Teacher mobility responses to wage changes: Evidence from a quasi-natural experiment

Torberg Falch*

Department of economics

Norwegian University of Science and Technology

Abstract

This paper utilizes a Norwegian experiment with exogenous wage changes to study teacher's turnover decisions. Within a completely centralized wage setting system, teachers in schools with a high degree of teacher vacancies in the past got a wage premium of about 10 percent during the period 1993-94 to 2002-03. The empirical strategy exploits that several schools switched status during the empirical period. In a fixed effects framework, I find that the wage premium reduces the probability of voluntary quits by six percentage points, which implies a short run labor supply elasticity of about 1¼ .

December 2010

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 reflects 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 on worker flows in monopsony models. How to instrument for wages at the firm level is the main econometric challenge in order to estimate 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 1993-94 to 2002-03. The empirical strategy exploits that few schools paid the wage premium in the whole 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 the case also 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, Elizabeth Glennie, Helen Ladd, and Jacob Vigdor (2008). They exploit a three-year long 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 life-long employment guarantees at a specific school. The school district cannot move a teacher in a permanent position from one school to another school without a major school downsizing or an explicit approval by the teacher. According to the school act, a person that is not certified as a teacher can only be employed if no certified teacher applies for a vacant teacher position, and the noncertified teachers can only be hired for up to one school year. Representatives of the teacher union are fully informed about every hiring process. The union closely monitor that the schools follow the law, which has been one of the cornerstones in the teacher trade union policy. Teacher shortages are frequently measured as the share of noncertified teachers.

The wage determination was in the empirical period completely centralized. The wage of an individual teacher was solely determined by educational level and teaching experience, but with one exception. Teachers in compulsory primary and lower secondary public–sector schools (first through tenth grade) with particular 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 1996-97 to 1997-98, while about three times as many schools paid the wage premium in the preceding and following years.[2] An important change in 1998-99 was that eligibility required persistent teacher shortages in the past. 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 wage premium increases except when the criterion changes. See Falch (2010) for a closer description of the relevant institutions.

Schools at which teachers received wage premium at least once during the empirical period will be denoted experimental schools in this paper.[3] In total there are 161 experimental schools, in which teachers in 104 schools received a wage premium in less than 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 also will estimate models with individual fixed effects, the sample includes all

[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 1993-94 (about 7.5 percent) and highest in 1998-99 (about 12.0 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 1997-98 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 that would pay the wage premium.

[3] The system with wage premium was in place until 2002-03, but since some local wage flexibility was introduced from the school year 2001-02, only data up to 2000-01 are included in the present analysis.

observations of the teachers that have been working at an experimental school. In order to identify teacher behavior, observations of temporary positions are excluded. The sample only includes certified teachers in permanent positions, and thus only voluntary quits.

Table 1. Wage premium eligibility criteria

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

77 percent of the relevant 90 school districts 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. Almost half of the quits are out of teaching and 30 percent are to a school in another school district. Teachers receiving wage premium if they stay move to another school to a somewhat smaller extent than teachers without wage premium.

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. This difference-in-difference at the school level shows the variation on which the wage effect is identified. 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, respectively. The difference of 13 percentage points indicates an average wage effect on the probability to quit of 6.5 percentage points.
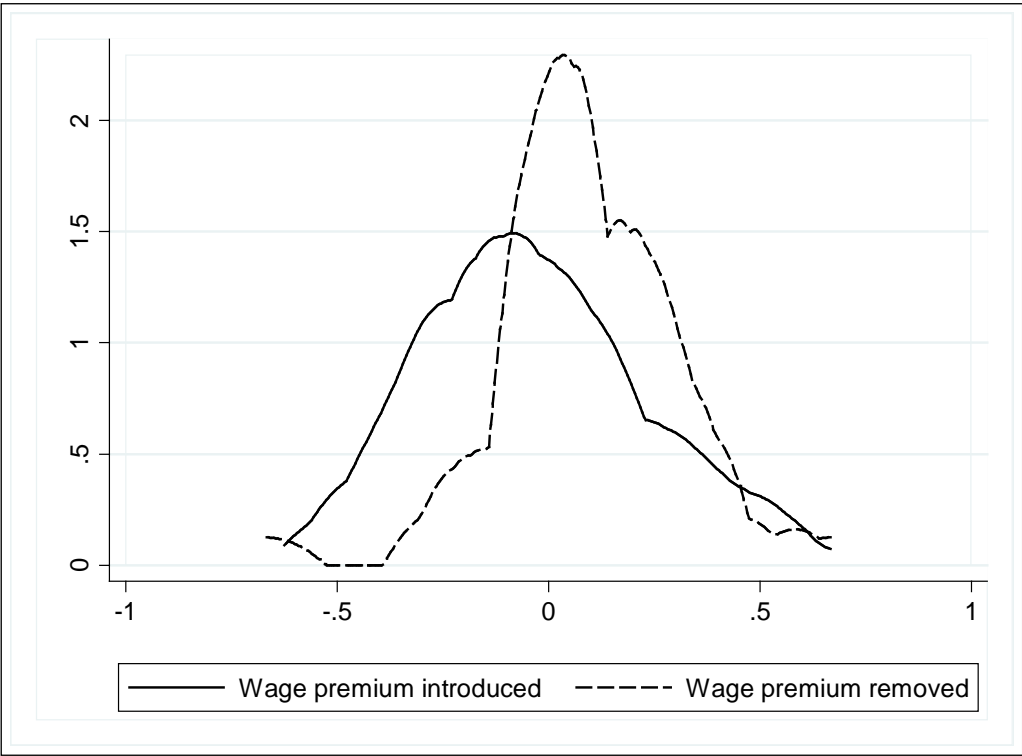

Figure 1. Kernel densities for changes in quit rate at the school level

I will estimate linear probability models that relate the quit decision to whether the school pays a wage premium the next school year, consequently relying on quits at the end of the school years 1992-93 to 1999-2000. The model includes several individual characteristics as marital status, age, and children, in addition to time fixed effects and school fixed effects. Thus, the

identification is on within-school variation in the wage premium. In addition, some specifications include time-specific school district fixed effects, which capture characteristics of the choice set of the teachers, and individual fixed effects.

The model in column (1) in Table 2 only includes time and school specific effects in addition to the indicator for wage premium. With this simple model formulation, the wage premium reduces the probability to quit by 4.8 percentage points. The next model includes interactions between time fixed effects and school district fixed effects. Then the effect of the wage premium increases to 5.8 percentage points. The more flexible description of teachers' choice set increases the estimated response, but characteristics at the school district level do not seem to be highly correlated with the wage premium and the quit probability.

The model in column (3) in Table 2 includes a range of observable individual characteristics, 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. This model specification is less vulnerable to omitted variable bias. On the other hand, since the dependent variable is a dummy variable, it only varies for individuals that 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.[4] The effect of the wage premium is larger in this model than in the previous models, which clearly indicates that the previous estimates are not biased upwards by excluded individual characteristics.

---

[4] The sample consists of 1810 teachers, for which 289 are observed in only one year. 59 percent of the rest of the 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.

Table 2. Wage effects on teacher quit decisions

	(1)	(2)	(3)	(4)
Wage premium next year	-0.048	-0.058	-0.058	-0.071
	(0.013)	(0.019)	(0.019)	(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

Note. 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.

The effect in column (3) in Table 2 implies a quit elasticity of about -3.5.[5] 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 vs. 3 years), the wage premium was higher (on average 10 vs. 4 percent), all certified teachers were included (in contrast to only math, science and special education teachers in North Carolina), and the system was well known for the teachers

---

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

(Clotfelter et al., p. 1355, 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 workers 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 to influence 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 (-0.046 vs. -0.080 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 differ between the first year with wage premium, the last year with wage premium, and the other years. If it was well known in advance which schools that would pay a wage premium, as intended, the wage effect should not be smaller either the first or the last year a school paid the wage premium. 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 less quit responsiveness. In addition, the evidence indicate that women are more risk averse than men, see for example Catherine C. Eckel and Philip J. Grossman (2008), which arguable work in the direction of less job shifts. 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 that allow the marginal effects of the wage premium to depend on teachers' gender, marital status, age, and parenthood. The latter is included since it is expected that people are less geographically mobile when they have school-aged 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 does have school-aged children in the empirical period or not. 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 $\varepsilon_{Sw}^{L} = \left(\varepsilon_{Rw} - \varepsilon_{qw} \right)$, where ε_{qw} and ε_{Rw} 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 $\varepsilon_{Rw} = -\varepsilon_{qw}$ 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.

Table 3. Heterogeneous wage effects on teacher quit decisions

	Female	Male	Married each year	Not married	Average age below 38	Average age above 38	Not children age 6–18 any year	Children age 6–18
Wage premium next year	-0.040	-0.110*	-0.092*	-0.028	-0.045	-0.073*	-0.086*	-0.067*
	(0.026)	(0.034)	(0.026)	(0.031)	(0.042))	(0.023)	(0.037)	(0.028)
Observations	4 723	3 274	3 920	3 940	3 281	4 579	3 534	4 326
Std error of eq.	0.3576	0.3604	0.3285	0.3805	0.3928	0.3272	0.3869	0.3326

Note. Same model specification as in column (3) in Table 2. * denotes significance at five percent level

One should, however, be careful in interpreting the present finding in terms of long run labor supply. The estimated wage effect on quits are 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 within-school variation is based on the fact that the premiums were short-lived.

The short run supply elasticity is simply $\varepsilon_{Sw} = q \cdot \varepsilon_{Sw}^{L}$, which is equal to 1¼ in the present case. This result is remarkably similar to the findings in Falch (2010). Falch (2010) analyzes the same experiment for the period 1995-96 to 2000-01, 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 in particular reduces identification related to mean reversion that plagues analyses using employment data.

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 1¼ 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, some wage-setting power exists at the establishment level. Exploration of monopsony power in the short run must, however, be balanced against long-run implications when workers can more fully react on wage differentials.

References

Card, David E., and Alan B. Krueger. 1995. *Myth and Measurement: The New Economics of the Minimum Wage*. New Jersey: Princeton University Press.

Clotfelter, Charles, Elizabeth Glennie, Helen Ladd, Jacob Vigdor. 2008. "Would Higher Salaries Keep Teachers in High-Poverty Schools? Evidence from a Policy Intervention in North Carolina." *Journal of Public Economics*, 92(5-6): 1352-1370.

Falch, Torberg. 2010. "The Elasticity of Labor Supply at the Establishment Level." *Journal of Labor Economics*, 28(2): 237-266.

Hanushek, Eric. A., John F. Kain, and Steven G. Rivkin. 2004. "Why Public Schools Lose Teachers." *Journal of Human Resources* 39(2): 326-354.

Hirsch, Boris, Thorsten Schank, and Claus Schnabel. 2008. "Differences in Labor Supply to Monopsonistic Firms and the Gender Pay Gap: An Empirical Analysis Using Linked Employer-Employee Data from Germany." *Journal of Labor Economics*, 28(2): 291-330.

Manning, Alan. 2003. *Monopsony in Motion. Imperfect Competition in Labor Markets*. New Jersey: Princeton University Press.

Manning, Alan. 2010. "Imperfect Competition in the Labour Market." CEP Discussion Paper No 981.

Pencavel, John. 1998. "The Market Work Behavior and Wages of Women: 1975-94." *Journal of Human Resources,* 33: 772-804.

Ransom, Michael R, and Ronald L. Oaxaca. 2010. "New Market Power Models and Sex Differences in Pay." *Journal of Labor Economics*, 28(2): 267-289.

Ransom, Michael R, and David P. Sims. 2010. "Estimating the Firm's Labor Supply Curve in a "New Monopsony" Framework: Schoolteachers in Missouri." *Journal of Labor Economics*, 28(2): 331-355.

Eckel, Catherine C., and Philip J. Grossman. 2008. ''Men, Women and Risk Aversion: Experimental Evidence.'' In Charles R. Plott and Vernon L. Smith (Eds.) *Handbook of Experimental Economic Results*. Amsterdam: Elsevier.