

Teacher Mobility Responses to Wage Changes: Evidence from a Quasi-Natural Experiment

By TORBERG FALCH*

Low mobility responses to wage changes imply rents associated with existing employment relationships. For example, continued employment after a wage cut, all else equal, reflects rents in the hand of the worker at the outset; and, symmetrically, voluntary quits after a wage rise reflect that a position with higher rent has become available. The idea that the flow of workers who leave and join a firm is determined by the wage the firm chooses is denoted dynamic models of monopsony by Alan Manning (2003). Imperfect competition in the labor market yields rents to be shared between employers and workers, and thus the individual employment relationship is important both for the worker and the employer (Manning 2010).

Wages respond to worker flows in monopsony models. How to instrument for wages at the firm level is the main econometric challenge in estimating wage effects on worker mobility. This paper utilizes a Norwegian experiment with exogenous wage changes to study individual teachers' turnover decisions. Within a completely centralized wage setting system, teachers in some schools with a high degree of teacher vacancies in the past got a wage premium of about 10 percent in the period 1993–1994 to 2002–2003. The empirical strategy utilizes that few schools paid the wage premium during the entire period.

There is a robust negative correlation between wages and labor turnover (see Manning 2010 for an overview of the literature). The size of the estimated relationship, however, varies. That is also the case for studies on the teacher labor market. For example, Michael R Ransom and David P. Sims (2010) find a highly significant effect of wages on the probability that teachers leave teaching, while Eric A. Hanushek,

John F. Kain, and Steven G. Rivkin (2004) find no effect in models with school district fixed effects. If wages are set in a compensating way and the empirical model does not sufficiently condition on the relevant amenities, the wage effect is likely to be underestimated. The bias is in the other direction if wealthy school districts use the wage to attract high-quality teachers. The only previous paper that uses a policy intervention to investigate teacher quit behavior is Charles Clotfelter et al. (2008). They exploit a three-year bonus program in North Carolina public schools serving disadvantaged students, and find that the bonus significantly reduced the turnover rate.

In a fixed-effects framework, I find that the wage premium reduces the probability of voluntary quits by about 6 percentage points. The implied quit elasticity of about 3.5 is in accordance with the results in Falch (2010) in which I exploit the same experiment, but in a static model focusing directly on teacher supply at the school level using data from another data source and for a shorter time period. Estimating the wage effect on voluntary quits eliminates possible behavioral effects at the school level.

I. The Quasi-Natural Experiment

In Norway permanent teacher positions are basically guaranteed life-long employment at a specific school. The school district cannot move a teacher in a permanent position from one school to another without an explicit approval by the teacher. According to the school act, a person who is not certified as a teacher can be employed only if no certified teacher applies for a vacant teacher position, and noncertified teachers can be hired for up to only one school year. The teacher union closely monitors the hiring process and ensures that the schools follow the law. Teacher shortages are frequently measured as the share of non-certified teachers.

*Department of Economics, Norwegian University of Science and Technology, N-7491 Trondheim, Norway (e-mail: torberg.falch@svt.ntnu.no). Comments from Bjarne Strøm, Julie Cullen, Barry Hirsch, Helen Ladd, Alan Manning, and Michael Ransom are greatly acknowledged.

TABLE 1—WAGE PREMIUM ELIGIBILITY CRITERIA

Eligibility criterion	School year	Number of eligible	
		Schools	Teachers
More than 20% teacher shortages previous school year	1993–94	70	405
	1994–95	88	489
	1995–96	97	581
More than 30% teacher shortages previous school year	1996–97	22	66
	1997–98	32	110
More than 20% teacher shortages on average during the four last school years	1998–99	63	297
	1999–00	91	454
	2000–01	106	543

In the period studied, the wage determination was completely centralized. The wage of an individual teacher was determined solely by education level and teaching experience, but with one exception. Teachers in compulsory primary and lower secondary public-sector schools (first through tenth grade) with teacher shortages and located in the northernmost part of the country were eligible for a wage premium of about 10 percent. The wage premium was paid by the central government, and had thus no financial implications for the school districts.¹

The eligibility criterion for the wage premium varied in the empirical period, as shown in Table 1. Few schools were eligible in the restrictive system in 1996–1997 and 1997–1998, while about three times as many schools paid the wage premium in the preceding and following years.² An important change in 1998–1999 was that eligibility required high teacher shortages over a longer period. In years without changes in the eligibility criterion, teachers staying at the same school did not lose the wage premium. That is why the number of schools with a wage premium increases, except when the criterion changes. See Falch (2010) for a more detailed description of the relevant institutions.

¹ The wage premium was a fixed amount in nominal terms that changed in 1994 and 1998. The average percentage wage premium was lowest in 1993–1994 (about 7.5 percent) and highest in 1998–1999 (about 12 percent).

² The classification of schools was done by state representatives in the relevant counties and based on teacher man-year data collected by the state representatives up to 1997–1998 and directly by the state thereafter. Because the criterion for a higher wage was previous teacher shortages, it has always been known well in advance of a new school year which school would pay the wage premium.

Schools at which teachers received a wage premium at least once during the empirical period are denoted experimental schools in this paper. The system of wage premia was in place until 2002–2003, but since some local wage flexibility was introduced from the school year 2001–2002, only data up to 2000–2001 are included in the present analysis. In total, there are 161 experimental schools, in which teachers in 104 schools received a wage premium in under four years.

Proprietary teacher data with school identifiers have been provided by Statistics Norway. The data consist of all teachers who have been working at an experimental school in the empirical period. Other teachers do not contribute to the identification in models with school fixed effects. Since I will include individual fixed effects in one model specification, the sample includes all observations of the teachers who have been working at an experimental school. In order to identify teacher behavior, observations of temporary positions are excluded. The sample includes only certified teachers in permanent positions, and thus only voluntary quits.

Of the relevant 90 school districts, 77 percent include at least one experimental school in the empirical period. The average quit rate in the sample is 18 percent. The relatively high quit rate seems to be partly related to the fact that experimental schools at the outset are unpopular among teachers, and partly related to the fact that the sample consists of relatively small schools. The average quit rate at the national level in the empirical period was about 10 percent. Almost half of the quits are out of teaching, about 30 percent went to schools in other school districts, and about 20 percent are in schools within the same district.

II. Estimating the Quit Elasticity

Figure 1 presents the density of the change in the average quit rate at the school level when a wage premium is introduced and removed. The difference-in-differences at the school level shows the variation which identifies the wage effect. The distribution of the change in the quit rate when a wage premium is introduced is clearly to the left of the distribution when the wage premium is removed. The mean values are -0.04 and 0.09 . The difference of 13 percentage points indicates an average wage effect on the quit probability of 6.5 percentage points.

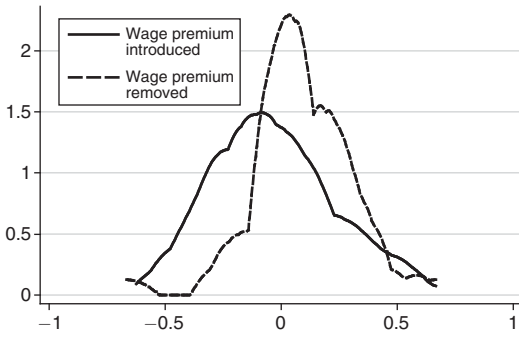


FIGURE 1. KERNEL DENSITIES FOR CHANGES IN QUIT RATE AT THE SCHOOL LEVEL

I estimate linear probability models that relate teachers' quit decisions to whether their school pays a wage premium the next school year, consequently relying on quits at the end of the school years 1992–1993 to 1999–2000. The model includes time fixed effects, school fixed effects, and school size. The identifying assumption is that time varying school characteristics, except school size, are not correlated with both the wage premium and the quit propensity.

With this simple model formulation, the wage premium reduces the probability to quit a school by 4.8 percentage points (see column 1 in Table 2). The next model in Table 2 includes time-specific school district fixed effects, which capture characteristics of the choice set of the teachers. Then the effect of the wage premium increases to 5.8 percentage points. Some characteristics of the school districts influence teacher behavior somewhat.

The model in column 3 of Table 2 includes a range of observable individual characteristics such as marital status, age, and children, which does not affect the wage effect. One reason may be that mobility costs are not important, another that mobility cost factors are captured by the fixed effects in the model.

Individual fixed effects are included in the last model in Table 2. Since the dependent variable is a dummy variable, it varies only for individuals who move in the sample period. Thus, the weight on mobile individuals is higher than in the other models, and these individuals may be most responsive.³ The effect of the wage

³ The sample consists of 1,810 teachers, for which 289 are observed in only one year; 59 percent of the rest of the

TABLE 2—WAGE EFFECTS ON TEACHER QUIT DECISIONS

	(1)	(2)	(3)	(4)
Wage premium next year	-0.048 (0.013)	-0.058 (0.019)	-0.058 (0.019)	-0.071 (0.024)
Time fixed effects × school district fixed effects	No	Yes	Yes	Yes
Individual characteristics	No	No	Yes	Yes
Individual fixed effects	No	No	No	Yes
Standard error of equation	0.3713	0.3674	0.3608	0.3103
Observations	7,867	7,867	7,860	7,860

Notes: Linear probability models. Standard errors that are adjusted for heteroskedasticity and clustered at the school level are reported in parentheses. All models include time and school fixed effects and the log of the number of students at school. The individual characteristics included in models (3) and (4) are gender; dummy variables for each age; dummy variables for marital status (unmarried, married, divorced, and widow/widower); dummy variables for children (below 6 years of age, 6–18 years of age, above 18 years of age, and no children); interaction terms between the dummy variables for marital status and gender; interaction terms between the dummy variables for children and gender; interaction terms between the dummy variables for children and marital status; 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. Full model results are available upon request.

premium is larger in this model than in the previous models, which clearly indicates that the previous estimates are not biased upward by omitted individual characteristics.

The effect in column 3 of Table 2 implies a quit elasticity of about -3.5.⁴ This is similar to the wage response estimated by Clotfelter et al. (2008), who find a quit elasticity in the order of -3 to -4, despite major differences in the policy intervention. The Norwegian experiment lasted for a much longer period (10 versus 3 years), the wage premium was higher (on average 10 versus 4 percent), all certified teachers were included (in contrast to only math, science, and special education teachers in North Carolina),

teachers quit at least once in the sample period. Using the latter sample, the wage effect increases to -0.099 in the model specification in column 3 in Table 2.

⁴ The quit elasticity is given by $\epsilon_{qw} = (\partial q / \partial P) \times (\partial P / \partial \ln W) (1/q)$, where q is the quit, P is the indicator for wage premium, and W is the wage.

and the system was well known for the teachers (Clotfelter et al., p. 1,355, argue that “the vast majority of teachers ... misunderstood the provisions of the bonus program”). The estimate is also larger than most other studies on teacher behavior. For example Ransom and Sims (2010) find a separation elasticity of -1.8 .

Does the estimated wage effect reflect worker response to pecuniary incentives or to specific features of the policy intervention? In the case of the former, the wage effect should be constant over time. The eligibility criterion for the wage premium changed in 1996 and 1998, as shown in Table 1. In particular, the latter change is not trivial since the wage premium then became related to average teacher shortages during the last four years and not only the past school year, as in the previous systems. The possibility of influencing the classification of schools seems largest in the former systems, but such potential gaming would arguably be most relevant for recruitment decisions. It turns out that the wage effect is larger in the last system than in the former systems (-0.080 versus -0.046 in an interaction term specification), but the difference is insignificant at conventional levels (t -value of 0.93).

I have also tested whether the wage effect differs between the first year with wage premium, the last year with wage premium, and the other years. If, as intended, it was well known in advance which schools would pay a wage premium, the wage effect should not be smaller either the first or the last year with wage premium at the school. The point estimates indicate that the wage response is largest the last year and smallest the first year, but the interaction effects are again insignificant (t -values of 0.76 and 1.27, respectively).

The literature on individual labor supply finds that women’s working hours are more responsive to the wage than men’s working hours (see, e.g., John Pencavel 1998). In addition, Pencavel finds that the wage elasticity is highest for married women and young women. It is not straightforward to translate these elasticities at the market level into mobility responsiveness. One could expect that people who respond strongly in terms of hours also respond strongly in terms of mobility. The usual interpretation, however, is that women are more responsive in terms of hours since an attractive alternative without much uncertainty is to stay at home. But

then they are also less geographically mobile, working in the direction of quit responsiveness. In addition, the evidence indicates that women are more risk averse than men, see, for example, Catherine C. Eckel and Philip J. Grossman (2008), who work in the direction of less mobility. At the firm level, Manning (2003) finds similar separation elasticities for both genders, while Boris Hirsch, Thorsten Schank, and Claus Schnabel (2010) and Ransom and Ronald L. Oaxaca (2010) find that the elasticity is smaller for women than for men.

Table 3 presents results from models estimated on subsamples related to teachers’ gender, marital status, age, and parenthood. The last is included since it is expected that people are less geographically mobile when they have school-age children. The wage effect is larger for male than for female teachers. Regarding marriage, the wage effect is larger for teachers who are married each year in the empirical period than for other teachers. The wage effect seems, however, to be independent of teacher age and whether the teacher has school-age children in the empirical period. These results combined indicate that the size of the wage effect is not simply related to how geographically mobile the individual teachers are.

Voluntary quits are related to changes in labor supply. In a dynamic steady state, where the number of quits equals the number of recruits, the labor supply elasticity is given by $\hat{\epsilon}_{S_w} = (\epsilon_{R_w} - \epsilon_{q_w})$, where ϵ_{q_w} and ϵ_{R_w} are the elasticity of quits and recruits with respect to the wage, respectively (see David Card and Alan Krueger 1995). Manning (2003, 2010) argue that $\epsilon_{R_w} = -\epsilon_{q_w}$ is a reasonable approximation in steady state, with the intuition that every quit from one employer is a recruit of another employer. Then the long-run labor supply elasticity for the Norwegian teachers is approximately 7.0.

One should, however, be careful in interpreting the present finding in terms of long-run labor supply. The estimated wage effect on quits is partial since the schools and the school districts cannot influence the wage level. General equilibrium effects are smaller in the case of wage spillovers. In addition, the policy intervention was short-term in nature since teachers in most schools received the wage premium only for a limited time period. That was indeed the intention of the intervention, and the identification on

TABLE 3—HETEROGENEOUS WAGE EFFECTS ON TEACHER QUIT DECISIONS

	Female	Male	Married each year	Not married	Average age below 38	Average age above 38	No children age 6–18 any year	Children age 6–18
Wage premium next year	−0.040 (0.026)	−0.110* (0.034)	−0.092* (0.026)	−0.028 (0.031)	−0.045 (0.042)	−0.073* (0.023)	−0.086* (0.037)	−0.067* (0.028)
Observations	4,723	3,274	3,920	3,940	3,281	4,579	3,534	4,326
Standard error of equation	0.3576	0.3604	0.3285	0.3805	0.3928	0.3272	0.3869	0.3326

Note: Same model specification as in column 3 of Table 2.

*Significant at the 5 percent level.

within-school variation is based on the fact that the premiums were short-lived.

The short-run supply elasticity is simply $\varepsilon_{S_w} = q \cdot \varepsilon_{S_w}^L$, which is equal to $1/4$ in the present case. This result is remarkably similar to the findings in Falch (2010), who analyzes the same experiment for the period 1995–1996 to 2000–2001, using school-level data on employment, and finds a supply elasticity of about 1.4. One would expect that investigating dynamic behavior, as in the present paper, reduces potential identification problems related to mean reversion that might plague analyses using employment data.

William M. Boal and Ransom (1997) have developed a measure of exploitation in monopsony markets that is given by the amount the wage lies below the marginal revenue of the firm in percentage terms. The rate of exploitation is a weighted average of the short-run and long-run inverse labor supply elasticities, where the weights are given by $r/(1+r)$ and $1/(1+r)$, respectively. Using the discount rate $r = 0.05$ and the labor supply elasticities calculated above, the rate of exploitation is about 17 percent in the present case.

The rate of exploitation can be considered as the monopsony rent per worker if the firm is able to capture the whole rent. Rent-sharing is probably a better description of reality, in particular in unionized markets, and the rate of exploitation can, rather, be considered as the potential rent to be shared between workers and employers.

III. Conclusion

Causal evidence on wage effects at the establishment level is hard to establish, since in general a myriad of factors influence observed wages. This paper exploits centralized determined wage differences for teachers in Norway

in a fixed-effects framework. I find that the effect of a wage premium on voluntary quits is significant, but not massive. Interpreted in a labor supply framework, the results imply a labor supply elasticity of about $1/4$ in the short run. In contrast to individual supply of working hours, the wage responsiveness is largest for married men.

When frictions are present in the labor market, as this study suggests, wage-setting power exists at the establishment level. The estimate in this paper indicates that the potential rent to be shared in an employment relationship is about 17 percent of the marginal revenue of the firm.

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