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The causal effect of profound organizational change when job insecurity is low

– a quasi-experiment analysing municipal mergers

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Abstract

The present article finds that the causal effect of profound organizational change on employee health can be very low, if job insecurity is mitigated. We demonstrate this by investigating a rare case of a large-scale radical public sector reform with low job insecurity, in which a large number of municipalities are merged into larger local governments while other municipalities are not merged. This adds to previous research, which documents that organizational changes following public sector reform impact employee health negatively. We argue that a conceptual distinction between organizational change and job insecurity may explain the divergence from previous results. An important strength of our study is that the reform investigated can be considered a quasi-experiment, as it was exogenous and implemented simultaneously by the affected local governments. We also have access to an objective measure of the extent of organizational change, which is combined with objective measures of health outcomes from administrative data. The unique availability of high-quality longitudinal data combined with an exogenous reform provides a strong research design, which allows us to draw causal inferences. A number of robustness tests are performed, including propensity score matching and in-depth analysis of particular sub-groups of public sector employees. The results indicate that profound organizational change *per se* does not necessarily lead to decreased health, if job insecurity is low. However, a very modest effect on long-term absenteeism is seen for employees coming from very small municipalities that have been merged into the newly formed organizations.

Introduction

Public sector reform has become an ongoing phenomenon, and as a consequence mergers and large-scale reorganizations frequently occur in the public sector (Pollitt and Bouckhart 1999; Pollitt 2013). Unfortunately, organizational changes stemming from public sector reforms put pressure on public sector employees (Noblet and Rodwell 2008). Organizational changes are documented to affect employee health negatively (Ferrie 2001; Vahtera Kivimäki, and Pentti 1997), and this may have negative impacts on public sector performance, because productivity and satisfaction of employees are dependent on their health (Adler et al. 2006; Brenninkmeijer, Houtman, and Blonk 2008; Wang et al. 2004). Hence, performance may be reduced, if job the satisfaction and well-being of employees is negatively influenced by organizational change (see Nelson, Cooper, and Jackson 1995). Thus, there is a need to consider possible factors that might alleviate some of the negative consequences of organizational change.

The argument in this article is that the negative impact on employee health found in the literature could be a consequence of job insecurity rather than a consequence of organizational change as such. In order to investigate this possibility further, it is necessary to start disentangling the two concepts both conceptually and empirically, which this article sets out to do. In relation to recruitment and retainment of employees, low job insecurity is often stated to be a job characteristic that the public sector provides with greater credibility than the private sector (Frank and Lewis 2004; Ritz and Waldner 2011; Baldwin 1991), although job security does not come out clearly as a motivation for public sector employment (Wright 2001; Feeney 2007). If low job insecurity mitigates the negative impact on employee health, it will be possible to carry out sweeping public sector reforms with reduced possible negative impacts on health and performance. Therefore, the aim of this article is to investigate the effects of organizational change on employee health in a highly specific situation, namely when job insecurity is low. Whereas much of the literature in the

field examines organizational change under high job insecurity we examine a rare case of sweeping organizational change when job insecurity is low.

The methodological contribution of the article is to utilize a quasi-experimental design combined with unique data, which allows us to make causal inferences. The pressure for organizational change differs between the public and private sectors. In the public sector, politicians ultimately decide whether organizations survive, whereas private organizations depend on their owners, who are likely to change organizations in response to poor bottom-line figures (Dixit 2002). Consequently, large-scale public sector reform may cause several organizations to undergo change in the same way simultaneously for reasons unrelated to the performance of the organizations in question, thereby providing leverage for researchers. Such a situation occurred in the 2007 local government reform in Denmark. The 2007 reform caused 239 local governments to be merged into 66 new municipalities, which consequently underwent radical organizational change, while 32 local governments remained relatively unchanged. The reform can be considered a quasi-experiment and is therefore an appealing venue for eliminating problems of endogeneity (Lassen and Serritzlew 2011; Blom-Hansen, Houlberg, and Serritzlew 2014). The exogenous change is combined with high-quality administrative panel data from approximately 370,000 individuals working in municipal organizations, allowing us to obtain reliable causal estimates of the effect of the mergers using difference-in-difference estimation. The data contain multiple objective indicators of employee health. Recent research suggests that register data on health care use has some advantages over survey data on health, as self-reports on health often underestimate illness (Datta Gupta and Jürges 2012; Datta Gupta and Larsen 2010). Even more importantly, labour market status seems to matter for misreporting, which is particularly problematic when studying the effect of mergers on health. In consequence, by using register-data on health the fallacies of self-reported data, such as social desirability bias, justification bias or strategic responses are avoided. Moreover, as

individuals' social security number can be used as a key in combining different administrative registers, health can be analysed including important controls from other registers in the analyses. In addition to the advantages of the individual level data, the extent of organizational change can be objectively assessed in a measure that is comparable across local governments. Hence, the research design avoids endogeneity, subjective measures and common-source bias, which are known to be detrimental to studies in the field (Bamberger et al. 2012; Imbens and Wooldridge 2008; Meier and O'Toole 2013).

In sum, the contributions of this study are firstly that we make a theoretical distinction between organizational change and job insecurity and study a case of municipal merger where organizational change is profound but job insecurity is mitigated. Secondly, we utilize a strong research design with a quasi-experimental set-up, behavioural panel data on health outcomes, objective measures on the extent of the organizational changes and a large number of observations encompassing almost the entire population of employees in municipalities. The results show that the effects on health outcomes are very modest. We find no effects on average for any of the seven outcomes, and when analysing heterogeneous effects we find only small detrimental health effects on one of the seven outcomes for employees coming from the smallest municipalities, which constitute a minor share of the new merged organizations.

The remainder of the paper is organized as follows. After this introduction, we discuss the subject matter; organizational change when job insecurity is low, the state of the art and the potential for contributing to the previous literature. Secondly, we present our quasi-experiment, with special attention given to the limited job insecurity in the reform under investigation, and the institutional setting in Danish local governments in the period leading up to and immediately following the municipal mergers in 2007. We then present the data and empirical strategy. Finally, we discuss the results and conclude the paper.

The subject matter: Organizational change and job insecurity

The subject matter in this article is organizational change, but organizational change of a particular form; namely profound changes in a situation of low job insecurity. Organizational change is used as an overarching term, referring to alterations to an organization's structure, processes, social system and/or physical location, and changes in these are expected to influence the behaviour of individuals, this again leading to changes in organizational outcomes (Robertson, Roberts, and Porras 1993). This overall definition of organizational change encompasses large-scale and fundamental transformations, such as privatization, downsizing, mergers and restructurings, as well as smaller changes, such as implementation of new IT systems, moving offices or a change in manager (see Kiefer et al. 2014).

Numerous attempts have been made to theorize the difference between first-order incremental changes and second-order transformative and radical changes (see Levy and Merry 1986, 8). The extent of the change is important, because the more transformative changes are the more they can be expected to influence employee health. Similarly, it is important to distinguish between episodic change and continuous change (Weick and Quinn 1999). In episodic changes, organizations are seen as inertial, and change is seen an occasional, intentional interruption away from equilibrium, which tends to be dramatic and externally driven. In contrast, continuous changes are seen as a redirection of what is already underway, and change is an ongoing practice with endless modifications in work processes and social practices (Weick and Quinn ibid.). Episodic change is associated with planned, intentional change, where existing organizational structures are 'unfrozen' in order for a new equilibrium to be reached. In such cases, the employees have little possibility of accommodating the changes; they are to a large extent forced upon them externally. This type of change is likely to have a more radical effect on the wellbeing of the employees.

Uncertainty and organizational change

The change management literature has drawn attention to the consequences of organizational change for the wellbeing of the employees, the focus primarily being on dysfunctional processes and negative outcomes for employees (Kelman 2005). For instance, it is argued that individuals develop situated roles with automatic responses to work and life events. These roles reduce uncertainty in everyday interactions, but as job demands, job control and social support are likely to change when organizations change uncertainty may increase (Schabracq and Cooper 1998, 637-638; see also Karasek 1979; Karasek and Theorell 1990; Bordia et al. 2004; Ashford 1988). However, even though organizational changes are likely to involve uncertainty, uncertainty comes in different forms, and there is a need to disentangle organizational change from negative job insecurity, both at a conceptual and empirical level.

Job security includes the features of the job situation which lead to assurance of continued employment, either within the same company or in the same profession or type of work (Herzberg, Mausner, and Snyderman 1959, 41). Important distinctions concern whether the anticipated loss is temporary or permanent, and whether the job is lost completely or only particular job features are lost. The latter is considered to be less severe because the organizational membership is not lost, though it is important to acknowledge that even if an employee retains his/her organizational membership, organizational change may involve job insecurity in the form of loss of control over career progress, status, autonomy and resource community (Greenhalgh and Rosenblatt 1984). Thus, in the following we reserve the term job insecurity to describe a situation where the employee dreads losing organizational membership. Thus, the argument is that there is a reason to disentangle organizational change and job insecurity conceptually, as the consequences for the individual may differ. This is interesting, because if the effects of organizational change depend on job insecurity,

increased security may provide an avenue for alleviating some of the detrimental effects of radical organizational change.

We argue that there is a two-dimensional space: On the one hand, organizational changes may be large or small, and on the other hand job insecurity may be high or low. Large-scale organizational changes are found where the changes are transformative, first-order changes with episodic characteristics, and large job insecurity is found in situations where the employee is unsure of whether organizational membership can be sustained and fears the loss of the income and status linked to having a job. Often, extensive organizational changes imply high job insecurity, for instance when a merger leads to changes in job tasks and in the workplace for many employees, and in the case of downsizing which creates insecurity as to whether organizational membership can be sustained.

Much of the literature in the field concerns situations with high job insecurity (see the following paragraph for a more detailed discussion), simply due to the strong relationship between organizational changes and job insecurity. To disentangle the effect of organizational change from the effect of job insecurity, it is relevant to compare this literature to a situation with extensive organizational change in which job insecurity is as low as possible. Such a situation can be found in the local government reform analysed in this article, in which municipal mergers during an economic boom where a job guarantee was given to employees is studied. In the following, we discuss what is already known about the association between organizational change, job insecurity and health outcomes in the public sector.

The state of the art: What do we know about organizational change, job insecurity and health?

A number of longitudinal studies from the public sector report negative associations between organizational change and employee health during conditions of high job

insecurity. Whitehall II is a seminal study of employees in the British Civil Service, starting in 1985. In the years after the initial data collection, major reforms were initiated in order to examine the possibility of 'Using Private Enterprise in Government'. The goal was to cut public expenditure, cut the number of staff and to improve efficiency. For the employees, the process involved a period of uncertainty during which transfer to the private sector was considered. In the following years, 74 per cent of the civil servants were moved into executive agencies (Ferrie et al. 1998, 244), and one of the departments – The Property Services Agency – was later privatized, which led to comparatively higher job insecurity for this agency (Ferrie et al. 1998). The main dependent variables in the studies are self-reported health status, health-related behaviours, blood pressure, body mass index (BMI) (Ferrie et al. 1998; 2002), longstanding illness, adverse sleep patterns and minor psychiatric morbidity (Ferrie et al. 1998). The results show that the civil servants who faced job insecurity experienced adverse health effects, both in the anticipatory phase (Ferrie et al. 2005; Ferrie et al. 1998) and as long-term effects (Ferrie et al. 2002).

A longitudinal study of how a large recession in the 1990s influenced the employees in Finnish municipalities when they underwent downsizing processes of varying degrees yielded similar findings (Vahtera, Kivimäki, and Pentti 1997; Kivimäki et al. 2001; Kivimäki et al. 2003; Kivimäki et al. 2007), and downsizing was found to have negative health outcomes. Similarly, a study of military officers (Rohall 2001) documents an association between downsizing and increased anxiety and depression, while a study of hospital workers finds an association between the perceived amount of overall change and GHQ caseness (general health questionnaire) (Loretto 2010). In addition, a number of studies provide evidence of a similar association in the private sector. Importantly, Dahl (2011) documents that organizational change increases stress in a study with longitudinal data combining survey data on the size of change combined with administrative data on medication. There are also studies that report an association between exposure to

downsizing and perception of job insecurity (Moore, Grunberg, and Greenberg 2006). Moreover, it is important to note that the relationship between downsizing and adverse outcomes is found not only for the workers losing their jobs, but also for those who retain their jobs (Kivimäki et al. 2007; Browning and Heinesen 2012). A possible reason for this finding is that all workers experience job insecurity, also those who ended up keeping their job.

Another important finding is a significant positive association between the degree of downsizing and sickness absence among permanent employees, but not temporary employees (Vahtera et al. 2004). In the latter case, the increased job insecurity leads to lower rates of sickness absence. Some people take no time off work, in spite of being sick; a phenomenon sometimes referred to as "sickness presenteeism" (Caverley, Cunningham, and MacGregor 2007; Bergström et al. 2009). The reason for this is arguably that job insecurity also acts as a disciplinary device, so that more insecurity yields less absence for a given health condition. Hence, if the threat of direct job loss is sufficiently large, the disciplinary effects may neutralize or dominate the health effects. The Whitehall study also offers evidence in that direction. Individuals affected by general organizational changes showed a rise in sickness absence, while those facing imminent privatization of their department showed a decline (Ferrie 2001, see also Kivimäki et al. 2003; Røed and Fevang 2007). While most studies in the field find a negative effect of job insecurity Bambra et al. (2007) find some indications that participatory interventions could have potential in alleviating the negative health effects of organizational downsizing.

In sum, from the previous literature we can be relatively confident that in most cases there is a negative association between job insecurity and employee health (e.g., Quinlan and Bohle 2009; Sverke, Hellgren, and Naswall 2002). However, there is a risk of conclusions being drawn in relation to organizational change as a larger phenomenon, without making a distinction between organizational change and job insecurity. Indeed, it is difficult to draw a clear fault line between the

two, both conceptually and empirically. Nevertheless, it is possible that negative health impact is mainly a consequence of job insecurity, rather than organizational change in general.

A further issue is that systematic reviews of the studies in the field conclude that at this point no conclusions can be drawn regarding the causal relationship between organizational change and employee health due to problems of endogeneity, and that there is a need for more longitudinal and quasi-experimental designs, possibly avoiding cross-sectional data with subjective measures and/or common source bias (Bamberger 2012; Ferrie 2001). One of the basic problems in studying the effects of organizational change is that the change is typically not exogenously determined. In other words, such change may occur in response to challenges that an organization is confronting, including factors related to the dependent variable. Furthermore, studies of organizational change are vulnerable to selection bias because of the voluntary nature of most organizational change.

One way around these two limitations is to study comprehensive organizational changes with low job insecurity that are exogenously determined, such as the 2007 Danish municipal reform, in which a number of municipalities were merged while others were left intact. 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, Houlberg, and Serritzlew 2014).[1]

We derive our hypothesis from the existing literature, which finds detrimental effects of organizational change in the context of high job insecurity, though, as indicated, it is an open question whether the results hold in a low-insecurity context like the one investigated here.[2]

Hypothesis H1: Municipal employees in merged local governments experience greater negative health outcomes than employees in non-merged local governments.

[1] The terminology regarding the type of design varies somewhat from author to author and between fields. Using the MRC guidance, our experiment can be categorized as a natural experiment (Craig et al. 2012).

[2] Hypotheses 1 and 2 were part of the original research proposal to the Danish Research Council and so were not developed during the analysis. As for hypothesis 3, we did consider heterogeneity in the effects in the proposal but did not specifically mention administrative employees. The project description is available from the authors on request.

Although organizational change itself can have both positive and negative consequences for employees, one would expect these consequences to be unevenly distributed in the new organizations according to the former institutional positions of employees. Employees working in a small municipality merging with a large municipality are likely to experience a greater change in control and social support, as they will have fewer former colleagues in the new organization, though they do not lose their organizational membership as such. In other words, even if we find no general effects under low job insecurity, effects may appear for the employees who experience the most profound changes:

Hypothesis H2: Mergers have a greater negative effect on health for employees from an organization that constitutes a small share of the new organization than for employees coming from an organization that constitutes a larger share of the new organization.

One further type of heterogeneity is likely, as some groups of employees are likely to be affected more than others. Indeed, we expect the administrative staff in a municipal administration to experience more pressure during the merger period. As preparation for the mergers will presumably largely be taken care of by the administrative staff, these employees may experience an increased workload during the merger:

Hypothesis H3: Mergers have a greater effect on the health of administrative staff than on the health of other employees.

It should be noted that while the threat of losing organizational membership was low, the administration could in many cases be expected to undergo more reorganization than other sections of the municipalities, which may lead to changes in status and control.

It is relevant to emphasize that the logic of H2 and H3 is not to vary job insecurity, but to include analyses of those who are subject to the most sweeping changes and thereby most prone to experience adverse health effects. It would be particularly noteworthy if, in a low insecurity setting, even these groups are not affected by the organizational change.

Context

In Denmark, local governments are responsible for central welfare services such as schools, day care, elderly care and primary health care, and these services are governed by a city council with the mayor as the head of the daily administration. Furthermore, local governments are entitled to claim taxes. In this paper, we utilize a radical reform in 2007, in which some municipalities were merged while others remained intact, to provide causal estimates of the effects of reform on employee health.

In 2003, the reform was unexpected and we consequently chose 2003 as our base year in the empirical analysis (as we will return to later, the results are robust against alternative choices of base year). In fact, in 2002 the liberal prime minister guaranteed the impossibility of local government reform, and even with the benefit of hindsight it is considered by observers as “the unthinkable reform” (Christiansen and Klitgaard 2008). In the summer of 2002, a debate regarding the possibility of an amalgamation reform began in the media, triggered by young but central politicians in the liberal party (Bundgaard and Vrangbæk 2007), and this debate led to the formation of the Commission on Administrative Structure. It was still far from clear, however, that a reform would be the consequence (and if so what the content would be). As Christiansen and Klitgaard

(2008, 11-12) note, in the late 1990s a similar commission with the same chairman had concluded that major changes were not needed.

In January 2004, the commission published a report recommending amalgamations, and within six months an agreement was reached between the government and its coalition partner, leading to the formation of 98 local governments. 32 of the old local governments remained unchanged, whereas 66 were the result of amalgamations. All local governments – whether merged or not – received new tasks transferred from the old counties. Thus, the reform involved both organizational size and organizational tasks. However, the latter changes in tasks were the same across all municipalities.

It was not until 2004 that it became clear that a reform would occur. At the local level, the decision triggered a decision-making process on how local governments would be partnered.[3] Analysis of the matching process shows societal connectedness in the form of commuting patterns as the main explanatory variable, whereas political and economic similarity did not play a significant role (Bhatti and Hansen 2011). Generally, the local governments freely formed coalitions with neighbouring local governments as the result of negotiations, under the restriction that the new local governments were to have jurisdiction over at least 20,000 inhabitants (Bhatti and Hansen ibid.). Thus, especially the small municipalities were forced to merge. In the following, the amalgamated local governments are regarded as belonging to the treatment group, which has been subject to an exogenously induced organizational change, while the non-amalgamated local governments are considered as the control group.

In addition to the quasi-experimental nature of the reform, an interesting feature of the design was the relatively low job insecurity induced by it; all employees in the local governments were as a minimum guaranteed a job by 1 January 2007 (in most cases until April 2008), when the

[3] A single municipality (the municipality of Farum) had trouble finding amalgamating partners, because of its poor economic and political situation (Klausen 2006), and there is only one example of a municipality attempting to avoid amalgamation with surrounding local governments as a result of economic and political differences (the municipality of Vallensbæk). These cases are exceptions rather than the rule and do not influence the overall picture.

reform took effect. As part of the reform, the government decided that all local government employees would be subject to the national law on change in ownership (Virksomhedsoverdragelsesloven 2002). The purpose of this law is to reduce employee uncertainty when companies are being merged or sold, by guaranteeing employees the same working conditions in the new organization as they had in their former organizations. In some municipalities, further guarantees were given. Moreover, the economy was booming in the years leading up to and immediately following the merger, and long-term recruitment rather than unemployment was regarded as the major challenge for the public sector, as the workforce was about to decrease as a result of demographic changes. From the time the reform took effect in January 2007, municipal employment increased steadily until it peaked in mid-2009 (a total increase of approx. 7 per cent in full-time equivalents). Subsequently, it remained relatively stable for a year and then started to fall in late 2010, due to the global financial crisis. Employment reached January 2007-levels in November 2012. Thus, compared to other studies in this field job insecurity is very low indeed, although the mergers were radical and episodic. Consequently, the mergers can serve as a critical case for investigating the health outcome of the employees, when organizational change is high but job insecurity is low. The radical and episodic nature of the changes studies also means that the impact of repeated waves of organizational change is outside the focus of the present study (see Moore, Grunberg, and Greenberg 2006 for results on this).

Data

The empirical analysis is based on individual-level administrative data from Statistics Denmark. The data, which were originally collected from official government registers, are made available to researchers in an anonymised form. These data, which are unique in an international context, cover almost the entire Danish population for the entire period analysed and are considered to be of a very

high quality. In this paper, we primarily use data for the 2003-2008 period to investigate the effect of the reform in 2007, but, as will be discuss below, the results are robust to other definitions of time periods.

We have access to data for a full sample of approximately 550,000 individuals who worked in the municipal sector in 2003. Individuals who were below 20 years of age in the first year of study or above 65 years in the final year were excluded, and observations from the two largest local governments, Copenhagen and Aarhus (which were not amalgamated) were also omitted as their exceptionally large size could make them incomparable to the other local governments. Though including these two municipalities in our models would not change the main results, it would reduce the balance in the subsequent matching procedure, which is implemented as a robustness test. The island of Bornholm is also excluded, as it had already undergone an amalgamation, in 2003. This procedure leaves us with 239 amalgamated (treated) local governments, 29 controls and 437,784 relevant individuals. Finally, approximately 15 per cent of these individuals were excluded, primarily because no information was available on their work address. The dataset contains individual-level information regarding long-term term sickness leave, the use of the health care sector, prescription drug use and individual socio-demographical variables. Furthermore, the dataset includes a range of local government-level controls based on administrative data from the Ministry of the Interior.

Three types of outcomes are examined, namely long-term sickness leave, the use of the health care sector and the use of prescription drugs, which together cover health outcomes that are observable and not observable by employers.[4] The sickness absence outcome is operationalised as the number of weeks that an individual received benefits from the state as a result of long-term sickness absence. We regard long-term sickness absence primarily as a measure of health.

[4] The order in which the outcomes are presented is not determined by relative relevance but is determined by how observable the three groups of outcomes are to employers. Sickness absence is the easiest to observe. Health care usage is observable only to the extent that an employee needs time off for a particular visit. Drug use is almost entirely unobservable.

According to Marmot et al. (1995), it is a major challenge to determine whether sickness absence can best be viewed as a measure of morbidity or a manifestation of job dissatisfaction or other problems. However, Marmot et al. (1995) conclude that it is important to distinguish between short versus longer periods, as for shorter periods absence can to a certain extent be explained by "illness behaviour" – i.e. the subjective tendency to take time off when feeling unwell, whereas longer absence periods are less likely to be explainable by such subjective factors. Our data on sickness absence are for long-term sickness absence, which is regarded as reflecting health to a larger extent than short-term sickness absence.

With regard to the use of health care, we use three indicators: the number of contacts with psychologists in the primary health care sector, the number of contacts with psychiatrists in the primary health care sector and the number of contacts with general practitioners. We examine the primary health care sector rather than admittances to hospitals, as the threshold for hospital admittance is substantially higher than that for primary care sector usage. Finally, the use of stress-related prescription drugs is measured by an indicator that determines whether an individual had used drugs with ATC codes N05CF, N05BA and N06AB in a given year (Dahl 2011). We refer to this variable as 'stress-related drug use'. We also report two alternative operationalizations: a narrower one including only drugs with the code N06A (antidepressants) and a broader one containing all types of prescription drugs. We include the indicator for all drugs, because stress can sometimes manifest itself through other illnesses and can thus be captured by more general health (e.g. somatic) outcomes (the logic is the same for the general practitioner outcome). Even though there is no reason to expect different effects for different outcomes, we include several indicators for health and stress, as an aggregation of several health measures could obscure which indicators were actually driving the results.

Below, we show graphs of the seven outcomes by treatment status during the period of

investigation to provide an initial view of the data. There are no clear differences in the trends across the period in which individuals in the treatment and control groups are compared. In the analysis below, we investigate whether this pattern holds in a more stringent difference-in-differences model.

Figure 1: The seven outcomes divided by treated and non-treated individuals from 2003 to 2008.

[Figure 1 is submitted as two separate .eps files]

The longitudinal, individual-level administrative data are unique in an international perspective and provide a range of advantages. First and foremost, because the dataset is longitudinal and covers the period before and after the Danish municipal reform for individuals working in both amalgamating and non-amalgamating entities the data are well suited for difference-in-differences estimation. Secondly, the data are based on official records and are not dependent on self-reporting, which increases the reliability of the measures. Our use of objective health care data is especially important, as we are examining outcomes on health and drug use related to stress, which may be considered embarrassing by some individuals. Thus, self-reports may suffer heavily from social desirability and non-response bias. However, the use of health care or sickness absence may not necessarily reflect people's true health statuses, as the inclination to report illness and/or use health care is individual in nature. Therefore, we also include indicators related to the use of prescription drugs. Thirdly, because the data cover most of the population for the entire period problems of bias resulting from non-random response and panel attrition are largely avoided. Fourthly, it is an advantage that we have access to a number of different variables related to health, namely sickness absence and use of health care services and medicine for the same individuals. Physical indicators such as cortisol levels, blood pressure and resting heart rate would be relevant additional outcomes but are not available for our population dataset.

Estimation

To estimate the causal effect of the mergers on health-related variables, we apply a difference-in-differences logic (e.g. Blundell and Costa Dias 2008; Imbens and Wooldridge 2008). The idea is to

compare the development in individual health outcomes over time among municipal employees in the amalgamated and non-amalgamated entities. The group of employees undergoing the treatment (municipal merger) consists of employees working in the amalgamating local governments. The control group is persons working in non-amalgamating local governments. If, for instance, employees in the merged local governments increase their drug use in the period surrounding the merger to a greater degree than employees in the non-merged local governments, then this result indicates a positive effect of the treatment (merger) on drug use. More formally, the following model is estimated:

$$\Delta health_i = \theta_0 + Z_i\gamma + \delta_1 merge_i + u_i$$

The variable $\Delta health_i$ denotes the change in health outcomes – long-term sickness absence, the use of the health care sector and prescription drugs – in the period under investigation (2003-2008). The year 2003 is chosen as the baseline year. δ_1 is the effect of the variable $merge_i$, and *merge* is measured by a dummy indicating whether the individual worked in an amalgamated or a non-amalgamated municipality in 2003. γ denotes a vector of the effects of a number of socioeconomic characteristics captured by Z_i, capturing municipal and individual-level control variables. These variables account for differences between the merged and non-merged local governments (as we are investigating a quasi-experiment rather than a perfectly randomised experiment[5]), including differences across local governments in the composition of their employees, from the perspective that trends in outcomes could vary among different types of employees. The model is estimated using Ordinary Least Squares (OLS).

All models are estimated with standard errors clustered at the level of the former local

[5] In appendix 1, Table A1, we investigate whether there was a pre-treatment difference between the amalgamated and non-amalgamated local governments. We find that, conditional on the controls, there are only for one of the outcomes significant differences between the treatment and control group in 2003, and this difference is moderate in size.

governments. One may speculate whether there are systematic differences between employees in merged and non-merged local governments, and whether these differences may contribute to some of our findings. As a robustness test, we therefore examine whether the results hold when applying propensity score matching (PSM) rather than OLS regression. The general idea behind propensity score matching and how it is used for our purposes is explained in Appendix 3.

Our analyses include all individuals in the dataset working for a municipality in 2003, i.e. we do not restrict our study to investigating only those who remained in the municipality in the entire period under investigation. In order words, our estimate is essentially an intention-to-treat estimate. Such an estimate is advantageous, as the choice to change organizations or to retire may be caused by the mergers. However, our results are robust to restricting the sample to individuals working for the same municipality in 2003 and 2008.

We test hypothesis 2 using an alternative continuous measure of *merge,* which is defined for each individual as follows:

$$\widetilde{merge}_i = 1 - \frac{number\ of\ inhabitants\ in\ old\ municipality_i}{number\ of\ inhabitants\ in\ new\ municipality_i}$$

If employee *i* was previously employed in a municipality that was small relative to the new merged municipality, then $\widetilde{merge}_i$ will have a high value (close to 1). However, if employee *i* was previously employed in a municipality with a number of employees close to the total number of employees in the new merged municipality, then $\widetilde{merge}_i$ will have a low value (close to 0). Hence, $\widetilde{merge}_i$ lies in the interval [0,1] . A value of 0 is assigned to local governments that did not merge as a result of the reform. $\widetilde{merge}_i$ accounts for the degree and breadth of the merger, depending on the relative size of the original municipality in the new merged municipality.

Results

The overall effect of organizational change on health outcomes

We begin by estimating the effect of the mergers on all individuals who were local government employees in 2003 using OLS (see Table A2 of the appendix 2 for summary statistics). For each of the seven outcomes, we show the results from a bivariate model and a model including municipal- and individual-level controls. The results are shown in Table 1. The variable of interest is the merger dummy, which indicates the difference in outcomes between individuals in the amalgamated and non-amalgamated local governments. The consistent finding from Table 1 is that there is no evidence that the mergers had an effect on employee health. None of the coefficients are significant in the multivariate models (in fact, none of the t-statistics has a value above one), although in most cases we find the expected direction (a positive coefficient indicating poorer health for individuals working in merged local governments compared to those working in non-merged entities).

Table 1: The effect of merging on the change in the seven outcomes from 2003 to 2008 (all municipal employees).

	(1) Long-term sickness absence	(2) Long-term sickness absence	(3) Visits to psychologist	(4) Visits to psychologist	(5) Visits to psychiatrist	(6) Visits to psychiatrist	(7) Visits to general practitioner	(8) Visits to general practitioner	(9) Stress-related drug use	(10) Stress-related drug use	(11) Use of anti-depressants	(12) Use of anti-depressants	(13) Total drug use	(14) Total drug use
Merged	0.0482	0.0443	-0.0027	0.0027	-0.0104	0.0019	0.1309	0.0534	-0.0013	0.0001	0.0015	0.0006	0.0032	0.0006
	(0.76)	(0.69)	(-0.59)	(0.44)	(-1.71)	(0.23)	(1.54)	(0.43)	(-0.92)	(0.07)	(0.96)	(0.30)	(1.62)	(0.25)
Log(inhab)		0.0429		0.0032		0.0044		0.0067		0.0009		0.0011		-0.0003
		(1.52)		(1.18)		(1.26)		(0.15)		(1.35)		(1.16)		(-0.31)
Taxation foundation		-0.0000		0.0000		0.0000*		-0.0000		0.0000		-0.0000***		0.0000
		(-1.11)		(0.27)		(2.23)		(-1.87)		(0.10)		(-3.43)		(0.45)
Expenditure need		-0.0000		-0.0000		0.0000		-0.0000		0.0000		0.0000		0.0000
		(-0.55)		(-0.66)		(1.37)		(-0.60)		(1.42)		(1.93)		(1.36)
Unemployed pct		6.5062**		0.3822		0.0357		4.0822		0.0593		-0.1289		-0.0514
		(3.09)		(1.82)		(0.12)		(1.07)		(0.84)		(-1.79)		(-0.48)
Left-wing mayor		-0.0252		0.0041		-0.0058		0.0569		0.0002		-0.0019		0.0007
		(-0.55)		(0.88)		(-1.03)		(0.64)		(0.15)		(-1.34)		(0.36)
Density		0.0000		-0.0000		0.0000		-0.0000		0.0000		0.0000		-0.0000
		(0.39)		(-1.50)		(1.81)		(-0.73)		(1.44)		(1.28)		(-0.75)
Male		-0.1756***		-0.0392***		-0.0046		0.3291***		-0.0075***		-0.0095***		0.0511***
		(-5.28)		(-12.43)		(-0.90)		(6.71)		(-6.21)		(-8.32)		(23.23)
Age		0.2833***		-0.0042**		0.0077***		-0.1266***		0.0019***		0.0041***		0.0045***
		(23.30)		(-3.00)		(4.11)		(-8.95)		(4.15)		(10.86)		(7.14)
(Age/100)^2		-38.1796***		0.1304		-0.9939***		19.5182***		-0.2530***		-0.5381***		-0.2669***
		(-26.18)		(0.79)		(-4.42)		(11.20)		(-4.47)		(-11.52)		(-3.50)
Lives with a partner		-0.0305		-0.0094		0.0067		-0.0635		-0.0037*		0.0007		0.0097***
		(-0.71)		(-1.79)		(1.17)		(-1.46)		(-2.55)		(0.56)		(4.39)
Vocational training		-0.2245***		-0.0038		-0.0050		-0.1917***		-0.0034*		-0.0024		-0.0015
		(-4.57)		(-0.76)		(-0.76)		(-4.01)		(-2.37)		(-1.88)		(-0.71)
Short higher educ.		-0.2232*		-0.0080		-0.0212		-0.4065**		-0.0076		-0.0083*		-0.0111
		(-2.20)		(-0.52)		(-1.25)		(-3.17)		(-1.94)		(-2.55)		(-1.68)
Medium higher educ.		-0.1150**		0.0237***		-0.0029		-0.3456***		-0.0056***		-0.0104***		-0.0039
		(-2.67)		(5.05)		(-0.47)		(-7.29)		(-3.99)		(-8.47)		(-1.78)
Long higher educ.		-0.4209***		0.0111		-0.0238		-0.3979***		-0.0095***		-0.0176***		-0.0091*
		(-6.35)		(0.88)		(-1.89)		(-4.17)		(-3.67)		(-8.25)		(-2.03)
No. of children		-0.0476*		-0.0048*		0.0011		-0.1900***		0.0009		-0.0011		-0.0011
		(-2.47)		(-2.25)		(0.42)		(-9.19)		(1.58)		(-1.91)		(-0.97)
Children 0-2 years		0.2818***		0.0154*		0.0105		-2.8503***		0.0172***		0.0153***		0.0074**
		(6.83)		(2.25)		(1.60)		(-41.67)		(10.35)		(10.04)		(2.91)
Constant	0.6467***	-4.0998***	0.0659***	0.2100***	0.0466***	-0.2639**	1.4113***	4.4333***	0.0260***	-0.0360*	0.0318***	-0.0598**	0.0279***	-0.1487***
	(10.85)	(-7.28)	(17.35)	(3.60)	(8.71)	(-3.32)	(19.05)	(5.17)	(22.00)	(-2.08)	(23.24)	(-3.28)	(16.31)	(-6.59)
N	369355	369336	370221	369326	370153	369258	368596	367708	370231	369336	370231	369336	370231	369336
Adj. R^2	0.0000	0.0038	-0.0000	0.0015	0.0000	0.0001	0.0000	0.0150	0.0000	0.0007	0.0000	0.0014	0.0000	0.0047
F-statistic	0.5714	112.9841	0.3455	34.5938	2.9106	6.7045	2.3835	221.2870	0.8513	19.7065	0.9196	30.5100	2.6262	119.4186
RMSE	9.1486	9.1316	1.0585	1.0589	1.3070	1.3079	10.4436	10.3684	0.3274	0.3274	0.2850	0.2850	0.4811	0.4798

t statistics in parentheses based on standard errors clustered by municipality. * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Sensitivity tests

We conduct a range of robustness tests for the results in Table 1 for the selection of time period by running regressions with 2001 and 2002 as the baseline years and using models with 2004-2007 as end years. When we change the baseline year, a small negative effect is found for visits to psychiatrists (b = -0.018, t = -2.44) for the 2001-2008 model. The coefficient is insignificant for 2002-2008. For the alternative end years, there were no indications in our models that an effect occurred in 2004, 2005, 2006 or 2007, and that the effect disappeared before 2008. In two cases, the coefficients cross the 0.05 threshold, but in a negative direction in both cases: stress-related drug use in the 2003-2005 period and visits to psychologists in the 2003-2006 period.

The results shown in Table 1 are also robust against applying propensity score matching instead of OLS. When this alternative method is used, all effects are still statistically indistinguishable from zero, although in three cases we have near-significant estimates (significant at the 10 per cent level). For full results of the matching analyses, see Appendix 3.

Furthermore, we test an alternative specification using a panel design for the 2003-2008 period with year dummies and treatment-year interactions that provide additional information regarding the dynamics over time (see Appendix 4). The results from this alternative specification confirm the findings shown in Table 1. Contrary to hypothesis H1, we do not observe a negative impact amalgamation around 2007-2008 for any of the seven health outcomes.

The influence of the relative size of local governments in the amalgamations

We next test hypothesis H2 using our alternative indicator of the size of the organizational change – $\widehat{merge}_i$ – which measures the degree of merger rather than the merger *per se*. As a larger share indicates greater changes, we expect a positive coefficient of $\widehat{merge}_i$.

Table 2: The effect of the degree of organizational changes from 2003 to 2008 (all municipal employees).

	(15) Long-term sickness absence	(16) Visits to psychologist	(17) Visits to psychiatrist	(18) Visits to general practitioner	(19) Stress-related drug use	(20) Use of anti-depressants	(21) Total drug use
$\widehat{merge}_i$	0.2802**	-0.0037	0.0027	0.0566	-0.0004	0.0028	-0.0018
	(3.14)	(-0.31)	(0.20)	(0.25)	(-0.09)	(0.70)	(-0.34)
Controls included	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	369336	369326	369258	367708	369336	369336	369336
Adj. R^2	0.0038	0.0015	0.0001	0.0150	0.0007	0.0014	0.0047
F-statistic	115.0361	35.9581	6.9991	222.1190	19.8641	30.1368	118.5356
RMSE	9.1315	1.0589	1.3079	10.3684	0.3274	0.2850	0.4798

t statistics in parentheses based on standard errors clustered by municipality. * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Table 2 provides some evidence that the mergers had more severe negative consequences for employees, relatively, in entities that underwent greater organizational change. We find the expected effect on long-term sickness, as the coefficient is positive and significant. It should be noted, however, that the substantive effect is only moderate in size. The merge variable is constrained between 0 and 1, and the coefficient of 0.28 indicates an effect of 1.4 days per year (0.28 weeks). By contrast, in 2003 the average duration of sickness absence for all municipal employees in our sample was 1.67 weeks (8.4 days per year).

We experimented with decomposing $\widehat{merge}_i$ into dummy variables to obtain insight into the functional form. This step revealed that there is no difference between non-treated and mildly treated individuals, whereas those working in local governments that constituted less than 40 per cent of the new local governments appear to be significantly (but modestly in substantive terms) more adversely affected by the reform than the others. In order words, there appears to be a mild effect only for employees who experienced the most sweeping changes. For all other outcomes, no significant differences were found. This variation across outcomes is puzzling; the employees are more likely to be off work due to sickness, but they are not ill in such a way that they need to take more medication or visit doctors more frequently. However, we should be very careful not to conclude too much on 1 significant test out of 21 models.

We experimented with an alternative indicator to refer to the degree of pressure that employees may be experiencing: the relative service level compared to other local governments in

an amalgamation. The logic is that service levels should be harmonized in an amalgamation. Such a situation could be seen as highly threatening and cumbersome by the local governments with the highest relative service level, as they would have to reduce expenses. However, we do not find a relationship between relative service levels and development in the health outcomes examined.

Individuals working in the administration

Thus far, we have examined the effect of the mergers in general terms, as we have sought to measure the effect on all employees, and have found almost no effects. However, as indicated by H3, some employees in an organization may be relatively unaffected by mergers, while others are more vulnerable to the changes that mergers induce. With respect to this, our expectation was that administrative employees would be more affected by mergers, because they are likely to experience the most dramatic increase in work pressure during the reform process. In Table 3 below, models similar to those in Table 1 are shown, but only for the subgroup working in municipal administration.

Table 3: The effect of mergers on administrative employees, changes from 2003 to 2008 (only individuals working in administration).

	(22) Long-term sickness absence	(23) Visits to psychologist	(24) Visits to psychiatrist	(25) Visits to general practitioner	(26) Stress-related drug use	(27) Use of anti-depressants	(28) Total drug use
Merged	0.0837	-0.0122	0.0053	0.0227	0.0064	0.0065*	-0.0014
	(0.79)	(-0.76)	(0.40)	(0.16)	(1.80)	(2.14)	(-0.29)
Controls included	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	63699	63697	63691	63422	63699	63699	63699
Adj. R^2	0.0026	0.0010	0.0001	0.0161	0.0005	0.0007	0.0062
F-statistic	15.6049	4.8709	1.2865	60.5067	5.4546	5.0993	30.8747
RMSE	7.6508	1.0185	1.2035	9.8905	0.3207	0.2654	0.4818

t statistics in parentheses based on standard errors clustered by municipality. * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Table 3 provides only limited evidence of H3. The coefficient for long-term sickness absence increases but is still not statistically significant. We find a slight positive effect for one of the indicators of drug use, namely the use of antidepressants. The effect is small, however, given that the variable is scaled from -1 to 1. The remaining outcomes are all insignificant, though three

have the expected direction.[6] We also estimate models (22)-(28) with $\widehat{merge}_i$ rather than the dummy indicator for treatment (see Table 4 below). Consistent with the results for the entire population of public employees, we find significant effects for long-term sickness leave (b = 0.43, t = 2.30) but no effects for the other variables (t = 1.66 for the use of antidepressants).

Table 4: The effect of the degree of merger on administrative employees, changes from 2003 to 2008 (only individuals working in the administration).

	(29) Long-term sickness absence	(30) Visits to psychologist	(31) Visits to psychiatrist	(32) Visits to general practitioner	(33) Stress-related drug use	(34) Use of anti-depressants	(35) Total drug use
$\widehat{merge}_i$	0.4298*	-0.0256	0.0069	0.0086	0.0114	0.0102	-0.0019
	(2.30)	(-0.82)	(0.26)	(0.03)	(1.52)	(1.66)	(-0.19)
Controls included	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	63699	63697	63691	63422	63699	63699	63699
Adj. R^2	0.0027	0.0010	0.0001	0.0161	0.0005	0.0006	0.0062
F-statistic	15.3620	4.8777	1.2792	61.0770	5.0656	4.5356	30.9654
RMSE	7.6505	1.0185	1.2035	9.8905	0.3207	0.2654	0.4818

t statistics in parentheses based on standard errors clustered by municipality. * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

In conclusion, there is only very limited evidence in favour of H3, and the main conclusion is that the effect of the mergers is close to zero.

Conclusion

This paper aims to analyse the effects of organizational change on employee health. The research design uses the 2007 structural reform in Denmark as a quasi-experiment, employing the merged local governments as a treatment group and the non-merged governments as a control group. The effects of the mergers are investigated for long-term sickness absence, use of the primary health care sector and consumption of prescription drugs. These objective indicators of health are based on longitudinal administrative data. As the organizational change is largely exogenous in relation to the employees, we are able to make causal claims regarding the relationship between mergers and employee health.

[6] One interesting possible perspective is that we need to look at staff at the bottom of the hierarchy and not administrative staff in general to find the employees who are most affected by the change. As argued by Marmot et al. (1991) and others, those at the bottom of the hierarchies generally have poorer health. Therefore, using robustness tests we examined whether administrative staff members at the bottom of the hierarchies experience more adverse effects than others. When looking at only employees with employment codes indicating "regular office tasks and secretary work", we still find no effects of organizational change on health.

Overall, we find no significant effects on our chosen health outcomes (i.e., when investigating H1). Our second finding (in investigating H2) is that the extent of the organizational change is relevant for one of the outcomes, namely sickness absence. However, given the number of tests we should be careful not to give one significant value too much significance, and the size of the effect that we observed is modest. The reason may be that the social support of employees from smaller municipalities is reduced more dramatically in relative terms, as they have fewer former colleagues in the new organization. This may be regarded by them as a loss of control over career progress, status, autonomy and resource community (Greenhalgh and Rosenblatt 1984). These changes may become so profound that negative effects on long-term absenteeism are found, even if the employees retain their organizational membership. However, as stated previously, the size of the effects is marginal. Finally, H3 indicated that we also expected to find more negative effects on health in administration, where the workload during the merger process was high. However, this result was not observed, at least not when analysing mergers *per se*.

Our findings contribute to the research in the field. Many of the important studies of organizational changes and health outcomes in the public sector are studies of downsizing in which organizational changes imply job insecurity (Ferrie et al. 2005; Head et al. 2006; Kiwimäki et al. 2003, 2007; Kouvonen et al. 2009; Vahtera, Kivimäki, and Pentti 1997). Conceptually, this study contributes by making a distinction between organizational change and job insecurity, where the organizational membership is lost. Empirically, the Danish 2007 reform is highly relevant to analyse, as – in contrast to many of the previous studies – it occurred in conditions of low job insecurity. Indeed, job insecurity was reduced to a minimum, as the Danish employees had a government-provided guarantee of job security. Furthermore, the mergers occurred during a period in which the economy was thriving and there was a scarcity of labour in the public sector. Thus, if the Danish reform does not lead to adverse health outcomes, as most other organizational changes

examined do, this may indicate that low job insecurity can help mitigate the negative consequences of large-scale reform. It should be noted that we measure effects until two years after the reform. Therefore, the possibility that health effects can occur after several years is not precluded, for instance after the financial crisis kicks in and job insecurity increases.

Furthermore, this study contributes to the literature by using an empirical approach which identifies causal effects rather than simple correlations (Bamberger et al. 2012). The municipal reform was not a randomized experiment in which we, by definition, can preclude confounders, but the quasi-experimental design increases the possibilities for identifying causal effects. Although organizational change is often perceived as a cause of health problems, this study indicates that the effect may actually be modest in some cases – at least when job insecurity is reduced. Thus, once again, the study underlines the need for caution when drawing causal conclusions. The results are surprising, as chief executives reported that their employees were overworked and seriously threatened by stress during the reform process. The empirical evidence presented here suggests that this experience may result from incidents of stress being attributed to the mergers rather than actually being caused by the mergers.

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Appendices

Appendix 1: Pre-treatment differences

Table A1: Pre-treatment differences between amalgamated and non-amalgamated local governments for the seven outcomes.

	(1) Long-term sickness absence	(2) Visits to psychologist	(3) Visits to psychiatrist	(4) Visits to general practitioner	(5) Stress-related drug use	(6) Use of anti-depressants	(7) Total drug use
Merged	0.0209	-0.0038	-0.0186	0.3639*	-0.0007	0.0030	-0.0065
	(0.30)	(-1.08)	(-1.81)	(2.33)	(-0.42)	(1.64)	(-1.92)
Log(inhab)	-0.0216	0.0033*	-0.0031	0.2103**	0.0023**	0.0002	0.0030*
	(-0.71)	(2.04)	(-0.90)	(3.17)	(2.91)	(0.21)	(2.18)
Taxation foundation	-0.0000	0.0000**	0.0000***	-0.0000**	-0.0000	-0.0000	-0.0000
	(-0.25)	(2.90)	(3.79)	(-2.96)	(-0.01)	(-1.71)	(-0.11)
Expenditure need	0.0000	0.0000	-0.0000	0.0000	-0.0000	-0.0000	0.0000
	(1.12)	(0.79)	(-0.60)	(0.57)	(-0.49)	(-0.72)	(1.10)
Unemployed pct	-2.5480	-0.1053	-0.8257***	-0.5993	0.0312	0.0402	-0.0389
	(-1.59)	(-0.80)	(-3.40)	(-0.11)	(0.34)	(0.45)	(-0.29)
Left-wing mayor	0.0240	0.0018	0.0120	-0.2497*	-0.0012	-0.0010	-0.0014
	(0.72)	(0.56)	(1.65)	(-2.30)	(-0.81)	(-0.68)	(-0.46)
Density	0.0000*	-0.0000	0.0000	-0.0001***	0.0000	0.0000	-0.0000
	(2.25)	(-0.55)	(1.88)	(-3.72)	(0.82)	(1.78)	(-1.95)
Male	-0.6202***	-0.0350***	-0.0192***	-4.0540***	-0.0373***	-0.0197***	-0.1888***
	(-28.15)	(-20.91)	(-4.65)	(-86.72)	(-33.67)	(-22.86)	(-81.47)
Age	0.1576***	0.0034***	0.0076***	-0.1816***	0.0072***	0.0071***	-0.0151***
	(18.71)	(5.58)	(5.09)	(-15.04)	(17.11)	(20.46)	(-23.50)
(Age/100)^2	-16.3672***	-0.5357***	-1.0304***	19.2639***	-0.5289***	-0.7706***	1.7596***
	(-15.70)	(-7.09)	(-5.75)	(13.57)	(-10.14)	(-17.92)	(21.99)
Lives with a partner	0.0175	0.0100***	0.0129**	-0.1085*	0.0098***	0.0064***	-0.0086***
	(0.66)	(4.30)	(3.01)	(-2.41)	(7.26)	(5.90)	(-4.31)
Vocational training	0.1332***	0.0027	-0.0047	0.1896***	-0.0032*	-0.0026*	0.0139***
	(4.03)	(1.66)	(-1.24)	(4.53)	(-2.57)	(-2.30)	(7.43)
Short higher educ.	-0.5795***	0.0041	0.0030	-0.7408***	-0.0179***	-0.0136***	-0.0184**
	(-8.22)	(0.62)	(0.23)	(-6.90)	(-4.89)	(-4.07)	(-3.17)
Medium higher educ.	-0.4165***	0.0282***	0.0134***	-0.5196***	-0.0117***	-0.0074***	-0.0106***
	(-15.71)	(14.23)	(3.53)	(-12.70)	(-8.88)	(-6.67)	(-5.32)
Long higher educ.	-0.8966***	0.0252***	0.0276*	-1.5025***	-0.0160***	-0.0145***	-0.0491***
	(-21.39)	(4.88)	(2.58)	(-16.20)	(-6.90)	(-6.98)	(-10.47)
No. of children	-0.1115***	-0.0055***	-0.0159***	-0.2457***	-0.0139***	-0.0081***	-0.0067***
	(-9.54)	(-5.49)	(-7.88)	(-14.04)	(-24.06)	(-16.50)	(-7.42)
Children 0-2 years	-0.2229***	-0.0107***	-0.0236***	3.0669***	-0.0136***	-0.0122***	0.0059**
	(-9.98)	(-4.15)	(-6.18)	(48.70)	(-12.58)	(-12.51)	(3.04)
Constant	-1.7664***	-0.0674	-0.0010	11.6265***	-0.0988***	-0.0670***	1.0822***
	(-3.98)	(-1.87)	(-0.01)	(9.39)	(-5.10)	(-3.45)	(29.07)
N	375335	375335	375319	372981	375335	375335	375335
Adj. R^2	0.0081	0.0022	0.0015	0.0586	0.0191	0.0055	0.0393
F-statistic	184.0218	69.7006	53.4238	698.6262	308.0081	154.2363	811.0980
RMSE	5.7115	0.4814	0.8627	8.3564	0.2976	0.2437	0.4081

t statistics in parentheses based on standard errors clustered by municipality. * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Appendix 2: Descriptive statistics

Table A2: Descriptive statistics for the variables included in the analysis (Table 1).

Variable name	Obs	Mean	Std. dev.
Change in long-term sickness absence	428,269	0.545	9.531
Change in visits to psychologist	429,552	0.066	1.083
Change in visits to psychiatrist	429,458	0.039	1.364
Change in visits to general practitioner	427,526	1.529	10.572
Change in stress-related drug use	429,565	0.025	0.332
Change in the use of antidepressants	429,565	0.033	0.292
Change in total drug use	429,565	0.030	0.481
Merged	376,364	0.739	0.439
Log(inhab)	376,364	10.185	0.955
Taxation foundation	376,364	136680	31472
Expenditure need	376,364	34332	1671
Unemployed pct	376,364	0.034	0.010
Left-wing mayor	376,364	0.467	0.499
Density	376,364	635.3	1544
Male	436,285	0.221	0.415
Age	437,784	42.24	11.141
(Age/100)^2	437,784	0.191	0.092
Lives with a partner	437,784	0.831	0.375
Vocational training	437,784	0.331	0.471
Short higher educ.	437,784	0.017	0.129
Medium higher educ.	437,784	0.319	0.466
Long higher educ.	437,784	0.038	0.191
No. of children	437,672	0.948	1.078
Children 0-2 years	437,672	0.108	0.338

Appendix 3: Matching

One may speculate whether employees in merged versus non-merged local governments are systematically different, and whether this difference may contribute to some of our findings. When estimating our model by OLS, we assume that the error term u_i is orthogonal to $(Z_i, merge_i)$. Thus, changes in health over time may potentially result from unobserved individual factors (e.g., unobserved heterogeneity in the individual tendency to report illness, take medication or visit doctors) or unobserved heterogeneity in the availability of health services across municipalities. If these unobserved factors are correlated with the explanatory variables $(Z_i, merge_i)$, then OLS estimation may produce biased and inconsistent estimates. Moreover, OLS is sensitive to differences in the covariate distribution, as it relies on extrapolation in areas with limited overlap in the covariate distribution between the treated and non-treated groups (Imbens 2004).

To investigate this concern further, we conduct an additional analysis using propensity score matching (PSM). The matching estimator balances the covariates across individuals in the treatment and control groups. Moreover, the mean effect of the reform is estimated without imposing functional form assumptions. The idea behind PSM is the following: in the first step, one estimates the probability of belonging to the treated group (i.e., employees in merged municipalities) based on the individual and municipality characteristics available in the data. This step enables us to calculate predicted individual probabilities of having been merged for both of the actual treated and non-treated groups. In the second step, individuals in the treated group are matched with individuals in the non-treated group with similar *predicted* probabilities of belonging to the group of merged employees. Hence, for each treated individual, PSM finds a match in the control group with similar characteristics. In our PSM estimation, we estimate the propensity score (i.e. the probability of belonging to the group of merged employees) using probit, individuals are matched using the nearest neighbour method without replacement, the calliper is set to 0.05, and we condition on

adequate common support across the treatment and control groups. A model estimating the propensity scores, a graph of scores and a balance test can be found below in Table A3, Figure A1 and Table A4.

Table A3: Estimation of propensity scores

	Merged
Log(inhab)	0.0201
	(0.09)
Taxation foundation	0.0000
	(1.33)
Expenditure need	0.0001
	(0.54)
Unemployed pct	16.4034
	(0.69)
Left-wing mayor	-0.7909
	(-1.88)
Density	-0.0052***
	(-4.14)
Male	-0.0098
	(-0.58)
Age	0.0024
	(0.44)
(Age/100)^2	-0.3691
	(-0.63)
Lives with a partner	-0.0058
	(-0.40)
Vocational training	0.0148
	(0.90)
Short higher educ.	0.0049
	(0.12)
Medium higher educ.	0.0360
	(1.79)
Long higher educ.	0.0711
	(1.96)
No. of children	-0.0090
	(-1.06)
Children 0-2 years	0.0244*
	(1.96)
Constant	-1.4638
	(-0.31)
N	375335
Pseudo R2	0.6220
Chi2	62.43
Log pseudolikelihood	-81483.8

z statistics in parentheses based on standard errors clustered by municipality. * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Figure A1: Comparison of the propensity scores for the treated (on support, off support) and untreated individuals.

[Figure A1 is submitted as a separate .eps file]

Table A4: Difference between the treatment and control groups before and after matching.

	Covariate balance before matching	Covariate balance after matching
Log(inhab)	-0.9478***	-0.5646*
	(-3.79)	(-2.14)
Taxation foundation	-2.9e+04**	-1.3e+04
	(-2.81)	(-1.01)
Expenditure need	-3.3e+02	650.6667
	(-0.76)	(1.00)
Unemployed pct	0.0035*	0.0050
	(2.08)	(1.59)
Left-wing mayor	-0.2379	-0.1495
	(-1.90)	(-0.86)
Density	-1.7e+03*	-1.5e+03*
	(-2.41)	(-2.07)
Male	-0.0339***	-0.0005
	(-6.62)	(-0.04)
Age	1.6089***	0.4456
	(5.91)	(0.71)
Lives with a partner	0.0107*	-0.0134
	(2.41)	(-0.84)
Vocational training	0.0345***	0.0193
	(4.33)	(0.70)
Short higher educ.	0.0005	0.0052*
	(0.68)	(2.01)
Medium higher educ.	0.0065	-0.0049
	(0.94)	(--0.10)
Long higher educ.	-0.0194***	-0.0107
	(-4.06)	(-0.98)
No. of children	0.1578***	0.1307***
	(6.87)	(4.88)
Children 0-2 years	0.0028	0.0107
	(1.71)	(0.57)

t statistics in parentheses based on standard errors clustered by municipality. * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

As the motivation behind matching is to ensure a better balance between treatment and control individuals on the covariates, we aim to reduce the difference in observed characteristics across the treated and non-treated controls. The median bias after matching was 4.6, compared with 13.8 before matching. The results depicted in Table A5 are generally comparable to those presented in Table 1, in the sense that all effects are statistically indistinguishable from zero, although in three cases we have marginally significant estimates (significant at the 10 per cent level). Standard errors are calculated using bootstrapping with clusters at former local governments (100 repetitions).

Table A5: Results for propensity score matching.

	(1) Long-term sickness absence	(2) Visits to psychologist	(3) Visits to psychiatrist	(4) Visits to general practitioner	(5) Stress-related drug use	(6) Use of anti-depressants	(7) Total drug use
ATT	0.0482	-0.0028	-0.0101	0.1321	-0.0012	0.0015	0.0029
	(0.75)	(-0.59)	(-1.78)	(1.68)	(-0.72)	(1.07)	(1.58)
Variables from Table 1 included in the propensity score	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	369336	369326	369258	367708	369336	369336	369336

z statistics in parentheses based on standard errors clustered by municipality. * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Appendix 4: Panel regressions

Table A5: Panel regressions for the 2003-2008 period.

	(1) Long-term sickness absence	(2) Visits to psychologist	(3) Visits to psychiatrist	(4) Visits to general practitioner	(5) Stress-related drug use	(6) Use of anti-depressants	(7) Total drug use
Merged*2004	-0.0860	-0.0154	0.0023	0.0016	-0.0001	0.0001	-0.0008
	(-1.45)	(-0.35)	(1.16)	(0.45)	(-0.03)	(0.14)	(-0.83)
Merged*2005	-0.0831	0.0108	-0.0013	-0.0105**	-0.0010	-0.0004	-0.0025*
	(-1.11)	(0.23)	(-0.43)	(-2.82)	(-0.51)	(-0.49)	(-2.00)
Merged*2006	-0.0700	-0.0825	-0.0052	-0.0141*	0.0015	-0.0008	-0.0020
	(-1.01)	(-1.65)	(-1.73)	(-2.34)	(0.84)	(-0.73)	(-1.33)
Merged*2007	0.0192	-0.0101	-0.0080*	-0.0180**	0.0004	0.0008	-0.0019
	(0.28)	(-0.17)	(-2.18)	(-2.81)	(0.22)	(0.74)	(-1.26)
Merged*2008	0.0481	0.0843	-0.0027	-0.0115*	0.0032	0.0015	-0.0013
	(0.76)	(1.10)	(-0.59)	(-1.98)	(1.62)	(0.96)	(-0.93)
Merged	0.0019	0.3346*	-0.0041	-0.0140	-0.0073*	0.0029	0.0000
	(0.03)	(2.28)	(-1.12)	(-1.48)	(-2.31)	(1.36)	(0.01)
Log(inhab)	0.0197	0.2113***	0.0031	-0.0033	0.0033**	0.0010	0.0027**
	(1.42)	(3.62)	(1.89)	(-1.12)	(2.74)	(0.81)	(3.05)
Taxation foundation	-0.0000	-0.0000***	0.0000**	0.0000***	0.0000	-0.0000**	-0.0000
	(-1.78)	(-3.86)	(2.79)	(5.31)	(0.07)	(-2.90)	(-0.25)
Expenditure need	0.0000	0.0000	0.0000	-0.0000	0.0000	0.0000	0.0000
	(0.00)	(0.21)	(0.61)	(-0.31)	(1.61)	(0.27)	(0.24)
Unemployed pct	2.2673	3.6245	0.0559	-0.5284	-0.0487	-0.0499	0.0350
	(1.53)	(0.75)	(0.48)	(-1.88)	(-0.47)	(-0.51)	(0.39)
Left-wing mayor	0.0058	-0.2307*	0.0032	0.0148*	-0.0010	-0.0016	0.0001
	(0.22)	(-2.45)	(0.93)	(2.34)	(-0.41)	(-0.94)	(0.07)
Density	0.0000***	-0.0001***	-0.0000	0.0000**	-0.0000**	0.0000*	0.0000
	(3.84)	(-4.11)	(-1.45)	(3.20)	(-2.68)	(2.30)	(1.72)
Male	-0.7984***	-3.9244***	-0.0511***	-0.0203***	-0.1627***	-0.0255***	-0.0427***
	(-54.07)	(-97.29)	(-35.24)	(-5.42)	(-89.18)	(-30.22)	(-44.49)
Age	0.3143***	-0.2176***	0.0032***	0.0116***	-0.0129***	0.0094***	0.0081***
	(50.75)	(-19.25)	(6.50)	(8.99)	(-28.73)	(31.41)	(23.26)
(Age/100)^2	-37.1102***	23.7419***	-0.6208***	-1.5678***	1.6116***	-1.0615***	-0.6488***
	(-52.24)	(17.72)	(-10.60)	(-9.97)	(28.23)	(-28.84)	(-14.93)
Lives with a partner	-0.0024	-0.1845***	0.0034	0.0153***	-0.0005	0.0067***	0.0074***
	(-0.14)	(-5.79)	(1.85)	(4.72)	(-0.41)	(7.08)	(6.31)
Vocational training	0.0474*	0.1520***	0.0016	-0.0047	0.0123***	-0.0035***	-0.0047***
	(2.25)	(4.14)	(1.07)	(-1.40)	(10.64)	(-3.42)	(-4.18)
Short higher educ.	-0.7393***	-0.8503***	0.0044	-0.0183*	-0.0278***	-0.0186***	-0.0223***
	(-16.22)	(-9.66)	(0.95)	(-2.21)	(-7.09)	(-6.89)	(-7.49)
Medium higher educ.	-0.5306***	-0.5808***	0.0366***	0.0137***	-0.0130***	-0.0135***	-0.0147***
	(-28.39)	(-14.51)	(21.47)	(4.42)	(-8.93)	(-12.77)	(-12.17)
Long higher educ.	-1.2006***	-1.6278***	0.0315***	0.0252**	-0.0536***	-0.0232***	-0.0215***
	(-39.04)	(-16.73)	(8.70)	(2.82)	(-16.24)	(-12.65)	(-9.94)
No. of children	-0.1198***	-0.3947***	-0.0064***	-0.0165***	-0.0072***	-0.0083***	-0.0124***
	(-14.27)	(-25.29)	(-8.34)	(-9.56)	(-11.14)	(-20.47)	(-27.70)
Children 0-2 years	0.0371	1.2550***	-0.0038	-0.0181***	0.0145***	-0.0040***	-0.0049***
	(1.58)	(35.84)	(-1.66)	(-6.14)	(11.72)	(-4.67)	(-5.24)
2004	0.4157***	0.2873***	0.0024	0.0084**	0.0073***	0.0053***	0.0065***
	(7.19)	(7.17)	(1.39)	(2.71)	(4.37)	(7.59)	(7.91)
2005	0.4682***	0.4847***	0.0136***	0.0261***	0.0122***	0.0138***	0.0139***
	(6.43)	(13.03)	(5.11)	(8.77)	(7.58)	(20.54)	(12.77)
2006	0.6049***	0.8935***	0.0258***	0.0371***	0.0175***	0.0230***	0.0214***
	(9.13)	(22.40)	(9.56)	(6.76)	(11.69)	(22.62)	(15.93)
2007	0.7814***	1.0341***	0.0548***	0.0494***	0.0265***	0.0287***	0.0261***
	(12.10)	(21.37)	(17.99)	(8.44)	(16.96)	(35.03)	(20.43)
2008	0.6452***	1.3411***	0.0659***	0.0462***	0.0279***	0.0318***	0.0260***
	(10.83)	(19.89)	(17.35)	(9.11)	(16.29)	(23.25)	(21.99)
Constant	-4.4714***	13.1039***	-0.0357	-0.1230	0.9876***	-0.1229***	-0.1320***
	(-14.49)	(12.21)	(-0.81)	(-1.61)	(29.15)	(-5.31)	(-6.29)
N	2219224	2201041	2219210	2218919	2219224	2219224	2219224
Adj. R^2	0.0092	0.0449	0.0035	0.0016	0.0319	0.0070	0.0165
F-statistic	361.2317	1.0e+03	190.9276	69.4736	990.7918	311.1485	335.1500
RMSE	6.8417	8.8326	0.6985	1.0261	0.3993	0.2708	0.3150

t statistics in parentheses based on standard errors clustered by municipality. * $p < 0.05$, ** $p < 0.01$ and *** $p < 0.001$.

Figure 1 part 1

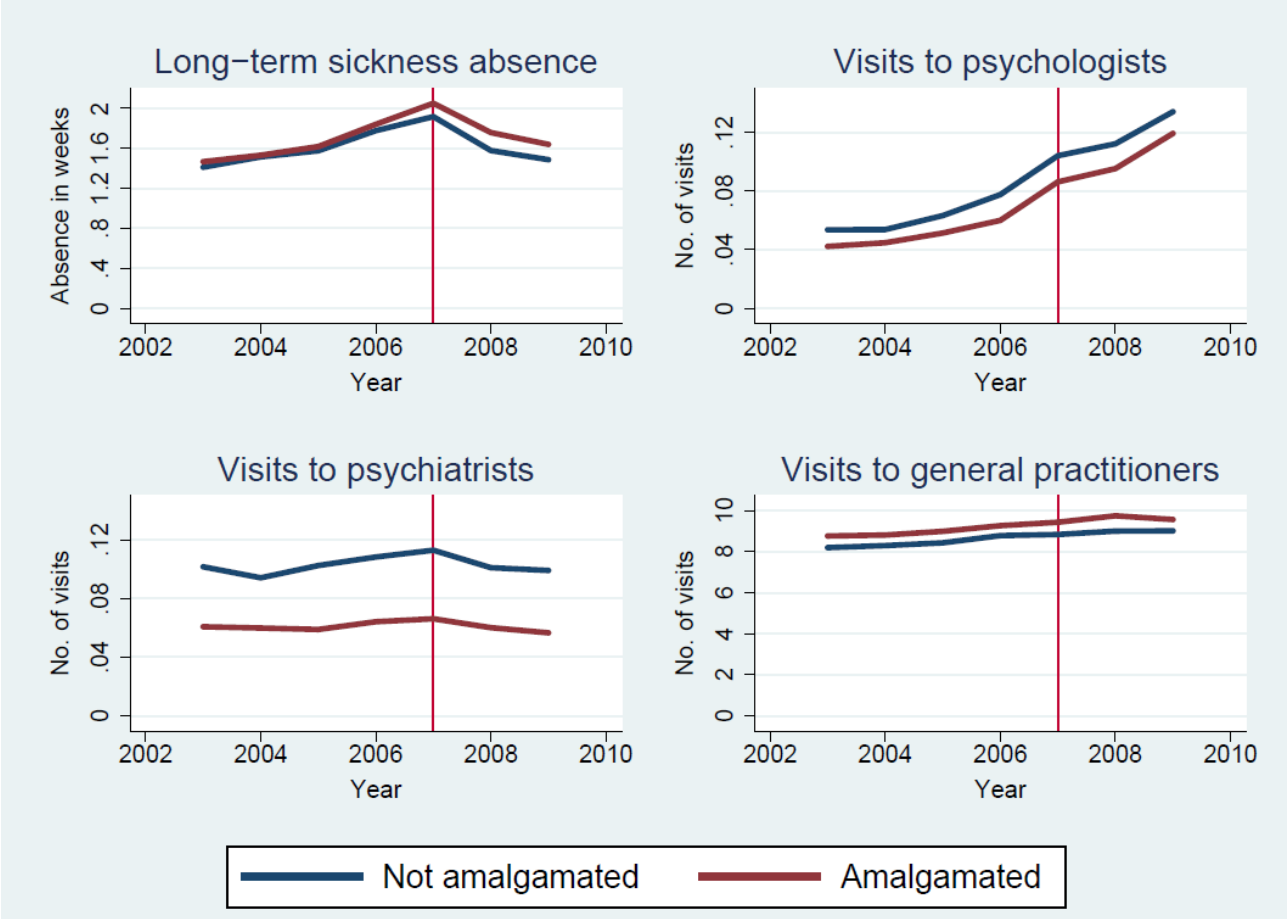

Figure 1 part 2