# Organizational change and employee labor market outcomes

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## Abstract

This paper exploits a natural experiment in the form of a substantial Danish municipal reform where municipalities were merged into new, fewer, and larger municipalities. Exploiting a rich register data set, I analyze the impact of municipal mergers on employee wages, job change and exit to early retirement. Results indicate that mergers were associated with an increase in the probability of early retirement for older employees, but also higher wages and reduced job exits for younger workers. Moreover, there is considerable heterogeneity in the age distribution of the effects on wages and job exits.

Keywords: Organizational change; Mergers; Personnel Economics; Natural experiment; Inverse Probability Weighting

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# 1. Introduction

Firms regularly carry out organizational changes in order to enhance firm productivity, improve work practices, and benefit from economies of scale. Yet, while firms may experience positive structural gains in terms of improved productivity in the long run, reorganizing work practices can have negative side effects in terms of short- and medium-term consequences for the employees (Napier, 1989; Pritchett, 1985; Røed and Fevang, 2007; Dahl, 2011) and ultimately reduce labor productivity and incur financial losses (Krueger and Mas, 2004; Mas, 2008). Among the most radical forms of organizational change are mergers of several firms or institutions. During the transition period of a firm merger, the staff may experience uncertainty concerning daily work routines, the future development of their work place, and their own personal career prospects in the organization. This in turn may affect job satisfaction and lead to personal stress, health problems, higher worker turnover, and earlier retirement than usual. Moreover, firms may lose employees with the most favourable outside options, incurring short- and medium-term costs.

This paper identifies the causal effects of organizational merger on three selected labor market outcomes, namely wages, job change and early retirement, of employees in Danish municipalities. Causality is established by exploiting a large reform that implied the merger of a number of municipalities. The Danish municipal reform of 2007, which reduced the number of municipalities from 270 to 98, provides a unique opportunity of gaining new insights into how employees adapt to a merger. As the many simultaneous municipal mergers were the result of an exogenously determined local government reform, rather than motivated by characteristics of the work force, the merger was exogenous to the outcomes studied. Exogeneity of the organizational merger is crucial for the identification strategy.

The paper uses an extremely rich longitudinal data set with individual level information on labor market outcomes and socioeconomic factors for all employees in the municipalities before and after the reform. This rich data set allows us to control carefully for employee background and to analyze heterogeneity in the response to organizational change. Furthermore, the data can be exploited to model the probability of individuals working in a merged municipality in a treatment effects framework that reduces the disparities across the treatment and control group by assigning weights to individuals in the treatment and control groups based on the inverse of their propensity score.

Organizational change as a consequence of municipal merger may affect the employees in a number of ways. For a substantial part of the employees, the merger will be associated with changes in geographical work site, new management, new colleagues, and various changes in work practices. These changes are expected to be more profound for employees from smaller municipalities being merged into larger municipalities as the power of smaller municipalities may be weaker than the power of municipalities that dominate in terms of size in the new municipalities. While changes are expected to create disturbances in the work routines and require adjustment on behalf of the employees in the short- and medium-run, the medium- and long-run effects are likely to be associated with technological progress and higher productivity. Given that the paper draws on such a large and unique longitudinal data set, it is possible to examine heterogeneity in employee reactions to the reform across education groups and age groups.

## 2. Background and hypotheses

When explaining employee behaviour to organizational change, the paper draws on human capital theory and the literature on personnel economics. According to human capital theory (Mincer, 1958; Becker, 1964; Ben-Porath, 1967), the optimal level of investment in human capital decreases over the life time. Employees in an organization as diverse as a municipality will typically have very different educational backgrounds. A substantial part of the municipal employees have no formal education beyond primary school or have undertaken a (firm-specific) short vocational education in the municipality system, e.g. in the administration and social services. Others possess medium-long or long academic educations (college level and above). Hence, different employee groups in the municipal sector possess a different level as well as composition of human capital. Employees who have acquired a relatively large part of their education through on-the-job training will possess human capital dominated by knowledge of routines, systems and work practices, which are to some extent specific to the organization (municipality) where they work. The extent of their (firm-specific) human capital is rising in their experience and tenure (and thus age). In constrast, employees with a college degree and above (medium-long or long education, e.g. an academic education or a distinct profession as nurse or school teacher) will have acquired a relatively large share of

their human capital through general or formal education, while firm-specific (municipal-specific) human capital plays a more modest role in their total human capital.

Drawing on the framework from personnel economics (Lazear, 1991, 1995, 2008; Lazear and Oyer, 2008; Lazear and Shaw, 2007), the effects of organizational change are comparable with the effects of an unexpected shock to the (usual) rate of technological change. First, organizational change through its many changes in work practices renders some parts of (mainly firm-specific) human capital obsolete. And second, organizational change leads to technological progress through more efficient work practices. For the average worker, this shock will raise the depreciation rate of human capital and create uncertainty associated with the returns to additional human capital investment. Bartel and Sicherman (1993, 1998) emphasise the importance of distinguishing between the *permanent* rate of technological change and *shocks* to technological change. While, on the one hand, organizations with a high permanent *level* of technological change hire workers with a high level of general or formal education who easily adapt to new work practices, *shocks* to an organization in terms of *unexpected exogenous technological change* may, on the other hand, make previously acquired skills obsolete. Thus, workers with lower levels of human capital are generally more vulnerable, while workers with a higher level of formal education (general human capital) are expected to more easily adjust to new work practices. In situations where an organization permanently experiences a high level of technological change, the organization will hire more workers with high levels of general human capital. And in situations where an organization experiences an exogeneous shock leading to technological change, a higher level of formal education will ease the adjustment to the new situation. Thus, workers with high and low levels of human capital, respectively, are expected to react differently to organizational mergers.

The empirical evidence on the effects of organizational change is mixed. Gaynor et al. (2012), examining the impact of British hospital mergers on a large set of outcomes including financial performance, productivity, waiting times and clinical quality, find little evidence that mergers achieved gains other than a reduction in activity. Caroli and Van Reenen (2001) investigate the consequences of organizational changes in a panel of British and French establishments, arguing that organizational changes and skills are complements, thereby offering support for the hypothesis of "skill-biased" organizational change. In particular, they show empirically that organizational change reduces the demand for unskilled workers, is

negatively associated with increases in regional skill price differentials (a measure of the relative supply of skill), and leads to greater productivity increases in establishments with larger initial skill endowments. Hanushek et al. (2011), however, find that there is a relative labor market advantage of vocational education over general education, but that this advantage decreases with individual age. They find strong support for the existence of a trade-off between general and vocational education.

A related issue is whether new organisational and workplace practices are biased against age. Aubert et al. (2006) suggest that the direction of the relationship between work reorganisation (measured e.g. by IT adoption) and the age structure of the workforce is a priori uncertain. *On the one hand*, the relationship may be *positive* as older workers who are often more skilled and experienced may benefit relatively more from technological and organisational innovations. *On the other hand*, the relationship may be *negative,* partly as innovation may accelerate skills obsolescence and thereby imply that productivity of older workers deteriorates relative to younger ones, partly as innovations disproportionately challenge older workers' adaptability skills. Using a French dataset with linked employer-employee, Aubert et al. (2006) find evidence that the wage-bill share of older workers is lower in innovative firms. Moreover, their analyses show that new technologies enhance hiring opportunities for younger workers more than for older workers. Thus, organisational innovations reduce exits much less for older workers than for younger ones, suggesting that older workers are struck by skills obsolescence or lack of adaptability during organisational change. Benaghel et al. (2011), using also a panel of French firms, find support of age biased technical and organisational change, but also show that training can ameliorate the deteriorated employment prospects of older workers.

Research on how organizational change impacts on worker well-being indicates that the effects depend on the nature of the change (for a survey, see Bryson et al., 2013). Organizational change that favours "high-involvement" work practices through ameliorating workers' decision-making power at the workplace is expected to improve job satisfaction. However, high-involvement work practices may also lead to higher levels of work intensity, overload, and worker stress. According to Bryson et al. (2013), employee influence through trade unions can shape both the nature and process of organizational change, improve security and social support among employees, and improve worker well-being through higher wages. The latter can be seen as a form of rent-sharing. Gant et al. (2002) stress the role of social capital in the

transmission to new work practices in a study that carefully maps networks between employees in steel finishing lines.

A recent Danish study by Dahl (2011) combines Danish high-quality register data with a firm-level survey of organizational changes in the period 1998–2000 in 1,517 of the largest Danish firms with comprehensive medical histories of their 92,860 employees. Dahl (2011) shows that the risk of receiving stress-related medication increases significantly for employees in firms subject to various forms of organizational change. Along the same lines, Askenazy et al. (2002) find that changing working conditions lead to mental strain and a higher probability of work injuries. However, Netterstrøm et al. (2010) find no significant effects on self-reported depression in a study of 685 employees in Danish municipalities and counties after the 2007 municipal reform.

The previous empirical literature thus shows that worker well-being is affected by organizational change. Moreover, recent research shows that there is a positive and statistically-significant relationship between job satisfaction and workplace performance, see e.g. Bryson et al. (2015), Böckerman and Ilmakunnas (2012), Forth and McNabb (2008). Moreover, Bartel et al. (2011) find that organizational performance is affected by employee attitudes. This suggests that the consequences for employees should be taken into account when considering organizational change.

This paper treats the Danish 2007 municipal reform as an unexpected exogenous shock to technological change. The paper explores whether municipal employees experienced a change in their labor market outcomes as a consequence of the reform. The analyses, which focus on the period 2003-09, investigate three labor market outcomes: wages, job change and exit to early retirement.

The empirical literature suggests that organisational change affects employees' wages, job search behaviour and exits into retirement. As discussed above, exit out of the job to either a new job in another organization or to early retirement is a rational strategy for individuals who find their work situation altered by the prospect of working in a new organizational structure. However, individuals who would have considered to change jobs anyway may also decide to wait-and-see, not only because the new organization potentially offers new career options, but also because many neighbouring municipalities were also undergoing mergers, so the alternative options were fewer than before.

Moreover, municipalities may try to ameliorate employee reactions to the mergers by raising the wages of the employees in order to convince them to stay on during the merger process (rent sharing). In particular, the analyses explore the following two hypotheses:

HYPOTHESIS H1: *Employees in the municipalities that merged experienced an increase in their wages relative to employees in municipalities that did not merge.*

HYPOTHESIS H2: *Employees in municipalities that merged reduced their propensity to change jobs relative to employees in municipalities that did not merge.*

Organizational innovations may be age- and skill biased. The paper explores whether the effects of mergers are heterogeneous across different employee groups, disentangling the effects of the mergers with respect to 1) employee education and 2) employee age, consistent with the idea of skill-biased and age-biased technological change found in the previous empirical literature discussed above.

Individuals may react differently depending on their age as the economic incentive to invest in additional human capital declines with age and proximity to retirement. According to Bartel and Sicherman (1993), an unexpected increase in the rate of technological change produces an increase in the depreciation rate of human capital, leading to a revised rate of investment in human capital. This in turn will induce older workers to retire sooner (than planned), because the required amount of retraining in order to adjust to new structures and new technology at the workplace will seem unattractive to older workers who have fewer remaining years in the labor market to harvest the returns to a human capital investment.[1] The early retirement scheme analysed here (the so-called *Efterlønsordning*) was offered to people aged 60-64 in the period studied. The expected effects can be summarized in hypothesis 3.

HYPOTHESIS H3: *Employees in the municipalities that merged experienced higher exit rates to early retirement than employees in municipalities that did not merge.*

Among employees who are below the age where early retirement was an option, job exits and wage changes may also depend on age. However, the direction of the heterogeneity may be difficult to predict. On

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[1] Unfortunately, there is no information on training at individual level in the data.

the one hand, younger employees may be more inclined to "wait and see" and take part in the reorganisation, thus reducing their job exits relatively more, while the middle-aged (40-55) may be less interested in investing in and getting to know the new organisation. Moreover, experience may work in favour of the older employees changing jobs. On the other hand, older employees may also experience worse outside options. Thus, it is difficult to foresee the direction of the age-bias in wages and job exits.

HYPOTHESIS H4: *Older (middle-aged, 40-54 years old) employees in the municipalities that merged are relatively less inclined to stay in the merged organisations, but may benefit relatively more from potential wage increases due to their higher experience.*

Consistent with the idea of skill-biased technological change, one may expect that employees will react differently according to their level of total human capital. Mergers that imply changes in organizational structures and work practices will reduce the value of some forms of human capital relative to others, possibly rendering firm-specific (municipal-specific) human capital obsolete to a larger extent than general/formal human capital. Therefore, employees with relatively more general/formal education may find it easier to adapt to new types of leadership, new structures and new routines - and may even experience that new challenges and professional possibilities follow with the new organisational structure. The paper distinguishes between individuals who have a college degree and above (and therefore a high level of general/formal human capital) and individuals who have none or a short education (but who may have more informal or firm-specific human capital acquired through tenure). Thus, individuals with a college education and above may presumably find it easier to adapt to the reform than individuals with little education and may therefore have fewer job changes. However, outside options were probably more favourable for those with relatively more formal education. One may, however, also observe that employees who had a position as managers or middle managers benefit less from the mergers in terms of career prospects as economies of scale were expected to lead to cost reductions, particularly in administration and management. As management positions are to some extent correlated with educational level, workers with higher levels of education will not necessarily gain from being part of a merger. This effect may to some extent outweigh the positive effect of human capital on labour market outcomes, so the net effect may be uncertain.

HYPOTHESIS H5: *Employees with relatively more formal education in the municipalities that merged benefit relatively more in terms of wages. The net effect on job change is uncertain.*

Finally, the paper hypothesizes that employees react differently depending on the relative size and power of their "old" municipality (i.e. the municipality they belonged to before the reform) compared to the new larger municipality. The analysis therefore distinguishes between individuals who experienced a merger coming from a "big brother" (a larger or dominating) municipality versus individuals coming from a "little brother" (a relatively smaller) municipality. This discussion, which links to Dahl (2011) on "breadth" and "depth" of organizational change, is formulated in the sixth and final hypothesis:

HYPOTHESIS H6: *Employees from "big brother" municipalities benefit relatively more from the merger in terms of wages and job prospects. The effect on early retirement is uncertain.*

## 3. Identification challenge and the 2007 municipal reform

A fundamental problem when trying to identify the causal effects of an intervention is that selection into the intervention is typically not exogenously determined. Organizational changes are often the result of the challenges an organization is facing, including factors related to the outcomes studied. Thus, mergers may happen in response to (unobserved) challenges that the firm faces. Consequently, when studying the effect of organizational change on e.g. employee outcomes, organizational change may be endogenous. In order to overcome such challenges for identification, the econometrician will usually look for exogenous variation in the explanatory variable of interest brought about through e.g. institutional changes. This paper exploits the 2007 Danish municipal reform as a case for organizational change in municipalities. The reform has previously been used as a quasi-experiment to study the causal effect of jurisdiction size on democracy (Lassen and Serritzlew, 2011) and on economies of scale (Blom-Hansen et al. 2014). In line with these studies, the effects of organizational changes on employee labor market outcomes are analyzed in a quasi-experimental design where the merged municipalities belong to the treatment group (subject to an exogenously induced organizational change), whereas the unmerged municipalities are part of the control group.

Municipalities in Denmark are responsible for public services such as schools, day-care, elderly care, and primary health care. Each municipality is governed by a city council from which a mayor is elected as the head of the daily administration. Municipalities finance part of their spending by levying taxes on income and property, the remaining part of the finances come from government transfers or transfers from richer municipalities.

Until 2003, which is our base year in the empirical analysis, the municipal reform was largely unexpected. In fact, in 2002, the prime minister guaranteed that a local government reform was unheard of. However, in the summer 2002 a debate on the possibility of local government reform evolved in the media, leading to the formation of a government commission on the regional administrative structure. In January 2004, the commission published a report that recommended structural reform both in terms of the size of the regional entities and in terms of the organization of tasks. Around six months later, negotiations between the government and the other parties and parliament were initiated, and, simultaneously with this, municipalities started negotiations with their neighbors on potential mergers. In 2005, a new law on local government structure was passed. The reform was passed with a small majority, as one of the largest Danish parties, the Social Democratic Party, did not vote in favor of the law. The reform has been named "the unthinkable reform" by political scientists Christiansen and Klitgaard (2008) based on the very short decision process and the lack of consensus which is unusual given the extent and potential impact of such a reform. The "surprise effect" of the reform, however, works in favor of the view presented in this paper of the municipality mergers being of an exogenous nature, supporting the identification of causal effects.

The reform lead to the formation of 98 local governments; 32 of the old local governments remained unchanged, whereas 66 were the result of amalgamations (mergers). The former (14) counties were merged into five large regions, which took over a number of tasks from the old counties. Moreover, the municipalities took over some of the tasks from the old counties. Thus, the reform was both a reform of organizational size and tasks. Societal connectedness in the form of e.g. commuting patterns played a key role, whereas political and economic similarity was not important in the matching-making process (Bhatti and Hansen, 2011). Generally, the municipalities freely formed coalitions with neighboring municipalities, under the restriction that the new local governments should have a size of at least 20,000 inhabitants.

The general anticipation among politicians and social planners was that the reform in general, and municipal mergers particularly, could potentially lead to leaner organizations and cost savings in the administration in particular due to economies of scale. Prior to the mergers, it was expected that the Danish local government structure reform would lead to significant improvements in productivity, particularly in administration. Moreover, the expectation was that larger local government entities would potentially lead the way for quality improvements in the delivery of public services through increased specialization, a higher reliance on highly educated employees, improved management and organization (Christoffersen, 2005). Three years after the mergers, in 2009, the amalgamated municipalities had not realized the expected administrative economies of scale with only modest productivity gains in the merged municipalities (Houlberg, 2012). However, a more recent study by the same author and co-authors points to a 10% reduction in administrative costs per capita in the merged municipalities after 2009, see Blom-Hansen et al. (2014) and Houlberg (2015). Table 1 summarizes the timeline of the reform.

**Table 1: Time table of municipal reform process**

| 2002 | Summer: The debate over a municipal reform started in the Danish media. October: The Prime Minister formed the so-called Structure Commission. |
|------|------|
| 2004 | January: The commission's report was released. Spring and summer: Negotiations between the government and its coalition partners. Negotiations among municipalities regarding possible mergers. |
| 2005 | Spring: The resulting laws were passed. November: Elections to the new municipal and regional councils. |
| 2006 | Process of implementation of municipal mergers. |
| 2007 | January 1: The reform ultimately came into effect. |

Municipalities that merged were spread geographically evenly over the country; see figure A.1 in the Appendix. Municipalities that did not merge, but on the contrary continued in an unaffected geographical form, were mainly concentrated around the larger cities. As I discuss later, this may constitute a challenge in our identification.

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## 4. Data

The empirical analysis builds on individual level register data from Statistics Denmark. These in an international context unique longitudinal data contain information for the entire Danish population.[2] The sample used in this paper covers the period 2002-2009, and the dataset contains individual information on wages, job switches, (early) retirement, family background, education, and prior labor market experience. Furthermore, the data contains information on a range of municipality level characteristics based on administrative data from the Danish Ministry of Interior.

A subsample of approximately 300,000 individuals who were employed in the municipalities prior to the reform (in 2003) is followed from 2003 to 2009, i.e. three years after the reform took place. Individuals who were younger than 25 or older than 64 in 2003 were deselected, and observations from the two largest municipalities, Copenhagen and Aarhus (that were not merged) were dropped, as their exceptional size made them unsuitable for the control group. Finally, some individuals had to be dropped due to missing information on e.g. municipality of work. The analyses on wages and job change focus on a subsample of individuals who were between 25-54 in 2003 and who worked in the entire period. The analyses on early retirement focus on individuals who were eligible for early retirement (i.e. in age groups 60-64) in the relevant periods under investigation.

## 5. Trends in wages, job change and early retirement

The paper focuses on three main outcomes: wages, job change and early retirement. These three outcomes are key in understanding the employment consequences for individual workers. If career prospects are altered with the reform, this is likely to be reflected in individual wages. Moreover, individuals may decide to leave the organization sooner than expected if their career options change, or if they find their work situation has deteriorated in the municipality. Employees can exit and find a job elsewhere, or they can choose to retire if they are close to the retirement age.

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[2] Register data from Statistics Denmark consists of individual level data from administrative registers. These high-quality data are available to researchers in an anonymized form, implying that individuals cannot be recognised in the data through e.g. social security numbers. Researchers are allowed to work with the data on a secure platform on Statistics Denmark's server.

Wages are measured as an employee's total annual salary, measured in 2010-price level. A job switch is defined as a job change out of the municipality. Internal job changes within the municipality sector within each municipality are not considered as a job change. Hence, a job switch occurs if the individual leaves municipal employment entirely (for a job in the private sector or for the state or a region) or leaves the municipality to continue employment in another municipality. Municipalities are defined as the "new" municipalities, both before and after the reform, to avoid (incorrectly) measuring the continuation of the same job in a newly merged municipality as a job switch. This also allows for measuring job switches over the entire period.

As the third outcome of interest, the paper analyzes entry into the publicly financed Early Retirement (ERP) Scheme. ERP was introduced in 1979 as a labor market programme aimed at reducing unemployment and facilitating early retirement for worn-out workers. [3] In the period under study, ERP was available for groups aged 60-64 while universal public old age pension was universally available from the age of 65. As in Danø et al. (2005), ERP is in this paper regarded as the only general route to *voluntary* early retirement for the 60-64 year-olds.[4]

The development in the three outcomes by treatment status during the period of investigation is illustrated in figure 1. The selected sample is individuals who working in a municipality in 2003 and who did not retire or change job during that year. Wage levels were lower in the municipalities that later merged in 2003, job exit and early retirement shares were 0 by definition due to the selection of the sample. In 2004, the year the reform was decided, job change and early retirement probabilities were lower in the merged municipalities. Over the period 2004-2009, the gap in job exits rose between these two groups, while early retirement probabilities approached each other.

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[3] The ERP scheme was reformed in 1980, 1992, 1999, and 2011 – the two latter reforms with the purpose of creating incentives for workers to postpone early retirement (Bingley et al. 2011). Simultaneously, the formal pension age for ordinary public pensions was reduced from 67 to 65. Eligibility for the ERP programme requires membership of an unemployment insurance scheme for a certain period of time. Until 1999, the required eligibility period was 20 years, and from mid-1999 the required membership period is 25 out of 30 years. The annual ERP rate is around 20,600 Euro if ERP is chosen at the age of 60 and 22,700 Euro if ERP is entered at 62 or older. Individuals on ERP are allowed to work for a maximum number of hours per year; labor income and private pensions are partially deducted from the ERP rate.

[4] One other main early exit option exists, namely the Social Disability Pension. However, granting of a Social Disability Pension requires that a medical or other expert declares that the individual is unable to work due to health or social reasons. Since 2003, disability pension has only been granted to people who have experienced a significant decrease in their occupational skills, thus being considered unable to become self-supporting in the future. In contrast, ERP is not awarded on the basis of health conditions, but depends on the degree of labor market participation and unemployment insurance membership.

**Figure 1. Wages, job switches, and early retirement by treatment status**
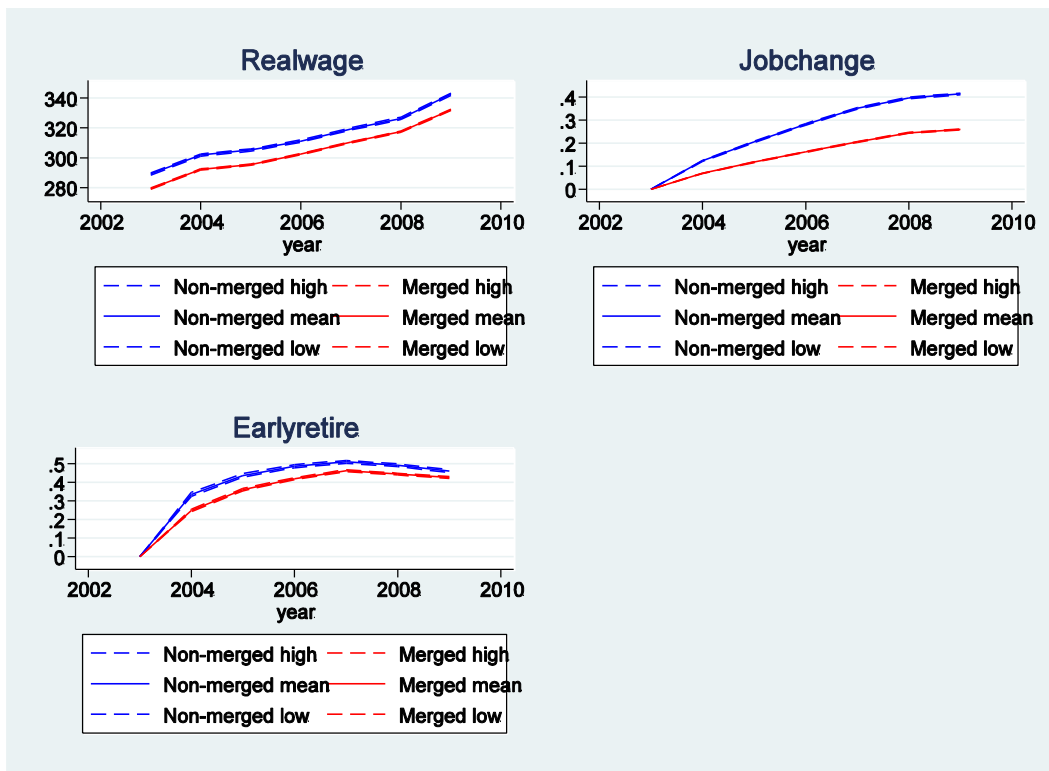


Note: Figures show annual means with 5 percent confidence intervals ("high" and "low"). Real wages are measured in thousand DKK, 2010 price level.

# 6. Empirical Method

## 6.1. *Difference-in-Differences estimation*

The effect of the mergers on labor outcomes are estimated using a Difference-in-Differences (DID) framework (see e.g., Blundell and Costa Dias, 2008; Imbens and Wooldridge, 2009; Wooldridge, 2010). The idea is to compare the development in individual labor market outcomes over time across municipal employees in the merged versus non-merged municipalities. A crucial assumption behind the DID model is that the decision of municipalities to merge was not affected by (expected) changes in the three outcomes (i.e. job change, wages, and early retirement). This assumption seems plausible as the decision to merge or not was primarily the result of a political process with the primary aim of achieving quality improvements in public service provision and economies of scale in municipalities. As the reform was announced in mid-2004, and the new municipal maps were established already in the summer of 2005, part of the adjustment to the new organizations and work practices had presumably already taken place in the years preceding the

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actual merger in 2007. Having access to annual panel data allows us to investigate the dynamics of the reform, including whether expectations and preparations for the forthcoming merger already had effects on individual labor market outcomes in the years up to the formal restructuring of the municipal organizations. Equation (1) shows the estimation model in levels:

$$outcome_{it} = \theta_0 + Z_{it}\gamma + \beta merge_i * D_t + D_t + \varepsilon_i + u_{it} \tag{1}$$

$outcome_{it}$ denotes individual $i$'s labor market outcomes (wages, job change and early retirement) in year $t$. $Z_{it}$ denotes a vector of municipal level and individual level control variables measured in each of the years. $\gamma$ captures the effect of the differences between individuals in merged and non-merged municipalities, including variation across municipalities in the composition of their employees. $merge_i$ is an indicator measuring whether the individual worked in a merged or an unmerged municipality prior to the reform (*merge* is measured for each individual in the base year, 2003).[5] $D_t$ symbolizes year dummies, and these are interacted with the dummy *merge* to analyze the effects of being merged year-by-year. Consequently, $\beta$, the primary variable of interest, is the effect of being merged in year $t$. $\varepsilon_i$ symbolizes unobserved individual heterogeneity, for example (time-constant) individual characteristics as unobserved ability and social skills that are correlated with individual labor market outcomes. $u_{it}$ is white noise. Estimating (1) by panel data estimation eliminates bias from unobserved heterogeneity. I use the within-estimator allowing for the individual fixed-effects to be correlated with the explanatory variables.

When analysing the effects of merger for different subgroups, I use an alternative version of the DID model formulated in first-differences as this more simple specification makes it easier to interact the indicator for being merged with different individual characteristics as e.g. age, education etc. More formally, the DID model in first-differences has the following form:

$$\Delta outcome_i = \beta_0 + \Delta Z_i \gamma + \beta merge_i + \Delta u_i \tag{2}$$

where $\Delta outcome_i$ denotes the change in each of the three labor market outcomes – wages, job change and early retirement – from 2003 to 2009. Model (2) is estimated by OLS.

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[5] In appendix 1 we investigated whether there was a pre-treatment difference between amalgamated and non-amalgamated local governments. We find that, conditional on the controls, there are only for two of the outcomes significant differences between the treatment and control group in 2003, and the differences are moderate in size.

## 6.2. *Inverse Probability Weighting*

Estimating model (2) by OLS in the first-differenced DID specification above implicitly assumes that the error term $\Delta u_i$ is orthogonal to $(\Delta Z_i, merge_i)$. One may, however, speculate whether employees in merged versus non-merged local municipalities were systematically different measured by observed as well as unobserved characteristics at the onset of the reform (i.e. *levels* of $Z_i$), and whether such differences in the composition of the work force are correlated with *changes over time* in the left- and right-hand side variables in the empirical model. For example, unobserved heterogeneity in preferences for leisure, ability or time preferences may not only explain individual selection into certain types of municipalities before the reform, but also affect wage profiles, job change behavior or exit into retirement. The concern that employees in merged municipalities are different from employees in non-merged municipalities is investigated further by conducting additional estimations using Inverse Probability Weighting (IPW). IPW methods rely on modelling the selection based on observable characteristics, assuming implicitly that unobservable characteristics are sufficiently correlated with observables. The advantage of IPW estimators is that they balance the covariates across individuals in the treatment and the control groups. IPW estimators are related to matching methods, which have previously been applied in the context of analysing an organizational reform by Lassen and Serritslev (2011) and Gaynor et al. (2012).[6] The extent of bias reduction depends on how well selection into treatment is captured by observable characteristics. Inverse Probabability Weighting (IPW) is particularly useful when evaluating multi-valued treatment effects, but can also be used for standard binary treatments (Cattaneo, 2010). The idea behind IPW estimators is that they assign weights to observations based on the inverse of their propensity score to reduce the difference in individual observed characteristics across the treated and non-treated controls. As it is the case with usual matching estimators, IPW relies on adequate common support across treatment and control group.[7] IPW estimators allow for clustering standard errors at the municipality level.[8] I use inverse-probability-weighted regression adjustment

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[6] Smith and Todd (2005) demonstrate that propensity score matching is useful by reducing the bias imposed by selection into treatment.

[7] IPW estimators rely on modeling selection in terms of observed confounders (Hogan and Lancaster, 2004) using probit or logit. Hirano, Imbens and Ridder (2003) show that there may be efficiency gains by using a method which weights the observations by the inverse of a nonparametric estimate of the propensity score rather than the true propensity score.

[8] Standard propensity score matching methods in software packages such as Stata 13 do not facilitate a simple estimation of estimate cluster-robust standard errors, as the bootstrapping method cannot be applied with PSM, see Abadie and Imbens (2008, 2011).

estimator (IPWRA, part of what is sometimes named "Wooldridge's double-robust" estimators, Wooldridge 2007; 2010).

# 7. Results

## *7.1.  DID model using entire panel*

We first look at the dynamics of the merger over the entire transition period 2003-09. Table 2 presents the results of estimating model (1) using fixed effects estimation. As previously noted, the sample consists of individuals working for a municipality in 2003. All estimations control for personal individual characteristics as gender, age, education, having children and having a high level position in the municipality. When analysing wage and job changes, I focus on a subsample of employees under 54 in 2003, and when looking at the probability of early retirement, only employees being eligible for early retirement, i.e. employees between 60 and 64 years of age, are included. Standard errors are clustered at municipality level.

The results shown in table 2 point to some interesting dynamics in the transition from many to fewer municipalities. Results in column (1) show that there was a positive difference between wages in merged and non-merged municipalities in all the years 2004-2009, but this effect was larger in 2006-2008, the years immediately before and after the mergers came into effect. Thus, estimation results in column (1) corroborate hypothesis H1. As pre-reform wages were lower in the municipalities that later merged, wage levels approached each other across merged and non-merged municipalities after the reform.

**Table 2: DID using entire panel, fixed effects estimation**

| | (1) ln real wage, all employees | (2) Job change, 2003-09 | (3) Job change, 2004-09 | (4) ln real wage municipal employees | (5) ln real wage, "stayers" | (6) Early retirement 2003-09 | (7) Early retirement 2004-09 |
|---|---|---|---|---|---|---|---|
| Merge*D2004 | 0.0058** | -0.0275*** | - | 0.0031* | 0.0021 | -0.0216 | - |
| | (2.75) | (4.31) | - | (2.02) | (1.23) | (1.24) | - |
| Merge*D2005 | 0.0063* | -0.0361*** | -0.0095* | 0.0044* | 0.0018 | 0.009 | 0.0305** |
| | (2.55) | (3.55) | (2.06) | (2.51) | (0.93) | (0.67) | (2.81) |
| Merge*D2006 | 0.0143*** | -0.0478*** | -0.0216*** | 0.0123*** | 0.0083*** | 0.0268 | 0.0479* |
| | (5.42) | (4.54) | (3.61) | (6.23) | (4.05) | (1.46) | (2.12) |
| Merge*D2007 | 0.0151*** | -0.0510*** | -0.0251** | 0.0150*** | 0.0102*** | 0.0554* | 0.0771* |
| | (6.71) | (4.23) | (2.72) | (6.64) | (4.33) | (2.08) | (2.32) |
| Merge*D2008 | 0.0163*** | -0.0412** | -0.0153 | 0.0195*** | 0.0180*** | 0.063$^{(*)}$ | 0.0843* |
| | (4.45) | (3.06) | (1.31) | (6.38) | (5.48) | (1.83) | (2.03) |
| Merge*D2009 | 0.0098** | -0.0369* | -0.0115 | 0.0121*** | 0.0127*** | 0.0826$^{(*)}$ | 0.1037* |
| | (2.64) | (2.44) | (0.85) | (3.82) | (4.97) | (1.96) | (2.09) |
| Age | 0.0763*** | 0.1283*** | 0.0997*** | 0.0707*** | 0.0672*** | 0.9176*** | 0.9947*** |
| | (46.02) | (32.16) | (20.76) | (61.98) | (83.17) | (16.51) | (19.44) |
| Age Squared | -5.9953*** | -8.6294*** | -6.5655*** | -5.2720*** | -4.8326*** | -64.5815*** | -73.3176*** |
| | (39.68) | (32.81) | (21.21) | (50.99) | (52.79) | (13.69) | (16.62) |
| lag Married | -0.0223*** | 0.0073*** | 0.0048* | -0.0251*** | -0.0215*** | 0.0336*** | 0.0350*** |
| | (14.99) | (4.11) | (2.51) | (18.95) | (17.12) | (5.14) | (5.10) |
| lag Dummy children | -0.0163*** | -0.0009 | -0.0026 | -0.0180*** | -0.0167*** | 0.0358*** | 0.0307*** |

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| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | (12.98) | (0.55) | (1.52) | (15.22) | (16.65) | (5.14) | (4.37) |
| lag Dummy college+ | 0.3861*** | 0.0707*** | -0.0055 | 0.3573*** | 0.3081*** | -0.0097 | -0.0136 |
| | (41.66) | (10.54) | (0.75) | (42.74) | (33.79) | (0.36) | (0.51) |
| lag Position top | 0.1028*** | -0.0276*** | -0.0238*** | 0.0943*** | 0.0694*** | -0.0689*** | -0.0467*** |
| | (29.67) | (7.49) | (6.65) | (30.00) | (26.38) | (10.74) | (8.15) |
| Constant | 10.2408*** | -3.8264*** | -2.9248*** | 10.3567*** | 10.4457*** | -31.7394*** | -33.1098*** |
| | (217.59) | (28.99) | (18.47) | (304.12) | (475.44) | (19.10) | (21.58) |
| Year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-Square | 0.09 | 0.158 | 0.099 | 0.107 | 0.112 | 0.251 | 0.201 |
| Number of observations | 1,718,493 | 1,718,493 | 1458328 | 1,560,418 | 1,417,082 | 274,308 | 260,080 |

Note: t statistics in parentheses. (*) $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered at municipality level.

Early retirement and job change were zero in 2003 by definition/selection.

Results in column (2) show that job shifts were lower in municipalities that merged, with the strongest effects around the time of the merger, i.e. 2006-2007, consistent with hypothesis H2. The negative impact on job change probability is perhaps not so surprising as municipalities that merged issued a two-year job guarantee after the reform in 2007. Thus, while layoffs became a reality in the private sector just after the onset of the financial crisis in 2008, employees in the merged municipalities were guaranteed to keep their job until the end of 2008. This may have impacted their propensity to seek a new job. Moreover, as municipalities that merged also increased in size, the new municipalities provided the employees with a new and larger labor market with more job change options within the municipality than before the merger. 2009 was the first year after the end of the two-year job guarantee, suggesting that employees in the merging municipalities perhaps decided to wait-and-see during the period of the job guarantee. As job changes were 0 in 2003 by definition, I also estimate the effects of mergers on job change probabilities without including year 2003. The results of the estimation on the period 2004-2009 are shown in column (4). These results back up hypothesis H2.

Given that working in a merged municipality seems to impact not only on wages, but also on job shifts, I show in column (4) the wage changes for employees who stayed in the municipality sector (to deselect those that changed to the private sector or to other parts of the public sector), while column (5) shows wage change effects for those who stayed on in the same (merged) municipality throughout the period (the compliers). Thus, the estimation results presented in column (5) allows us to investigate whether municipalities raised wages for the employees that stayed on through the reform (rent sharing). Generally, I find that there were positive wage effects for employees in the merged municipalities suggesting that merging municipalities were conscious about providing incentives for their employees to stay on also in the transition period, consistent with the rent sharing hypothesis (H1).

Columns (5)-(6) shows the results for the employees eligible for early retirement. As the sample was selected among people who were employed in 2003, exits to early retirement were per definition 0 in 2003. Column (6) shows the results for the entire period 2003-2009, i.e. including 2003, while column (7) focuses on the period 2004-2009, thus excluding 2003 where everyone worked. The results for early retirement show a positive effect on early retirement around the time of the reform. The positive impact of being merged on the probability of early retirement is consistent with hypothesis H3. This confirms the idea that some older

employees find it tiresome having to move work place, adjusting to new work practices, a new management etc. in a new merged municipality and may instead prefer early retirement. Early retirement take-up was lower in the merged municipalities before the reform, but approached the level of the merged municipalities in the period 2004-2009.

All in all, the panel data estimations show fairly stable dynamics of the changes over the entire period. Changes in the three outcomes observed were fairly stable over the period, with the strongest effects around the implementation of the reform in 2006-2007. As the first-differenced model is simpler to explore and interpret in a treatment effects setting (using IPW), I focus on the first-differenced model in equation (2) in the following.

## 7.2. *OLS results, first-differenced model*

The DID model presented in (2) estimates the results of the effects of municipality mergers on changes in wages, exit from employment in the municipality and early retirement using OLS. The results in table 3 suggest the following. First, results in column (1) confirm the findings found in table 2, namely a positive effect on wages for employees from merged municipalities of around 1.2 percent. Second, job exit probabilities were lower for employees in merged municipalities (column (2)). Focusing in column (3) on wage changes for "stayers", i.e. employees that stayed in the (new) merged municipality over the entire period 2003-09, leads to similar findings as the full sample in column (1) and the results using fixed effects estimation in table 2. Third, there is a significant and positive association between municipality mergers and exit to early retirement (column 4). While on average around 40 percent of the initial sample of older workers had gone into early retirement by 2009, belonging in 2003 to a municipality that would subsequently merge was associated with a 4.5 percent higher retirement probability in 2009.

When studying the effect of merger versus no-merger, one implicitly assumes that employees are sensitive towards whether they worked in a municipality that merged or not. One may, however, alternatively speculate that employees are rather affected by the degree of the merger, and that this in turn depends on the "power" of their "old" municipality in the new, merged, organization.[9] In fact, as reflected in hypothesis H6, employee reactions are perhaps more likely to depend on whether the employee came from a

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[9] Dahl (2011) discusses the impact of the "degree of change" and "breadth of change" for employees in private sector firms undergoing organizational change.

large municipality that dominates in the new merged municipality, or whether the employee came from a smaller municipality that had maybe limited influence in the merger process. I investigate this question by distinguishing between municipalities being the "big brother" in the new merged municipality and municipalities that were not (being instead the "younger brother", i.e. from a small municipality that was absorbed and dominated in the new municipality construction). Big brother municipalities are defined by their population share in the new municipality. Columns 5-8 illustrates the difference in the reactions to a municipal merger depending on whether the municipalities that merged took a dominating position in the new municipality (being a "big brother") or not. The estimation results suggest a stronger and more negative reaction of the big brother municipality workers on the job change probability, implying that employees from the dominating municipalities were less prone to change jobs than other employees in the new municipality. This finding may suggest that employees in "big brother" municipalities felt more secure and optimistic about their future options in the new organization structure.

However, "big brother" employees also had a relatively lower rise in wages that "small brother" employees. This suggests perhaps that the mergers levelled out wages across employees in two dimensions: 1) *across* merged versus non-merged municipalities, and 2) *within* merged municipalities.

**Table 3: Difference in outcomes over 2003-98, OLS estimation results**

| | (1) ln real wage, all employees | (2) Job change | (3) ln real wage, "stayers" | (4) Early retirement | (5) ln real wage, all employees | (6) Job change | (7) ln real wage, "stayers" | (8) Early retirement |
|---|---|---|---|---|---|---|---|---|
| Merged | 0.0122*** | -0.0699*** | 0.0118*** | 0.0453*** | 0.0133*** | -0.0516* | 0.0163*** | 0.0477*** |
| | (4.73) | (3.38) | (4.53) | (3.63) | (4.72) | (2.54) | (6.14) | (3.78) |
| Merged * Big brother | | | | | -0.002 | -0.0321** | -0.0078*** | -0.0043 |
| | | | | | (0.94) | (2.90) | (3.49) | (0.52) |
| Ch. Age squared | -5.8032*** | -9.1865*** | -5.1250*** | -15.0540*** | -5.8043*** | -9.2037*** | -5.1311*** | -15.0470*** |
| | (43.40) | (42.17) | (51.83) | (12.58) | (43.35) | (42.85) | (51.76) | (12.54) |
| Ch. Dummy married | -0.0225*** | 0.0100*** | -0.0282*** | 0.0401*** | -0.0225*** | 0.0102*** | -0.0281*** | 0.0401*** |
| | (10.21) | (3.44) | (12.97) | (5.33) | (10.21) | (3.54) | (12.95) | (5.33) |
| Ch. Dummy children | -0.0067*** | 0.0065* | -0.0080*** | 0.1129*** | -0.0067*** | 0.0070* | -0.0079*** | 0.1130*** |
| | (3.69) | (2.33) | (4.83) | (20.78) | (3.67) | (2.52) | (4.76) | (20.84) |
| Ch. Dummy college+ | 0.4026*** | 0.1440*** | 0.3393*** | -0.0892** | 0.4026*** | 0.1441*** | 0.3394*** | -0.0891** |
| | (40.99) | (18.87) | (32.65) | (2.83) | (40.99) | (18.93) | (32.67) | (2.82) |
| Ch. Dummy Top position | 0.1241*** | -0.0159** | 0.0895*** | -0.0920*** | 0.1241*** | -0.0157** | 0.0895*** | -0.0919*** |
| | (24.37) | (2.81) | (23.18) | (12.86) | (24.37) | (2.77) | (23.18) | (12.86) |
| Constant | 0.4553*** | 0.8363*** | 0.4282*** | 1.4581*** | 0.4554*** | 0.8373*** | 0.4286*** | 1.4576*** |
| | (58.73) | (32.67) | (83.11) | (16.58) | (58.67) | (32.79) | (82.92) | (16.53) |
| Adj. R-Square | 0.093 | 0.05 | 0.086 | 0.014 | 0.093 | 0.051 | 0.086 | 0.014 |
| Number of observations | 231,085 | 231,085 | 163,267 | 51,760 | 231,085 | 231,085 | 163,267 | 51,760 |

Note: t statistics in parentheses. (*) $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors clustered at municipality level.

### 7.3. *Heterogeneity in parameters depending on education and position*

I initially hypothesized (hypothesis H5) that employees with relatively more formal education (general human capital measured by college and above) do relatively better in adapting to organizational change. This hypothesis is examined further in table 4. Columns (1)-(4) investigate whether there is heterogeneity in the reactions depending on education level, and columns (5)-(8) compare reactions depending on job level of the employee ("top level" worker is a dummy for position as a manager or with a highly specialized position[10]). Surprisingly perhaps, the interaction between the merger dummy and the dummy for having college and above experience is only significant for the wage effect in column (1).

Columns 5-8 look at heterogeneity depending on whether the employee had a "Top level" position in the hierarchy (defined here as employees with management responsibilities or higher-level public employees). The interaction between the dummy for merger and being top level employee is positive and significant for wages, suggesting that top level public employees enjoyed higher wage rises than other municipal workers.

All in all, I cannot generally confirm hypothesis H5 regarding heterogeneity in responses based on education. One potential explanation for not finding support for skill-bias in the effects if the rather short time horizon of this analysis. Looking instead at human capital measured by level in the organization, I find some evidence that top level employees reduced job exits less and experienced higher wages than other employees.

### 7.4. *Heterogeneity in parameter estimates depending on age*

We next turn to investigate whether age had implications for the employee reactions to mergers as suggested by hypothesis H4. As early retirement is confined to a fairly short age span of the 60-64 year-olds, it does not make sense to inspect age heterogeneity of early retirement responses. Table 5 thus investigates the heterogeneity in effects on wages and job change probabilities depending on age. The sample is split in two: younger employees (up to 44 years of age, corresponding to 54 percent of the sample) and older

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[10] Top level employees are defined from the register data as employees with the variable PSTILL =31 or 32, i.e. directors, managers, or superior salaried employees.

employees (45 years old and above, corresponding to 46 percent of the sample). Generally, older employees reduce their job exits to a smaller extent than younger employees.

Finally, I interacted the dummy for merger with the dummies for having college and above and the dummy for being among the oldest of the employees. The results show no heterogeneity in wage responses for "stayers", but the results do indicate that among the younger employees with more education, job shifts were decreased even more than the average employee, while older employees, and especially older employees with college and above did not reduce their job exits to the same degree as the average employee.

**Table 4: OLS estimation results with interactions for education and position in organization**

| | (1) ln real wage, all employees | (2) Job change | (3) ln real wage, "stayers" | (4) Early retirement | (5) ln real wage, all employees | (6) Job change | (7) ln real wage, "stayers" | (8) Early retirement |
|---|---|---|---|---|---|---|---|---|
| Merged | 0.0176*** | -0.0612*** | 0.0127*** | 0.0444** | 0.0153*** | -0.0735*** | 0.0145*** | 0.0444** |
| | (5.50) | (3.60) | (4.02) | (2.94) | (4.83) | (3.58) | (4.83) | (3.13) |
| Merged * College+ | -0.0125** | -0.0197 | -0.0025 | -0.0129 | | | | |
| | (3.16) | (1.64) | (0.62) | (0.90) | | | | |
| College+ | 0.0005 | 0.0076 | -0.0110** | -0.1408*** | | | | |
| | (0.14) | (0.66) | (2.98) | (11.34) | | | | |
| Merged * Top | | | | | -0.0122** | 0.0132 | -0.0105* | -0.0223 |
| | | | | | (2.90) | (1.34) | (2.51) | (1.42) |
| Dummy Top position | | | | | 0.0095** | -0.0200* | -0.0001 | -0.2844*** |
| | | | | | (2.81) | (2.16) | (0.02) | (19.84) |
| Ch. Age squared | -5.8083*** | -9.1961*** | -5.1208*** | -17.9309*** | -5.8089*** | -9.1461*** | -5.0958*** | -21.5247*** |
| | (43.42) | (41.96) | (52.31) | (14.84) | (43.25) | (42.76) | (52.16) | (17.11) |
| Ch. Dummy married | -0.0223*** | 0.0101*** | -0.0278*** | 0.0465*** | -0.0226*** | 0.0102*** | -0.0280*** | 0.0502*** |
| | (10.15) | (3.51) | (12.81) | (6.31) | (10.26) | (3.51) | (12.89) | (7.14) |
| Ch. Dummy children | -0.0061*** | 0.0070* | -0.0072*** | 0.0957*** | -0.0068*** | 0.0071* | -0.0077*** | 0.0842*** |
| | (3.34) | (2.58) | (4.42) | (17.28) | (3.69) | (2.52) | (4.65) | (15.21) |
| Ch. Dummy college+ | 0.4019*** | 0.1436*** | 0.3383*** | -0.1313*** | 0.4026*** | 0.1443*** | 0.3397*** | -0.0934** |
| | (40.93) | (18.30) | (32.70) | (4.12) | (40.96) | (18.99) | (32.62) | (3.05) |
| Ch. Dummy Top position | 0.1239*** | -0.0161** | 0.0901*** | -0.1643*** | 0.1243*** | -0.0188** | 0.0875*** | -0.3094*** |
| | (24.25) | (2.86) | (23.18) | (21.92) | (23.96) | (3.19) | (22.49) | (38.34) |
| Constant | 0.4554*** | 0.8334*** | 0.4332*** | 1.7099*** | 0.4532*** | 0.8394*** | 0.4267*** | 1.9783*** |
| | (56.89) | (37.16) | (72.13) | (19.09) | (55.79) | (33.14) | (77.91) | (21.31) |
| Adj. R-Square | 0.094 | 0.05 | 0.087 | 0.032 | 0.093 | 0.05 | 0.086 | 0.064 |
| Number of observations | 231,085 | 231,085 | 163,267 | 51,760 | 231,085 | 231,085 | 163,267 | 51,760 |

Note: t statistics in parentheses. (*) p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001. Standard errors clustered at municipality level.

**Table 5: OLS estimation results – age effects and age combined with education**

| | (1) ln real wage, all employees | (2) Job change | (3) ln real wage, "stayers" | (4) ln real wage, all employees | (5) Job change | (6) ln real wage, "stayers" |
|---|---|---|---|---|---|---|
| Merged | 0.007 | -0.0996*** | 0.0092** | 0.0133* | -0.0823*** | 0.0087 |
| | (1.45) | (3.72) | (2.82) | (2.38) | (3.55) | (1.39) |
| Merged * 40+ | 0.0088 | 0.0500*** | 0.004 | 0.006 | 0.0355** | 0.0053 |
| | (1.51) | (4.20) | (0.94) | (0.92) | (2.79) | (0.75) |
| Dummy 40+ | -0.0118* | -0.0361** | -0.0072 | 0.0005 | -0.0270* | 0.0022 |
| | (2.47) | (3.03) | (1.75) | (0.10) | (2.17) | (0.34) |
| Merged * College+ | | | | -0.0138* | -0.0378** | 0.001 |
| | | | | (1.98) | (2.91) | (0.11) |
| College+ | | | | 0.0176** | 0.0186 | 0.005 |
| | | | | (2.85) | (1.54) | (0.62) |
| Merged * 40+ * College+ | | | | 0.0043 | 0.0310*** | -0.0047 |
| | | | | (0.53) | (3.34) | (0.55) |
| Dummy 40+ * College+ | | | | -0.0295*** | -0.0189* | -0.0245** |
| | | | | (4.01) | (2.37) | (3.17) |
| Ch. Age squared | -5.5716*** | -9.2218*** | -4.9454*** | -5.5033*** | -9.2376*** | -4.8374*** |
| | (31.86) | (36.32) | (34.96) | (31.38) | (36.53) | (33.80) |
| Ch. Dummy married | -0.0226*** | 0.0095** | -0.0282*** | -0.0226*** | 0.0096*** | -0.0280*** |
| | (10.26) | (3.29) | (13.03) | (10.31) | (3.36) | (12.92) |
| Ch. Dummy children | -0.0069*** | 0.0066* | -0.0081*** | -0.0071*** | 0.0071** | -0.0081*** |
| | (3.77) | (2.36) | (4.90) | (3.79) | (2.64) | (4.97) |
| Ch. Dummy college+ | 0.4026*** | 0.1422*** | 0.3394*** | 0.4025*** | 0.1420*** | 0.3383*** |
| | (41.59) | (19.11) | (32.67) | (41.37) | (18.52) | (32.66) |
| Ch. Dummy Top position | 0.1240*** | -0.0160** | 0.0895*** | 0.1236*** | -0.0162** | 0.0898*** |
| | (24.41) | (2.84) | (23.17) | (24.26) | (2.88) | (23.08) |
| Constant | 0.4499*** | 0.8592*** | 0.4231*** | 0.4389*** | 0.8513*** | 0.4162*** |
| | (44.21) | (31.40) | (60.44) | (43.46) | (34.39) | (45.92) |
| Adj. R-Square | 0.093 | 0.05 | 0.086 | 0.094 | 0.051 | 0.087 |
| Number of observations | 231,085 | 231,085 | 163,267 | 231,085 | 231,085 | 163,267 |

Note: t statistics in parentheses. (*) p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001. Standard errors clustered at municipality level.

## 7.5. *Inverse Probability Weighting*

As discussed in section 6.2 in the *Methods* section, one may be concerned that employees in the merged municipalities are generally different measured by (observed as well as unobserved) individual characteristics, and that such differences impact the estimated effects. To investigate this further and thus reduce the discrepancy in observed characteristics across the treatment and control group, the first-differenced model (equation (2)) is estimated using Inverse Probability Weighting estimation (IPW). Selection into being an employee in a merged municipality is modelled as a function of individual characteristics (in levels) as age, gender, education, having children, being married and having a top position. The predicted propensity score, which is estimated by logit (see table A2 in Appendix 1), is used as a weight in the regression.

Table 6 shows the estimation results when using Inverse Probability Weighting. As mentioned, one of the advantages of using Inverse Probability Weighting rather than conventional Propensity Score Matching is that we can use bootstrap with this estimation procedure to produce cluster-robust standard errors. Overall, the IPW estimation results are of similar size and sign as the results using OLS. We can therefore infer that even though employees were significantly different in terms of observable characteristics in 2003, this does not impact the effects of mergers over time.

**Table 6: Inverse Probability Weighting (IPW)**

|  | (1) ln real wage, all employees | (2) Jobchange | (3) ln real wage, "stayers" | (4) Early retirement |
|---|---|---|---|---|
| ATET, Merged | 0.0119*** | -0.0614** | 0.0103*** | 0.0345** |
|  | '(4.65) | '(3.27) | '(3.75) | '(3.02) |
| Number of observations | 231,085 | 231,085 | 163,267 | 51,760 |

Note: t statistics in parentheses. (*) p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001. Standard errors for IPW are based on bootstrapping with 100 replications and clustered at the municipality level. Control variables: Gender, age, age squared, dummy for married, dummy for having younger children, education dummies, dummy for high level position.

The overlap assumption – i.e. whether there is sufficient overlap between the treated and untreated individuals – can be checked by plotting the estimated densities of the probability of belonging to a merged

versus non-merged municipality in figure A.2. By simple inspection of the graphs, the two estimated densities overlap fairly well in their distributions, suggesting that there is sufficient common support between the treated (merged) municipalities and the controls (unmerged municipalities), so that the overlap assumption is not violated.

### 7.6. *Heterogeneous effects*

As discussed in the theory section and as further suggested by the OLS estimates, the effects of organizational change may vary across different groups of employees depending on age and education. Therefore, although the average effects on labor market outcomes are on average modest, this can conceal heterogeneous responses that balance out on average. This question is explored further below by exploiting the predicted individual effects of the reform produced by the IPWRA estimator. This approach allows us to further explore whether there are systematic differences in the responses across different groups in the population, see e.g. Leth-Petersen (2010) for a previous similar application. By regressing non-parametrically the predicted effects on age, one can investigate whether and how the individual effects on wages and job change depend on age. Figure 2 depicts that individual effects are rising with age for both real wages and job change probabilities. For real wages, the effects are positive for all age groups, but larger for the older than the younger employees. For job change, the effects are negative for all age groups, but numerically larger for the younger employees.

**Figure 2. Kernel regressions of estimated salary and job change effect of the reform by age**

| Eff_Realwage | Eff_Jobchange |
|---|---|
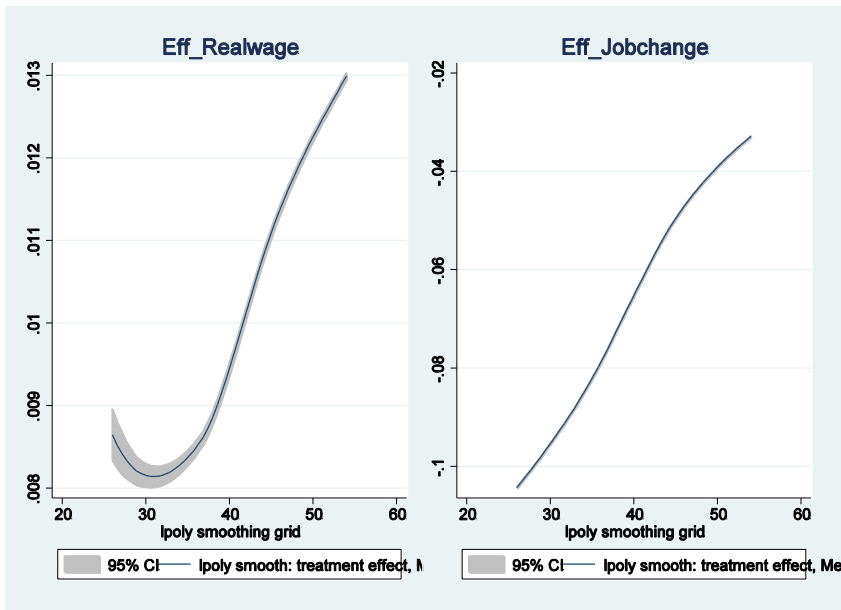
Note: The kernel regressions presented in both panels are over-smoothed relative to the gross validated level. Standard errors are not clustered at municipality level.

## 8. Discussion and conclusion

This paper investigates the implications of a large reform on the municipality structure in 2007. The empirical analyses use a unique dataset consisting of an almost full sample of municipality employees from a Danish panel dataset. The paper contributes to the literature on organizational change by identifying the causal effects of a merger on employee wages, job change frequency and probability of early retirement. The paper employs a number of different approaches to identifying the causal effects of merger. The chore estimation strategy builds on the idea that the municipality mergers can be seen as a quasi-experiment in that the reform and the subsequent mergers were exogenously determined and largely unexpected. If exogeneity holds, as assumed, OLS and Fixed Effects methods are efficient when estimating the effects of the merger on employee labor outcomes.

However, given that one may be concerned that employees in merged municipalities are initially different than employees in municipalities that did not merge, the paper applies a supplementary estimation method from the literature on treatment effects, namely Inverse Probability Weighting (IPW). Reassuringly, the parameter estimates using IPW estimation procedures lead to largely similar results as OLS, suggesting that initial heterogeneity across treated and untreated individuals is unimportant for the effects found.

The most consistent and significant finding is a positive impact on early retirement. Furthermore, job exits were lower, mainly for employees from "big brother" municipalities, suggesting that these employees perhaps found themselves relatively more "safe" in the new organization structure or had greater influence on the organizational changes and the way they would impact their own work situation.

One explanation for the reduction in job search may be found in the observation that the municipality structure reform implied a two-year job guarantee. Moreover, due to the organizational restructuring, many municipalities probably postponed issuing new vacant positions immediately before the mergers in the anticipation of new prospective employees from the municipalities they merged with.

Finally, wages were initially lower in the later merged municipalities, but rose relatively more in the merged municipalities around the time of the merger. This may be interpreted as employers attempting to hold on to their most efficient and qualified employees (rent sharing). All in all, the effects on labor market outcomes were modest effects. This can partially be explained by the fact that municipal employees had a two year job guarantee, that layoffs were seldom in the municipality sector in the period considered, and that part of the downsizing was solved by (voluntary) early retirement.

In contrast to the initial hypothesis on heterogeneity in responses with respect to individual human capital, I find little evidence of skill-bias in the effects, but some evidence of heterogeneity in responses depending on the position in the municipality. However, there is an age-bias in the effects as older employees (40-54 in 2003) experienced higher wage increases. Thus, the gap in job exits of employees in the merged municipalities rose over the period relative to employees from non-merged municipalities, but more so for the younger than the relatively older. This finding is consistent with hypothesis H4.

**Acknowledgements**

# 9. Appendix

**Table A.1. Descriptive statistics**

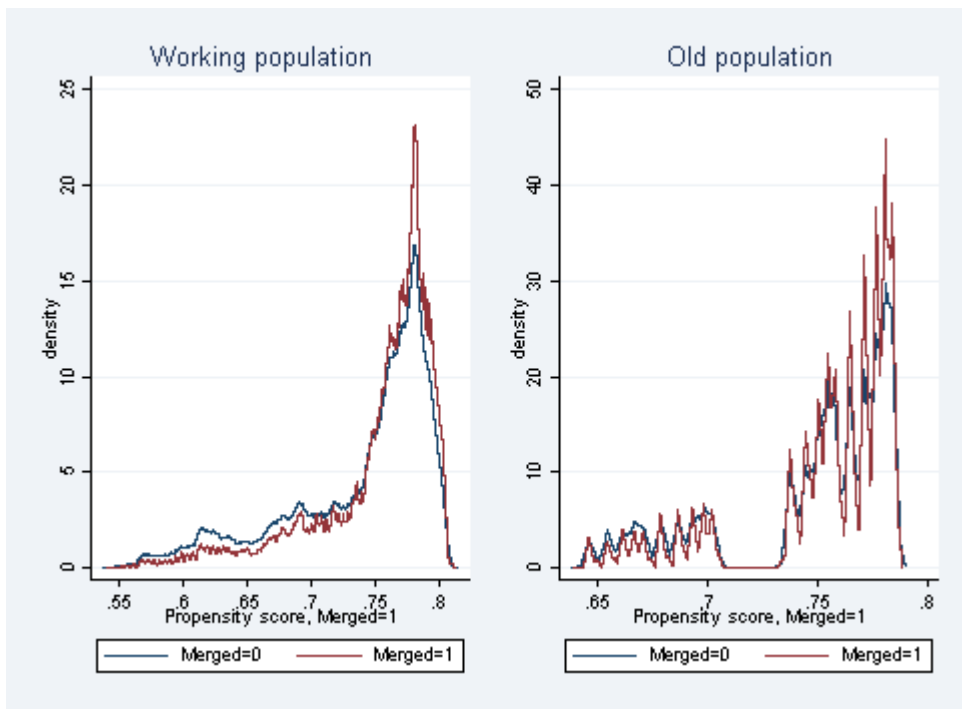| | Employees aged, 25-54 | | | | Older employees, 60-64 | | | |
|---|---|---|---|---|---|---|---|---|
| | 2003 | | 2009 | | 2003 | | 2009 | |
| | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. |
| Real wage | 281,767 | 92,467 | 335,840 | 102,803 | - | - | - | - |
| Change job | 0.000 | 0.000 | 0.301 | 0.459 | - | - | - | - |
| Early retirement | - | - | - | - | 0.000 | 0.000 | 0.428 | 0.495 |
| Age | 41.910 | 7.925 | 47.736 | 7.861 | 57.780 | 2.197 | 62.907 | 1.380 |
| Age squared | 0.182 | 0.065 | 0.234 | 0.074 | 0.334 | 0.026 | 0.396 | 0.017 |
| Dummy married | 0.822 | 0.382 | 0.824 | 0.381 | 0.813 | 0.390 | 0.783 | 0.412 |
| Dummy children | 0.670 | 0.470 | 0.582 | 0.493 | 0.126 | 0.332 | 0.030 | 0.170 |
| Dummy college | 0.416 | 0.493 | 0.460 | 0.498 | 0.370 | 0.483 | 0.373 | 0.484 |
| Dummy top position | 0.235 | 0.424 | 0.246 | 0.431 | 0.295 | 0.456 | 0.143 | 0.350 |
| Number of observations | 260,165 | | 233,797 | | 67,830 | | 56,739 | |

**Table A2. Logit estimation of selection into being merged**

|  | (1)<br>Real wage,<br>all employees | (2)<br>Job<br>change | (3)<br>Real wage,<br>"stayers" | (4)<br>Early<br>retirement |
|---|---|---|---|---|
| Male | -0.1915*** | -0.1915*** | -0.2418*** | 0.0134 |
|  | '(4.90) | '(4.29) | '(5.27) | '(0.28) |
| Age | 0.0332** | 0.0332** | 0.0153 | 0.3215 |
|  | '(2.63) | '(2.71) | '(0.90) | '(0.42) |
| Age squared | -2.5728 | -2.5728 | -1.3889 | -30.6187 |
|  | '(1.68) | '(1.73) | '(0.64) | '(0.45) |
| Dummy married | 0.4681*** | 0.4681*** | 0.4578*** | 0.4303*** |
|  | '(10.35) | '(8.89) | '(7.70) | '(5.78) |
| Dummy children | 0.1379** | 0.1379** | 0.0832* | -0.0184 |
|  | '(3.19) | '(3.18) | '(2.25) | '(0.36) |
| Dummy college+ | -0.1018* | -0.1018* | -0.0884 | -0.1455* |
|  | '(2.45) | '(2.51) | '(1.77) | '(2.52) |
| Dummy Top position | 0.0371 | 0.0371 | 0.0243 | 0.0006 |
|  | '(0.94) | '(0.85) | '(0.51) | '(0.01) |
| Constant | -0.2224 | -0.2224 | 0.4656 | -7.5648 |
|  | '(0.68) | '(0.60) | '(1.22) | '(0.35) |
| Number of observations | 231,085 | 231,085 | 163,267 | 51,760 |

**Figure A.1. MAP OF MUNICIPAL MERGERS IN DENMARK**



32 municiplities did not change municipal borders
225 municipalities merged without changing municipal borders
14 municipalities were divided in relation with the municipal merger
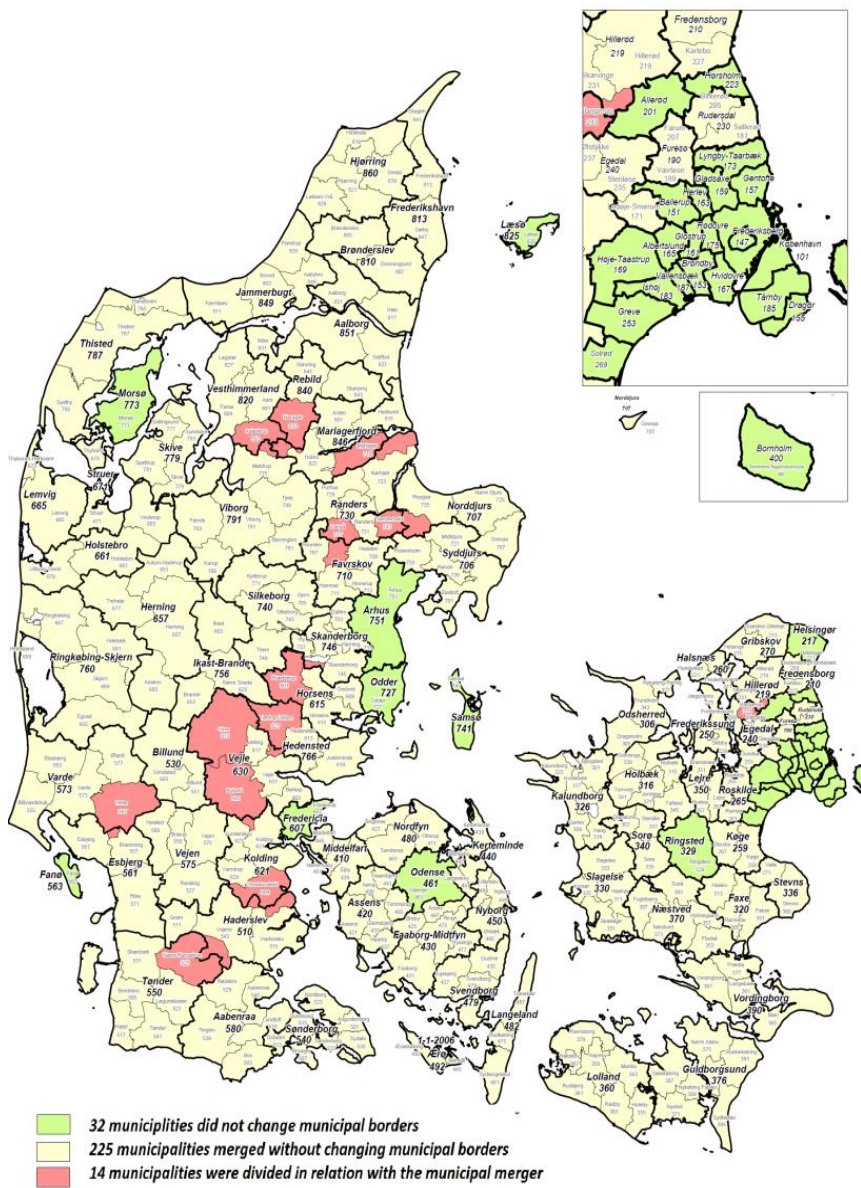
**Figure A.2. DISTRIBUTION OF PROPENSITY SCORES**

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