is to estimate the effect of the mean wage premium at the local government level, weighted by the number of students. A positive effect of the mean percentage wage premium implies that the number of teachers in the local government increases. The estimated elasticity of the mean wage premium is close to unity for the first-differenced estimator and significant at the 5% level, as shown in column 1 in table 8. Schools paying a wage premium seems to a large extent to attract teachers outside local schools.

The effect of the mean wage premium captures both the increased supply at schools with a wage premium and the decreased supply at neighboring schools without a wage premium. The model in column 2

[30] Two of the three instruments have the expected sign in the selection equation. If the instruments are included in the equation of interest, they are jointly insignificant at the 5% level. The selection equation is available on request.

[31] One way to avoid the incidental parameter problem in the tobit model is to specify the selection equation without school fixed effects. This is done in an early draft of this paper (Falch 2003), which modeled the fixed effects. The estimated elasticity in Falch (2003) is, however, very close to the results for the tobit models in table 7. The main difference is that the instruments have a stronger effect in the selection equation and that independence of the selection and supply equations is rejected.

[32] In this model demand is in the dependent variable. Then the model should include determinants of demand even though demand is exogenous to supply. Including the same demand variables as in the tobit models in table 7, however, does not change the estimate.

Table 8
Nonconstant Labor Supply Elasticity (Dependent Variable Is Log Teacher Supply)

	(1)	(2)	(3)	(4)
Percentage wage premium	. . .	1.654	−.265	. . .
		(.332)	(2.385)	
Weighted mean percentage wage premium in the local government district	1.123	−.756	. . .	. . .
	(.511)	(.547)		
(Percentage wage premium) × log(Population)	. . .	. . .	.211	.178
			(.297)	(.034)
Log(Population)	1.127	1.179	1.204	1.212
	(.464)	(.459)	(.461)	(.457)
Standard error of equation	.2036	.2007	.2008	.2007
Observations	1,091	1,091	1,091	1,091

NOTE.—All models estimated by the first-differenced estimator. Year-specific effects included. Standard errors corrected for within-school clustering in parentheses.

in table 8 disentangles these effects by including the wage premium at the school level in the model. To interpret the results, consider first a local government where all students are enrolled at schools with a wage premium of 10%. The supply increases by 8.98%, which is significant at the 5% level. Consider next the case in which half of the students in the local government are enrolled at schools in which the teachers receive the wage premium. Then the model implies that the supply decreases by 3.78% at schools without a wage premium and increases by 12.76% at schools paying a wage premium, which implies that the teacher supply elasticity at the local labor market level is 8.98. The latter estimate is independent of the number of schools paying a wage premium and slightly lower than in column 1 in table 8. The estimate indicates that about 40% of the improved supply is at the sacrifice of other schools in the same local government.

The classical example of labor monopsony is an isolated firm in a remote, sparsely populated area. Incumbent workers have high mobility costs and can accept a relatively low wage without quitting, and the firm must pay the mobility costs of new hires. Remote firms are faced with less elastic labor supply than other firms. Column 3 in table 8 reports the results when the baseline model is extended with an interaction term between the percentage wage premium and the log of the population size in the local government. The interaction term is positive as expected, although insignificant at conventional levels. The effect of the wage premium at level becomes negative. Excluding this variable, column 4 shows that the effect of the interaction term is clearly significant. The results imply that, using the sample variation in population size, the supply elasticity varies from 1.1 to 2.0 with a mean of about 1.6. Small local governments seem to have greater problems with recruitment of new teachers than large local governments, which, however, implies that they have larger potential monopsony power.

VII. Conclusion

The direct evidence on whether firms have monopsonistic wage-setting power is whether the labor supply faced by the individual establishments is not highly elastic with respect to the wage. Given the problem of identifying good instruments for wages, this study has argued that utilization of experiments and institutional features are fruitful ways to establish evidence on the elasticity of labor supply directed toward individual establishments. To estimate the elasticity of labor supply, I utilize an experiment in the Norwegian public sector school system in which the variation in wages across schools for identical teachers is solely determined by the central government. In addition, and in contrast to the existing literature on labor supply, I am able to identify the establishments with excess demand. In such establishments, the supply is equal to actual employment. Using fixed-effects models, the average elasticity of labor supply is estimated to be in the range 1.0–1.9. The results also indicate that the elasticity is largest at schools located in densely populated areas in which few other teachers have a wage premium.

The estimated supply elasticity in the present paper is a partial short-run effect. It is a partial effect because schools and school districts cannot influence the wage level. The general equilibrium effect is smaller in the case of wage spillovers. It is a short-run effect because at a large majority of schools the teachers receive a wage premium only for a limited time period. With expectations of permanent wage premiums, the expected value of the wage premium increases. In addition, higher teacher wages at average would stimulate more students to undertake teacher education. When frictions are present in the labor market, as this study suggests, some wage-setting power lies in the hands of the establishments. However, the possible exploration of monopsony power in the short run must be balanced against long-run considerations when workers can more fully react to wage differentials.

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