Lagging Behind the Joneses:
The Impact of Relative Earnings on Job Separation

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Abstract

We investigate a causal relationship between relative earnings and job separation by utilizing a unique information shock in Norway. In the fall of 2001 information about all Norwegians' taxable income from year 2000 was made available online. We employ matched employer–employee registry data and characterize employees as low or high relative earners based on how much they earn relative to colleagues of similar education and age. Our empirical analysis demonstrates that the information shock increased the job separation rate by 2.84 percentage points for the low relative earners in comparison to the high relative earners. We see a corresponding 4.8 percent increase in earnings, suggesting that the information shock motivated the low relative earners to find better paying jobs. Consistent with this conjecture, we find small and mostly insignificant treatment effects of the information shock among employees at workplaces with small variation in earnings, whereas large and significant treatment effects are found among employees at workplaces with large earnings variation.

Key words: motivation, job separation, peer groups, social comparisons, relative earnings

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"Men do not desire to be rich, but richer than other men."
John Stuart Mill (1806–1873).

1. Introduction

The idea that humans compare themselves to others has long been acknowledged as a basic human trait, even reflected in the Ten Commandments: "You shall not covet your neighbor's wife, or male or female slave, or ox, or donkey, or anything that belongs to your neighbor." People do not only care about their absolute assets in terms of beauty, popularity, authority, and possessions, but also about their assets relative to others. Research on social comparisons has a long tradition in the fields of sociology, psychology, and economics. Economists have been particularly interested in people's concerns about relative earnings.[1] A number of studies indicate a positive association between relative earnings and job satisfaction (e.g., Clark and Oswald 1996; Hamermesh 2001; Brown et al. 2008), subjective well-being (e.g., Solnick and Hemenway 1998; Frey and Stutzer 2002; Luttmer 2005; Clark, Westergård-Nielsen, and Kristensen 2009; Boyce, Brown and Moore 2010), health (e.g., Marmot 2004), redistributive preferences (e.g., Kuziemko et al. 2014), and even reward-related brain activity (e.g., Fliessbach et al. 2007). There is also a sizable body of experimental literature documenting that relative earnings affect behavior in the laboratory (e.g., Fehr and Schmidt 1999; Charness and Rabin 2002; Charness and Kuhn 2007; Clark, Masclet, and Villeval 2010; Thöni and Gächter 2010). Moreover, two field experiments have recently demonstrated a causal relationship between relative earnings and intensions to quit the job (Card et al. 2012) and between relative earnings and effort (Cohn et al. 2012).

Despite the rich evidence suggesting that people care about relative earnings, we still know little about how this concern affects real market behavior. By utilizing a unique information shock in Norway, in which information about all Norwegians' taxable income from year 2000 was made available online, this paper empirically investigates how people's concerns about relative earnings affect important decisions in real markets. An improved understanding of how concerns about relative earnings affect real labor market behavior is important because those concerns may have implications for the functioning of labor markets (Hamermesh 1975). For example, in the fair-wage model by Akerlof and Yellen (1990),

[1] See, for example, the classical references Veblen (1899), Dusenberry (1949), and Easterlin (1974). See also the seminal papers by Hamermesh (1975), Frank (1985), and Akerlof and Yellen (1990), which address important labor market implications from concerns about relative earnings.

concerns for relative earnings lead to involuntary unemployment. Moreover, Frank (1984) develops a theory of how people's relative earnings concerns may reduce variation in wage rates within firms, thereby limiting monetary performance incentives. Consistent with this theory, Frank demonstrates in the same paper that wages within firms vary substantially less than individual productivity. Finally, concerns about relative earnings could lead to costly outsourcing decisions (Cohn et al. 2012). Outsourcing will likely reduce social comparisons between the incumbent and outsourced workers, making it easier to reduce wages.

Existing literature points to several reasons for why low earnings, when compared to one's colleagues, could increase an individual's likelihood of quitting his or her job.[2] First, relative earnings may enter directly into the utility function (e.g., Fehr and Schmidt 1999), thereby affecting well-being and happiness at work. Realizing that colleagues earn more may, for instance, be perceived as unfair, thus increasing an individual's motivation to quit his or her job. Alternatively, high relative earnings may generate higher social status and respect at work. Several authors have suggested that people care about social status because it is instrumental to obtaining other goods they care about—for example, by providing access to exclusive networks (see, e.g., Veblen 1899; Frank 1985; Cole, Mailath, and Postlewaite 1998; Rege 2008). As such, low relative pay may induce individuals to seek new employment for obtaining advantages associated with higher social status. Finally, relative earnings may provide information about relative performance. According to cognitive evaluation theory, people have an intrinsic motivation to perform well, but an individual's motivation is affected by how he or she performs relative to others (Deci 1975). If relative earnings provide a signal about relative performance, then low relative earnings may decrease an individual's motivation for his or her job and thereby increase the likelihood of quitting.

Identifying a causal relationship between relative earnings and job separation is difficult because an individual's relative earnings are not random. Specifically, we are concerned that unobserved individual characteristics that affect job separation differ across relative earnings. Similar to Card et al. (2012), we combat this endogeneity problem by utilizing exogenous variation in *access* to information about earnings among colleagues. By utilizing a unique information shock in Norway, we investigate how people's concerns about relative earnings

[2] The literature also points to mechanisms for why low earnings, when compared to one's colleagues, could decrease the likelihood of job separation for the low relative earners in comparison to high relative earners. First, earnings among colleagues may serve as a proxy for future earnings. Realizing that there is potential for wage increase in the firm, low relative earnings could increase an individual's motivation. Second, more information about earnings may serve as a proxy for productivity observable for employers searching for new employees (Lazear 1986; Gallizi and Lang 1998). This may lead to increased demand—and job separation—for high relative earners.

affect job separation. In the fall of 2001, information about the taxable income for all Norwegians from year 2000 was made available online. Soon after, several online newspapers developed software that facilitated income records search, making it possible to learn, free of charge, about the taxable income of anybody in the country simply by doing a name search on the Internet. As such, Norwegians could learn about the earnings of each of their colleagues in a matter of minutes. This information shock allows us to test our hypothesis. If low earnings relative to one's colleagues increase an individual's motivation to quit his or her job, then the information shock should lead to a higher increase in the job separation rate among those with low relative earnings than among those with high relative earnings.

Our empirical analysis employs detailed longitudinal matched employer–employee registry data for every person in Norway. Utilizing unique identifiers for workplace,[3] we define colleagues at the same workplace and of similar age and education level as an individual's peer group. We then define relative earnings as the individual's income rank in his or her peer group prior to the information shock. Notably, since the information shock made information on taxable income available for all Norwegians, we have no control group of individuals who were not treated by the information shock. Therefore, we investigate how the association between relative earnings and job separation is affected by the information shock. In particular, we design our experiment by comparing differences in job separation rates across relative earnings quartiles prior to and after the information shock. Our identifying assumption is that the differences in job separation rates across relative earnings would have remained constant in absence of the information shock. This assumption may be problematic for several reasons. For example, our estimates could be biased by general diverging trends in job separation rates across relative earnings or by heterogeneous effects of business cycles across relative earnings. Importantly, our detailed registry data allow us to do a placebo analysis and several robustness and specification tests addressing the validity of our identifying assumption.

The empirical results demonstrate that the information shock increases the job separation rate by 2.84 percentage points among those with low relative earnings compared to those with high relative earnings, which is about 10 percent of the overall job separation rate. Furthermore, we find that the information shock leads to a 4.8 percent increase in earnings among those with low relative earnings compared to those with high relative earnings, suggesting that the information shock motivated the low relative earners to find a better

[3] By workplace we refer to the establishment at which an employee works. A workplace is, therefore, distinct from an employee's firm when a firm operates multiple establishments.

paying job. Notably, if these estimated treatment effects reflect that low relative earnings increase an individual's motivation to find a better job, then we should expect the effects to be stronger for individuals at workplaces with high variation in earnings, as the strength of the treatment should be stronger for these individuals. Consistent with this conjecture, we find small and mostly insignificant treatment effects of the information shock for individuals at workplaces with small earnings variation, whereas large and significant treatment effects are found for individuals at workplaces with large earnings variation.

The information shock also gave people information about earnings for all Norwegians—not only those at the same workplace. As such, our estimates could potentially reflect an information mechanism—for example, that individuals learn about market opportunities by looking at the earnings among individuals of similar education and age in the same industry. Our dataset allows us to investigate this hypothesis. By exploring the relevance of other reference group definitions, we show that our results do not seem to be driven by an information mechanism.

Our identification strategy is similar to the identification strategy in Card et al. (2012), which is the first study to identify a causal relationship between information about colleagues' earnings and job satisfaction and intentions to quit the job. Card et al. (2012) obtain exogenous variation in access to information about colleagues' earnings by sending letters to a random sample of University of California employees, informing them about a new web page that lists the pay of all university employees. A few days later, the authors conduct a survey on job satisfaction and job search intentions for all university employees. They find that the information treatment has differential effects: Treated workers with earnings below the median of their pay unit and occupation report lower pay and lower job satisfaction and increased intentions to quit, whereas they find no treatment effects among those with earnings above the median.

Our study adds to Card et al. (2012) in three important ways. First, we look at actual job separation observed in registry data and not self-reported job satisfaction and intentions to quit.[4] As such, this is one of the first papers investigating a causal relationship between people's concerns for relative earnings and real actions in real markets. Second, we investigate how information about relative earnings affects future earnings. Indeed, we show that the increased job dissatisfaction identified in Card et al. (2012) may, in the end, be a

[4] In Card et al. (2012) it is difficult to analyze real market behavior because, as the authors note themselves, their experimental design has been diluted by the diffusion of information about the web site over time. Nevertheless, the authors find some suggestive evidence that the information treatment increased the two- to three-year turnover rate of lower-ranked employees.

constructive force for the individual: When people realize their earnings are lower than those of their colleagues, they seem better motivated to exploit their opportunities, either by renegotiating their wages with their current employer or by switching to a better paying job. Third, our study demonstrates that the concerns about relative earnings identified in Card et al. (2012) apply to the full working population of Norway and not only a sample of university employees in California.

Our paper is also closely related to a field experiment by Cohn et al. (2012), which investigates the effect of relative earnings on effort. The experiment formed groups of two employees for a one-time sales promotion. Both members in each group initially received identical hourly wages independent of performance. Cutting the pay for both group members caused a decrease in performance. However, pay cuts for only one member decreased the affected worker's performance more than twice as much as when both workers' wages were cut, providing convincing evidence that effort is not only affected by absolute pay but also by relative pay.

The remainder of the paper is organized as follows: Section 2 provides a brief overview of the Norwegian labor market's institutional settings and describes the nature of the information shock. Our empirical strategy is presented in Section 3, while Section 4 describes the data. Section 5 presents our results, and Section 6 concludes.

2. The Information Shock

In Norway information on taxable income and tax payments has been publicly available since 1863. The purpose has been to facilitate transparency within the tax system and reveal how tax contributions to the society are distributed across the population. The idea is that such transparency builds confidence in the tax system and promotes tax compliance.[5]

Even if records on taxable income and tax payments have been publicly available, access was limited prior to 2001. Transcripts from the tax protocols were only available to the public at the city hall during regular working hours for three weeks immediately after the authorities finalized the tax assessments from the previous income year, usually in mid-October. In addition, journalists had extended access to the tax listings, including e-transcripts starting in 1991. In the days succeeding the release of new tax records, most newspapers devoted considerable column space to listing the earnings and tax payments of famous people and the top earners in various cities and at the national level. Hence, while information about famous

[5] Notes to the Tax Assessment Act of 1980: Ot.prp.nr.29 (1978–1979), Chapter 10.1, pg. 104 (Our translation. Norwegian title: Om ligningslov og endringer andre lover: Merknader til ligningsloven).

people and top earners has been easily accessible for decades in the daily press, information about the average Norwegian (e.g., colleagues and neighbors) has, prior to 2001, only been available by visiting the city hall during three weeks in mid-October during regular business hours and by reading through the printed tax transcripts.[6]

As new information channels evolved, an ongoing discussion during the late 1990s was whether or not to make the income and tax records available on the Internet. In 2001 the parliament decided that the tax transcripts should be made available online. On October 10th of the same year, information on taxable income, tax payments, and net wealth for all individuals in Norway was posted online. Immediately thereafter several online newspapers developed software that facilitated earnings records search, making it possible to learn, free of charge, about the taxable income of anybody in the country simply by doing a name search on the web page of an online newspaper. As such, any Norwegian with Internet access could retrieve information on the earnings of each of their colleagues in a matter of minutes.

Several indicators suggest that the online posting of income and tax records reached the general public. First, in 2001 around 60 percent of the population between 9 and 79 years old had home access to the Internet.[7] In addition, an even higher proportion of the population would presumably have Internet access from work and through friends and family. Second, a number of press releases in the days following the online posting of tax information suggest that the activity was immense.[8] Due to the September 11th terror attack, the activity on newspaper web pages was high in late September and early October of 2001. Still, when the tax information was posted online, the daily number of hits on the newspapers' web pages more than doubled within a few days, and around 3 million searches were registered within A-pressen[9] one week after the online posting. Several newspapers reported severe capacity problems and server breakdowns during these days.[10]

[6] In small municipalities it was common that local sport clubs made printed earnings lists for all inhabitants in the municipality. The sport club then sold these documents to raise funds. Slemrod, Thoresen, and Bø (2013) conducted a survey of municipalities and identified which municipalities sold (control) or did not sell (treatment) such documents prior to 2001. Among the 428 municipalities in Norway, they identified 107 municipalities as the control group and 31 municipalities as the treatment group. We chose not to utilize this dataset because 51 percent of our sample did not live in the same municipality as most of their colleagues. Moreover, as the identified control and treatment municipalities were very small, they constitute only 23 percent of our sample.

[7] Statistical Yearbook 2011, Table 231.

[8] Unfortunately, data on search frequencies on these web pages are not available.

[9] A-pressen is one of the three largest media companies in Norway and the owner of the first newspapers to provide the software to search by name in the tax information databases.

[10] In order for the online tax listings to be an information shock on relative earnings, people should search for their colleagues' earnings. We do not have information about this, but Card et al. (2012) found that people did in fact search for their "close" colleagues in the same department.

We expect the information shock from the tax posting to have differential effects across different types of workplaces. In particular, we expect peer groups with small variation in absolute earnings to experience a smaller treatment effect from the information shock, compared to peer groups with large variation in absolute earnings. Earnings variation is small in many peer groups, as the Norwegian labor market is characterized by high wage compression and strong labor unions. Most workers are members of a labor union and covered by collective wage agreements. While there are local wage negotiations over specific additions to the centralized wage agreements, a large number of workers, in particular blue-collar workers, are fully covered by the centralized wage agreements. These workers face a very small variation in absolute earnings among their peers. This is also the case for a majority of public employees, such as teachers, who are also covered by wage agreements based on centralized bargaining and only to a small extent subject to additional negotiations. At the time of our study, public employees constituted about one third of all Norwegian workers.

3. Empirical Strategy

While measuring absolute earnings is rather straightforward, the concept of relative earnings is more ambiguous. First, it requires a peer or reference group: Earnings relative to whom? Depending on the research question, a number of different peer groups may be relevant. In this paper, we investigate the hypothesis that low earnings relative to others affect an individual's motivation to quit his or her job. Presumably, people are more likely to compare themselves to colleagues at the same workplace with a similar work experience and education levels, as these factors are important predictors of earnings. Consequently, in our main analyses we define an individual's peer group as colleagues at the same workplace with similar age and education. In addition, we will explore the relevance of other reference groups, thus allowing us to investigate if our estimates are driven by mechanisms other than a concern about relative earnings. For example, an individual may learn about market opportunities by looking at the earnings among individuals of similar education and age in the same industry. Realizing that earnings could be substantially higher at other workplaces may stimulate those with low relative earnings to exploit such opportunities.

Second, having identified the relevant peer group for each individual, there are a number of measures that could reflect relative earnings. In this paper we measure relative earnings as the earnings rank within the peer group in a given year.[11] Another possible measure is, for example, the deviation from the mean or median earnings in the peer group. One possible weakness with our rank measure is that it does not account for the earnings variation in the peer group. We address this in subsample analyses, where we investigate differential effects of the information shock across peer groups with different earnings variations.

Similar to Card et al. (2012), our empirical strategy addresses endogeneity problems by utilizing exogenous variation in *access* to information about one's peers' earnings. Notably, since the information shock made information on taxable income available to all Norwegians, we do not have a control group with individuals who were not treated by the information shock.[12] We, therefore, investigate *differential* effects of the information shock about peers' earnings across the different relative earnings quartiles. In particular, we design our experiment by comparing differences in job separation rates across relative earnings prior to and after the information shock. Our identifying assumption is that the differences in job separation rates across relative earnings would have remained constant in the absence of the information shock.

The experiment is illustrated in Figure 1. Our analytic sample consists of a treatment group, which is all full time workers aged 25 to 45 in year 2000, and a control group, which is all full time workers aged 25 to 45 in year 1998.[13] We ensure the comparability of colleagues' observed earnings by restricting this sample to individuals with at least one year tenure at their workplace. For individuals in the treatment group, we observe earnings in base year t = 2000, and workplace affiliation at the end of year 2000. The information shock occurred on October 10th, 2001. To allow the treated individuals sufficient time to respond (i.e., to learn about peers' pay, react to the new information, search for new jobs, and actually obtain a new job), we observe workplace affiliation at the end of year $t + 2 = 2002$. An individual is coded as separated from his or her job if the workplace affiliation in year $t + 2$ is different from the workplace affiliation in year t. We refer to this as biannual job separation.[14] For individuals in the control group, we observe earnings in base year t = 1998 and workplace affiliation at the end of the same year and at the end of year $t + 2 = 2000$. By including the control group in the analysis, we control for time-invariant structural differences in job separation across relative earnings.

[11] This is the typical measurement within sociology; see, e.g., Sørensen (1979) and Jasso (2001).
[12] Preferably, we should have had a control group of individuals not affected by the information shock, as in Slemrod, Thoresen, and Bø (2013). Unfortunately, as discussed in footnote 5, the treatment and control groups identified in Slemrod, Thoresen, and Bø (2013) were not suitable for our analysis.
[13] Several individuals enter our dataset both in the control group and in the treatment group.
[14] We use job separation because our data do not allow us to distinguish between voluntary quits and layoffs.

We identify an individual's peer group as *i*) colleagues working in the same workplace; *ii*) colleagues of approximately the same age (plus/minus 3 years); and *iii*) colleagues with the same education level (not completed high school, completed high school, some university education). An individual's relative earnings are measured as his or her rank with respect to earnings within the peer group in base year *t*. Empirical evidence suggests that the impact of relative earnings may be asymmetric around a given reference point and that the strongest effects are in the lower part of the earnings rank.[15] We allow for non-linearity by dividing individuals in each peer group into four relative earnings quartiles k, where k = 1 for individuals in the lowest relative earnings quartile. Individuals in the highest relative earnings quartile, k = 4, will serve as the reference category. Our identifying assumption implies that the differences in job separation rates across the four relative earnings quartiles would have remained constant in absence of the information shock.

We estimate the following logit model for the probability that a worker full-time employed in year *t* has been separated from his or her job by year *t* + 2:

(1) $$Pr(Q_{i,t+2} = 1) = \Lambda\left(\alpha + \theta \cdot y_{2000_i} + \sum_{k=1}^{3}\left(\gamma_k \cdot R_{i,t}^k\right) + \sum_{k=1}^{3}\delta_k\left(R_{i,t}^k \cdot y_{2000_i}\right) + \beta X_{i,t}\right),$$

where $Q_{i,t+2}$ is biannual job separation, measured as an indicator taking the value 1 if individual *i* is not registered as employed at the same workplace in year *t* + 2 as in base year *t*, and 0 otherwise. The term $X_{i,t}$ is a vector of observable individual, plant, peer-group, and county characteristics in base year *t*. The year fixed effect is denoted by y_{2000_i}, taking the value 1 if base year is *t* = 2000. It captures general trends in job separation. The terms $R_{i,t}^k$ are the earnings quartile indicators, taking the value 1 if individual *i* is assigned to the k^{th} relative earnings quartile in base year *t*. The coefficients γ_k captures fixed effects on job separation of the earnings quartile *k* = 1, 2, 3 compared to the highest relative earnings quartile, *k*=4, which is the reference category.

Our key coefficients of interest, δ_k for *k* = 1, 2, 3, capture how the information shock affects job separation differently across relative earnings. Specifically, δ_k captures how the information shock affects job separation for individuals in the k^{th} relative earnings quartile compared to those in the 4^{th} relative earnings quartile. If our hypothesis is correct, then we should find that $\delta_1 > \delta_2 > \delta_3 > 0$, reflecting that low relative earnings increase an individual's

[15] See, e.g., Card et al. (2012).

likelihood of quitting his or her job in comparison to high relative earnings. Notably, since the estimates reflect differential and not absolute effects on job separation across relative earnings, we cannot distinguish between reduced job separation among high relative earners and increased job separation among low relative earners. Hence, finding strictly increasing estimates of delta, as suggested above, may also reflect that the information shock has no effect on individuals with low relative earnings, but it reduces job separation among those with high relative earnings, for instance, due to increased job satisfaction.

Our identifying assumption is that the association between relative earnings and job separation, i.e., the differences in job separation rates across the four relative earnings quartiles, would have remained constant in absence of the information shock. There are many reasons for why this assumption may be problematic. For example, the compositional differences between the relative earnings quartiles might change over time, which may affect the job separation rate differently across the relative earnings quartiles. Moreover, job separation is likely affected by business cycles. While the year fixed effect, y_{2000_i}, controls for the effect of unemployment on overall job separation, the impact of unemployment is potentially heterogeneous across relative earnings. In particular, if relative earnings are correlated with performance at work, individuals with low relative earnings may be more likely to lose their job when unemployment rises.

The detailed registry data allow us to conduct several robustness and specification tests for addressing these types of concerns. For example, we investigate if our estimates are robust to the inclusion of controls for observable individual and workplace characteristics and for the local unemployment rate interacted with each of the four relative earnings quartiles. We perform a placebo analysis by moving the observation window two years back, as illustrated in Figure 1.[16]

Looking at reasons for why people are separated from their jobs could permit us to investigate if our estimates are biased by business cycles. Unfortunately, our data does not allow us to distinguish between voluntary quits and layoffs, but observing the individual's new labor market status and earnings provides important insights. On the one hand, if our effect estimates are driven by a downturn in the business cycle, then we would expect that affected individuals experience unemployment and lower (growth in) earnings. On the other hand, if our estimates are driven by individuals with low relative earnings seeking better jobs,

[16] Notably, since we look at biannual job separation but only have data on workplace identifiers from 1995, we can conduct a placebo analysis only by moving the observation window two years back. Since we restrict our sample to individuals with at least one year tenure, we also need to link individuals to their workplace in year *t* - 1.

then we would expect that affected individuals experience a relative growth in earnings either from renegotiating their wages with their current employer or by switching to better paying jobs. Notably, the information shock may also lead to unemployment, if, for example, discovering low relative pay induces individuals to quit even if they have not found an alternative job.

Finally, we investigate if our estimates could potentially reflect an information mechanism, rather than a concern about relative earnings, by exploring the relevance of other reference group definitions. For instance, it could be that individuals learn about market opportunities by looking at the earnings among individuals of similar education and age within the same industry. Realizing that earnings could be substantially higher at other workplaces may stimulate those with low relative earnings to exploit such opportunities.

4. Data

Our empirical analysis utilizes a combination of several official Norwegian registers, prepared and provided by Statistics Norway. The dataset contains records for every Norwegian from 1992 to 2005. The variables include individual demographic information, such as gender and age, socio-economic information (years of education, earnings, and county of residence), and employment status. Importantly, from 1995 the database contains a workplace identifier, allowing us to identify individuals working together and job separations.

As described in the empirical strategy, our analytic sample consists of full-time workers aged 25 to 45 in year 2000 (treatment group) and full-time workers aged 25 to 45 in year 1998 (control group). We exclude older workers since the responses to the information shock and mechanisms affecting job separation are likely to be different at older ages. Additionally, we exclude part-time workers in order to limit peer earnings differences related to different working hours, thereby obtaining a more meaningful measure of relative earnings.[17] Related to this, we also ensure the comparability of colleagues' observed earnings by excluding individuals with less than one year tenure at their workplace. Finally, we exclude individuals working in recent start-ups or in very small or closing businesses by dropping individuals working in companies with less than 10 employees in any of the years from t - 1 to t + 2. This last exclusion criterion constitutes 24 percent of our gross sample.

[17] Since in some cases there appears to be a lag in reporting employment status, some individuals are recorded as full-time workers with nearly no earnings. As such, we also exclude individuals with earnings of less than 2 times "the basic amount" (approximately €20 000 in 2012). The basic amount is an earnings thresholds defined by the Norwegian Social Insurance Scheme, used to determine eligibility for old-age pension, disability pension, and unemployment benefits.

The inclusion and exclusion criteria described above also apply to the pool of potential peers. After constructing the peer groups, we ensure a meaningful earnings rank measurement by excluding individuals with less than four peers. This excludes another 32 percent of our sample, leaving us with an analytic sample of 423 831 individuals working at 13 436 different workplaces.

Our key outcome variable is biannual job separation as defined in the empirical strategy. Furthermore, we use earnings in year $t + 2$ and an indicator for unemployment in year $t + 2$ as outcome variables. Our key dependent variables are the relative earnings quartile indicators, $R_{i,t}^{k}$, which are constructed from earnings. For relative earnings comparisons, the relevant measure is actual earnings paid by the employer. Our dataset does not contain this measure; however, we observe pensionable earnings, which are the earnings that generate rights for pension benefits in the Norwegian public pension system. In addition to actual earnings paid by the employer, this includes public transfers like sick leave, unemployment, and parental leave benefits. The publicly available tax and income records list taxable income, which includes both labor and capital income and is net of tax deductions. Both pensionable earnings and taxable income may deviate from actual earnings paid by the employer, but they still serve as noisy measures of employer-paid earnings.

We construct a rich set of covariates, including individual, workplace, and county characteristics. Our individual characteristics are age (linear), gender, education (not completed high school, completed high school but not completed college, completed college), pensionable earnings (log of deflated earnings), and county of residence fixed effects (19 categories). As workplace characteristics, we include year-specific industry fixed effects (31 industry categories), number of employees at the workplace (linear)[18], number of peers (linear), and year-specific workplace fixed effects. Finally, as labor market characteristics, we include county-level unemployment rates (separately and interacted with relative earnings quartiles) and the biannual job separation rates in the relevant economic region and industry (linear).[19]

5. Empirical Results

[18] Total number of colleagues also includes those excluded from our analytic sample.

[19] Norway is divided into 95 economic regions and we have 31 industry categories. When assigning workers region–industry indicators, 38 percent of the sample was assigned to a group with less than 100 individuals. In these cases, we use the industry-specific job separation rate at the county level (19 categories) rather than economic region. For the 15 percent of the sample where this extension still resulted in less than 100 individuals, we use the industry-specific job separation rate at the national level.

5.1. Summary Statistics

Figure 2a illustrates the biannual job separation rates from base year *t* to year *t* + 2 for the four relative earnings quartiles from 1996 to 2000. In creating this figure, we use the same selection criteria for all base years from 1996 to 2000, as described above for our analytic sample with base years 1998 and 2000. We see that biannual job separation for the full sample ranges from 26 to 34 percent, which is in line with previous evidence on job separation in Norway.[20] Importantly, and consistent with our identifying assumption, Figure 2a reveals that trends in job separation are similar until base year 1999, which represents the first individuals whose biannual job separation rates may be affected by the 2001 information shock. We see that trends are diverging for base year 1999. These emerging differences are apparent in Figure 2b, where the lines show the difference in job separation rates between each of the three lower relative earnings quartiles and the highest relative earnings quartile. All levels are calibrated to the 1996 level. In the case of similar trends in job separation, the three graphs should be close to the horizontal axis. We find that trends in job separation rates are similar until base year 1999 and then start to diverge in a pattern consistent with what we would expect to find if information about low relative earnings reduces job satisfaction and increases motivation to find another job. The job separation rates increase among those in the bottom three relative earnings quartiles versus those in the top relative earnings quartile. Moreover, the relative increase in the job separation rate seems higher in the lower relative earnings quartiles. Indeed, the job separation rate for base year 2000 is nearly 4.5 percentage points higher for those in the bottom relative earnings quartile compared to those in the top quartile.

Notably, Figure 2a also illustrates that prior to the information shock, job separation rates are u-shaped across the four relative earnings quartiles: Job separation is more frequent among those with high or low relative earnings than among the median relative earners. This is likely due to compositional differences between the relative earnings quartiles and different mechanisms generating job separation. For instance, if relative earnings reflect innate ability and motivation, then job separation for low relative earners may reflect job loss, while job separation for high relative earners may reflect good labor market opportunities. As noted in the empirical strategy section, such compositional differences across relative earnings are unproblematic as long as the differences in job separation rates between the four relative earnings quartiles would have remained constant in the absence of the information shock. We will investigate the plausibility of this assumption.

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[20] Annual turnover is approximately 15 percent in Norway and close to 30 percent in the US (see Hunnes, Møen, and Salvanes 2009). Note that numbers in our study reflect biannual exit rates, i.e., over a period of two years.

Finally, Figure 2a conveys a spike in the job separation rates from base year 1998. This coincides with the business cycle at the time. The fact that the spike is in 1998, which is the base year for our control group, highlights the importance of controlling for differential effects of labor market changes across different relative earnings quartiles, as discussed in the empirical strategy section.

Table 1a depicts summary statistics for the main characteristics of interest for the control group from base year 1998 and treatment group from base year 2000. We see that women constitute only one third of the sample in both groups mainly because the majority of women work part time, and part-time workers are excluded from the sample to ensure meaningful earnings comparisons. The average age is roughly 35 years, around one fourth of the individuals in the sample have not completed high school, and around one fourth have completed a college degree. The average number of colleagues in total (size of workplace) is slightly less than 400, while the average number of peers is around 40.[21] We see that apart from a slight increase in average education level from 1998 to 2000, workplace and individual characteristics are fairly similar across the two groups.

Table 1b shows summary statistics for the four relative earnings quartiles in base year 1998 (Panel A) and base year 2000 (Panel B). The differences in job separation rates correspond to the illustration in Figure 2a. By construction, absolute earnings are lower among those with low relative earnings: Average earnings in the lowest relative earnings quartile are only 60 percent of average earnings in the highest relative earnings quartile.[22] Notably, women are substantially overrepresented among those with low relative earnings. Age, education, and workplace characteristics are, by construction, similar across relative earnings quartiles, and therefore not reported in the table. Important for our identification strategy, we see that the composition of individuals across gender and earnings between the four relative earnings quartiles is similar in 1998 and 2000.

5.2. Main Results

The main results are reported in Table 2, which estimates different versions of the model presented in Equation 1. In Model 1 we include no controls $X_{i,t}$ for individual or workplace characteristics. The three coefficients of interest demonstrate how the information shock

[21] Median number of colleagues in total and in peer group is substantially lower: In 1998 (2000), the median number of colleagues is 157 (139) and median number of peers is 14 (12). These numbers are not reported in the table.

[22] The differences in sample size across relative earnings quartiles are due to peer groups sizes that are not divisible by four (quartiles).

affects job separation rates differentially across different relative earnings quartiles. We see that the estimates reflect the changing pattern observed in Figure 2b. The information shock significantly increases the probability of job separation for those with low relative earnings in comparison to those with high relative earnings. Moreover, the relative increase in the job separation rate appears higher when moving down the relative earnings quartiles. Also note that the effect that additional information about peer earnings has on job separation seems to be convex across relative earnings quartiles.

In Table 2, Model 1 we can also see that the three main coefficients for the relative earnings quartiles reflect the u-shaped level of job separation rates across relative earnings illustrated in Figure 2a. As discussed earlier, these differences are likely due to compositional differences between individuals across relative earnings quartiles. In models 2 and 3 we investigate if our estimates in Model 1 are biased by changes in compositional differences across the relative earnings quartiles. Model 2 includes our observed individual characteristics in the base year, including county of residence fixed effects. Even if this significantly changes the main effects of relative earnings quartiles, the treatment estimates remain robust to this inclusion. This suggests that the estimated increase in job separation for those with low relative earnings is not generated by compositional changes in individual characteristics.

We are also concerned that industry-specific shocks may lead to changes in workplace characteristics that affect the relative earnings quartiles differently. In particular, the dot-com collapse occurred during our treatment period and may potentially affect our estimates. Model 3 controls for workplace characteristics and year-specific industry fixed effects. The estimates are robust to this inclusion, supporting our identifying assumption.[23] In the following, we will refer to Model 3 as our preferred model. Marginal effects[24] (in brackets) suggest that additional information about peer earnings increases the lowest relative earnings quartile's probability of separating from the job by 2.84 percentage points compared to the highest relative earnings quartile, which is about 10 percent of the mean job separation rate.

We are particularly interested in how differences in relative earnings *within workplaces* affect job separation rates differentially across relative earnings. As such, we add year-specific workplace fixed effects to the model in order to identify differential effects on job separation within workplaces. Since the logit specification does not allow for the inclusion of

[23] The same applies to a model specification where the IT sector is excluded from the sample (results not reported here).

[24] Marginal effects are calculated by using the following procedure: For each marginal effect, the relevant subsample is selected (for example, where R1 * y2000 = 1 for the marginal effect of this coefficient), and the probability for both job separation and no job separation is predicted for each individual. The marginal effect is calculated as the mean of the difference between these two probabilities.

the large number of workplace dummies, we estimate the model by using an OLS specification. We first replicate the logit estimates in Model 3 as OLS estimates in Model 4 and find that Model 4's OLS estimates are similar to the marginal effects from the logit estimation in Model 3. Year-specific workplace fixed effects are added in Model 5, and the estimated treatment effects are robust to this inclusion. This is as expected, since characteristics of the relative earnings quartiles are by construction balanced across year-specific workplace characteristics.

5.3. Heterogeneous Treatment Effects

In Table 3 we investigate differential treatment effects across peer groups with small and large earnings variation. If the estimated treatment effects in Table 2 reflect more information about relative earnings, then we should expect the effect to be stronger for individuals in peer groups with high earnings variation compared to individuals in peer groups with low earnings variation. There are at least three reasons for this expectation: First, the absolute earnings differences within peer groups are larger, and hence the effect of the treatment is likely stronger.[25] Second, large variation in earnings probably reflects a more flexible wage policy. This in turn implies that earnings are likely a more accurate measure of (and feedback on) performance. Third, in companies or industries with little wage flexibility, colleagues' earnings are likely already known because they are set through commonly known, rigid wage setting systems.

Figure 3 illustrates the distribution of earnings variation within the peer groups.[26] The dotted lines denote individuals in peer groups with the 25 percent lowest and the 25 percent highest earnings variation. Note that 25 percent of the individuals are in a peer group with less variation in earnings than 43 000 NOK (compressed wages), whereas 25 percent of the sample are in a peer group with earnings variation of more than 98 000 NOK (flexible wages).[27]

In Table 3 we apply our preferred Model 3 from Table 2 for the three subsamples. Notably, Model 1 reveals only small and mostly insignificant treatment effects of the information shock for individuals in peer groups with small earnings variation, whereas Model 3 captures strong treatment effects for individuals in peer groups with large earnings

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[25] This is in line with the alternative measure of relative earnings as deviation from mean or median earnings: On average, deviation from mean or median earnings is larger in peer groups with more variation in earnings.
[26] The top two percent are excluded from the figure.
[27] As an example, teachers in Norwegian primary schools face a very rigid wage system, where wages are almost exclusively determined by experience. More than 70 percent of all teachers are in the lowest earnings variation group constructed here.

variation. These heterogeneous responses to the information shock are consistent with our hypothesis that information about peer earnings increases job separation for those with low relative earnings.

5.4. Heterogeneous Responses to Business Cycles

One main concern in our analysis is that the information shock occurred at a time when the economy was entering into a recession. Unemployment in Norway fell during the 1990s and started rising in 2000. A negative association between job separation and unemployment is a probable explanation for the peak in the biannual job separation rate in base year 1998, as reflected in Figure 2a.[28] While year fixed effects control for the overall effect of unemployment on job separation, the impact of unemployment and outside labor market opportunities is likely heterogeneous across relative earnings. In particular, if relative earnings are correlated with performance at work, individuals with low relative earnings may face a higher probability of losing their job when unemployment rises. Such heterogeneous effects are a concern as they may bias our estimates for the effects of relative earnings on job separation.

In Table 4 we investigate if our estimates are biased by heterogeneous responses to business cycles. An important measure of labor market conditions is the local unemployment rate. In Model 1 we add controls for the local (county level) unemployment rate to Model 3 from Table 2, our preferred model. Importantly, in order to account for differential effects across relative earnings, the local unemployment rate is also interacted with the indicators for relative earnings quartile. We see that including these controls does not affect our estimated effect of relative earnings on job separation.[29] Another relevant measure of labor market conditions is the average annual job separation rate within relevant industries and economic regions.[30] As Model 2 demonstrates, our effect estimates are robust to the inclusion of industry- and region-specific job separation rates interacted with relative earnings. The robustness demonstrated in models 1 and 2 suggest that our estimated effects of relative earnings on job separation are not biased by heterogeneous effects of business cycles.

[28] Note that the job separation rate in 1998 is measured as the proportion of workers separating from their job between December 1998 and December 2000.

[29] The main effects of relative earnings quartiles and year fixed effects are not reported, since the inclusion of interaction variables (with the relative earnings quartiles) implies that the coefficients for relative earnings quartiles do not reflect the full main effects.

[30] There is a potential endogeneity issue when controlling for the relevant job separation rate. However, as described in Section 5, industry- and region-specific job separation rates are calculated for groups consisting of at least 100 individuals, suggesting that the endogeneity problem is negligible. Furthermore, the potential bias pulls in the direction of underestimating the treatment effect.

By looking at the reasons for why people separated from their jobs, we investigate if our estimates are biased by business cycles. Model 1 in Table 5 captures how the information shock affects labor market status in year $t + 2$. In the multinomial logit model, possible outcomes are *i*) having a new job, *ii*) being unemployed, and *iii*) remaining in the same job. The latter outcome serves as the base status. The independent variables are the same as in our preferred Model 3 in Table 2. Coefficients reflect the log odds ratios of the new labor market status relative to staying in the same job. Marginal effects (in brackets) reflect the percentage point change in probability of having the given labor market status. Focusing on the marginal effects, we see that the probability of being in a new job has increased by 1.9 percentage points for those with low relative earnings compared to those with high relative earnings. The probability that those with low relative earnings will be unemployed has also increased compared to those with high relative earnings, but only by 0.8 percentage points. Hence, the majority of those not remaining in the same job have a new job in year $t + 2$.

Model 2 investigates how the information shock affects earnings. The model summarizes the results from an OLS regression with log of earnings in year $t + 2$ as the dependent variable.[31] The independent variables are the same as in our preferred Model 3 in Table 2. As the estimates reveal, growth in earnings is 4.8 percent higher for those with low relative earnings compared to those with high relative earnings.

Summing up, we find that after the information shock, those with low relative earnings have increased their job separation, they are more likely to have a new job, and they experience a stronger growth in earnings, all compared to those with high relative earnings. This suggests that the estimated treatment effects are not driven by heterogeneous responses to business cycles. In particular, if our estimated increase in job separation among those with low relative earnings were due to layoffs associated with a negative unemployment shock, it seems unlikely to find that their earnings would increase more than the earnings for those with high relative earnings.[32]

5.5. Placebo Test

[31] 0.46 percent of the sample have very low or no earnings in the outcome year. Since we measure earnings on a logarithmic scale, earnings for these individuals are replaced with NOK 10 000. Excluding these individuals from the sample reduces the treatment estimates, but the significant effect on earnings remains. Furthermore, we find similar results when earnings are measured linearly.

[32] It can be argued that *relative* rather than *absolute* earnings in year $t + 2$ is the relevant measure. However, most of the literature shows that the reference point with respect to social comparisons is flexible: When searching for a new job, a likely reference point is related to the earnings distribution in the current job, although earnings satisfaction in the new job will most likely be related to the earnings distribution in the new job. Furthermore, relative earnings in the new job are not known prior to job change.

Our estimated effects of relative earnings on job separation reflect general and diverging trends in job separation across relative earnings, and not a break in the trends associated with the information shock. Similarly, the estimated effects on growth in earnings may stem from a general trend toward a more compressed earnings distribution. If our effect estimates are biased by such diverging trends, then we should find similar "treatment" effects on job separation rates and growth in earnings when moving our observation window back in time. Since workplace identifiers are available only from 1995 onward, we obtain the only possible placebo analysis by moving the observation window two years back in time, as illustrated in Figure 1.[33]

Results from the placebo analysis are reported in Table 6. Model 1 is identical to our preferred Model 3 from Table 2; however, the sample selection and observation of variables are moved two years back in time, yielding no differential effects on job separation across relative earnings. In Model 2 we report results from the corresponding placebo analysis with log of earnings in year $t + 2$ as the outcome variable. Again, we see no differential effects across relative earnings. This suggests that the effects estimated in our main models do not reflect general and diverging trends across relative earnings in job separation and growth in earnings, but effects of the information shock that occurred in the fall of 2001.

5.6. Alternative Reference Groups

Our peer group definition is colleagues of similar age and education who are employed at the same workplace. Another relevant comparison group could consist of individuals working at other workplaces but within the same industry. For example, earnings compared to others working within the same industry could reflect outside market opportunities. In Table 7 we explore the relevance of other reference group definitions. For the sake of comparison, we include our preferred Model 3 from Table 2 as Model 1 in Table 7.[34] Models 2 through 4 extend the reference groups to include all individuals working in the same industry within the county (Model 2a), to everybody living in the same municipality (Model 3a), and to the full population (Model 4a). The age and education criteria in Model 1 apply to all reference group definitions. To each of these three alternative reference group definitions, we add a model specification that simultaneously investigates the alternative reference group effects *and* the

[33] As noted in the empirical strategy, since we restrict our sample to individuals with at least one year tenure, we need to identify companies and employees one year prior to base year.

[34] In all models we replace controls for year-specific industry fixed effects with controls for industry fixed effects. The inclusion of year-specific industry fixed effects may bias the estimates in models 3 and 4 as the peer group definitions in these models are across industry.

plant-level peer group effect (models 2b, 3b, and 4b). If the correlations between the relative earnings quartile in plant-level peer groups and other reference groups are strong, then other reference group effects may simply capture the plant-level peer effects. We can control for this by including both relative earnings measures. When extending the reference groups, the sample increases due to the four-peer inclusion criteria. In order to ensure that the estimates are unaffected by the inclusion of more individuals, we exclude from the regressions all individuals who are not in our main analytic sample.

The reference group in Model 2 includes all individuals in the same industry and county. Comparing models 1 and 2a, we find similar but somewhat smaller treatment effects from the information shock. The estimates in Model 2a are consistent with the information mechanism discussed in the empirical strategy. It could be that individuals learn about market opportunities by looking at the earnings among individuals of similar education and age who are working in the same industry. The estimates suggest that realizing that earnings are substantially higher at other workplaces stimulates those with low relative earnings to exploit such opportunities.

Model 2b further investigates the information mechanisms, where we simultaneously examine industry-level reference group effects and plant-level peer group effects. We see that the industry-level reference group effects, potentially reflecting the information mechanism, disappear. The plant-level peer effect, on the other hand, is similar as in Model 1, suggesting that the plant-level peer effect estimated in Model 1 is not driven by the information mechanism. This finding might reflect that the information shock did not convey much about outside opportunities, for instance since workers would have to know the names of those working in other plants in order to obtain information about their earnings.

In Model 3 we extend the reference group definition to include all individuals living in the same municipality, independent of industry affiliation. We find no significant treatment effects of the information shock, and the pattern does not resemble the one we found for peer groups within workplaces or industry. Also, no treatment effects are found in Model 4, where the reference group is extended to the national level. When including plant-level peer dummy variables in models 3b and 4b, relative earnings effects at the municipal and national levels remain unchanged, and we find plant-level peer effects that are similar to those in Model 1.

6. Conclusion

Even if people's concerns about relative earnings have long been recognized as a basic human trait, we know little about how these concerns affect the way individuals make decisions in real markets. This is one of the first papers to investigate how concerns about relative earnings affect real market behavior, in particular on job separations.

For identification we utilize a unique information shock in Norway. In the fall of 2001, information about the taxable income for all Norwegians from year 2000 was made available online. As such, Norwegians could, in a matter of minutes, suddenly discover earnings information on each of their colleagues. The present analysis demonstrates that this information shock increases the job separation rate by 2.84 percentage points among those with low relative earnings compared to the job separation rate among those with high relative earnings, which accounts for a 10 percent increase in the overall job separation rate. Moreover, we find that the information shock leads to a 4.8 percent increase in earnings among those with low relative earnings at base year compared to those with high relative earnings at base year. This suggests that people do not simply stand still and mope when realizing their earnings are lower than those of their colleagues. Rather, they seem motivated to create a better opportunity for themselves, either by renegotiating their wage with their current employer or by switching to a better paying job.

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Tables and figures

Figure 1: Illustration of empirical strategy

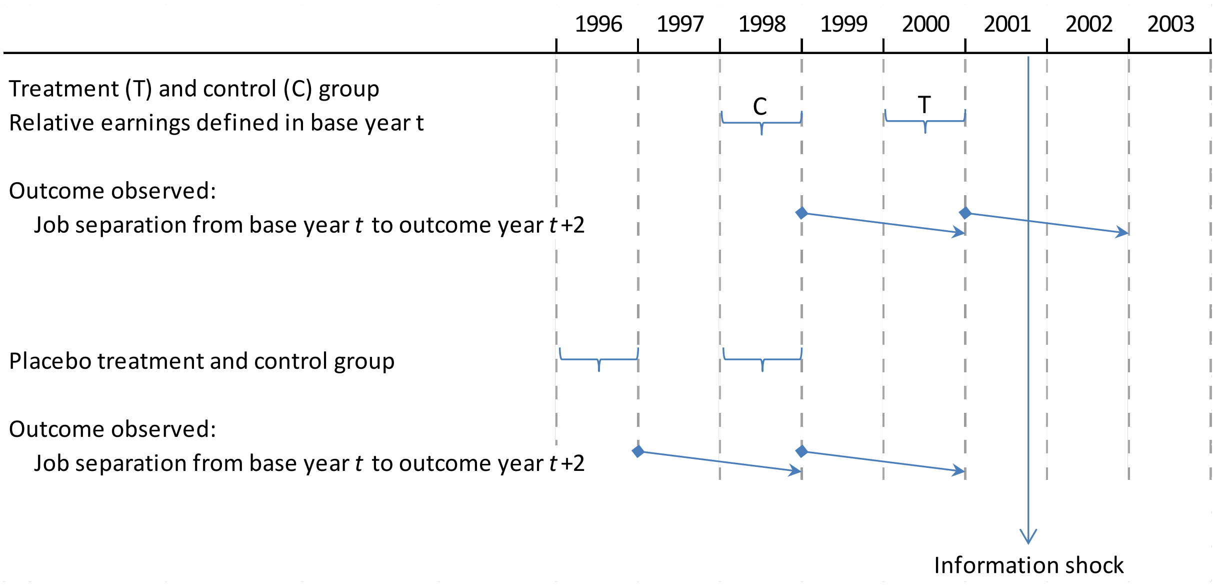


Figure 2a: Biannual job separation rates from year *t* to *t* + 2, by relative earnings quartiles and base year *t*.

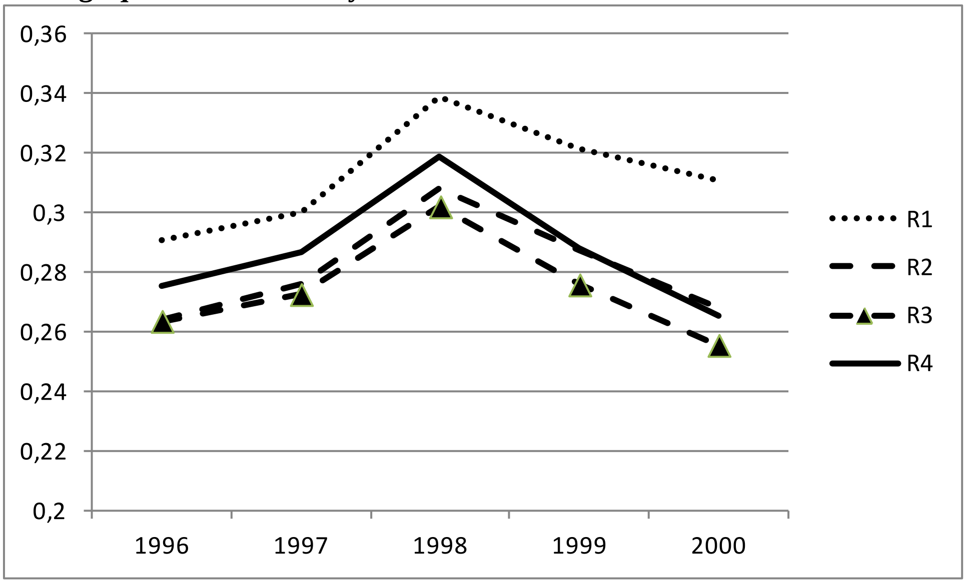
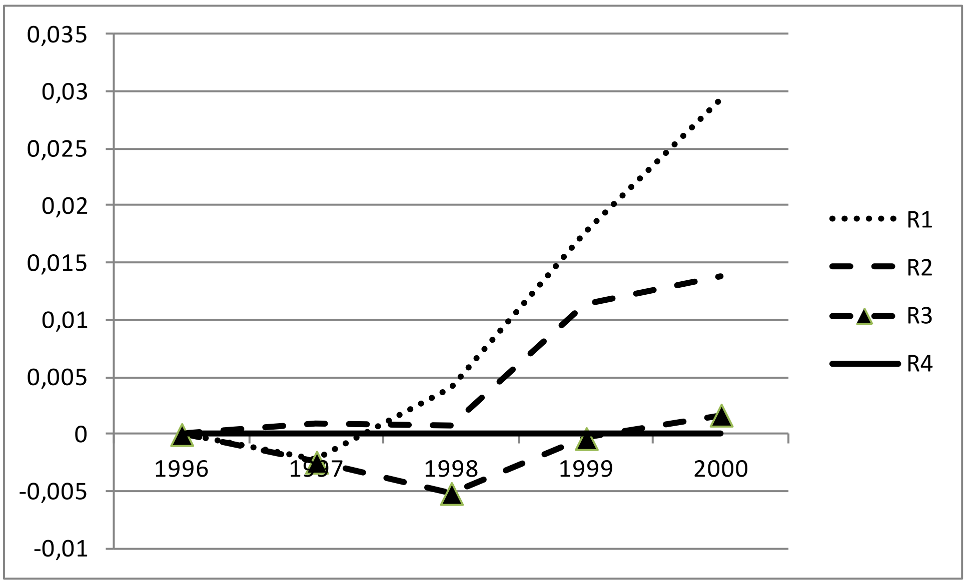
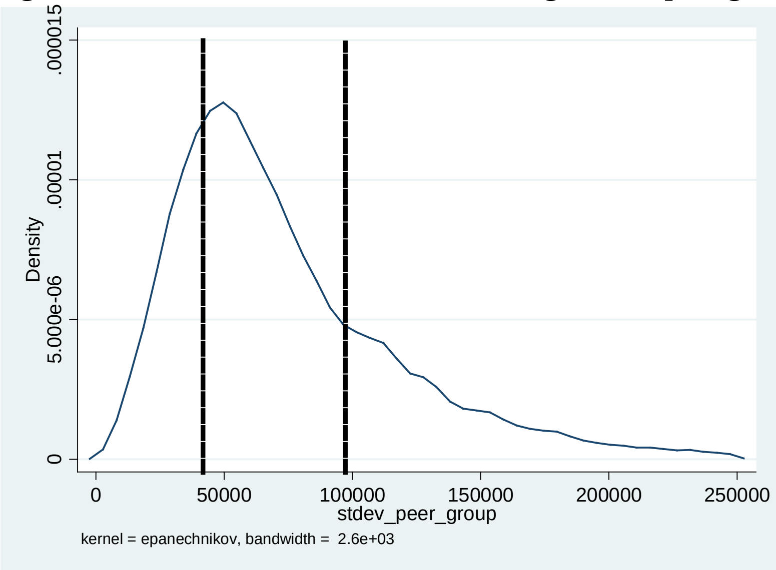


Figure 2b: Deviation in job separation rates from the high relative earnings quartiles. All levels are calibrated to the 1996 level.



Note: R1 through R4 denote relative earnings quartiles, where R1 is the lowest relative earnings quartile. Years refer to base year *t*, and job separation rates reflect proportion of individuals who are not registered in the same job in year *t* + 2 (biannual job separation rates).

Figure 3: Distribution of variation in earnings within peer group



Note: Dotted lines denote individuals in peer groups with the least (lowest quartile) variation and the most (highest quartile) variation, respectively. The top two percent are excluded from the figure.

Table 1a: Summary statistics by base year.
Means (and standard deviations)

	1998	2000
Job separation rate	0.315	0.274
Earnings	342 151	341 948
	(147 642)	(171 665)
Female	0.332	0.337
# colleagues	423	375
	(750)	(661)
# peers	41	38
	(72)	(66)
Age	35.53	35.56
	(5.35)	(5.29)
High-school drop-outs	0.294	0.246
High-school degree	0.467	0.499
College degree	0.239	0.255
N	228 756	195 075

Notes:
colleagues = number of all colleagues at workplace.
peers = number of individuals assigned to this peer group

Table 1b: Summary statistics, by base year and relative earnings quartiles.

	R1	R2	R3	R4
Panel A: Base year = 1998				
Job separation rate	0.337	0.308	0.301	0.318
Earnings	259 660	307 501	349 158	432 886
	(69 095)	(82 025)	(102 821)	(209 404)
Female	0.523	0.370	0.277	0.193
N	50 615	59 054	54 753	64 334
Panel B: Base year = 2000				
Job separation rate	0.311	0.268	0.256	0.266
Earnings	258 041	306 549	349 272	434 319
	(70 403)	(86 609)	(112 083)	(261 022)
Female	0.526	0.371	0.283	0.202
N	43 169	50 306	46 820	54 780

Table 2: Effect of information shock on job separation

	Model 1	Model 2	Model 3	Model 4	Model 5
R1*y2000	0.1322**	0.1378**	0.1409**	0.0267**	0.0252**
	(0.026)	(0.027)	(0.027)	(0.006)	(0.006)
	[0.0276]	[0.0281]	[0.0284]		
R2*y2000	0.0594**	0.0641**	0.0648**	0.0133**	0.0140**
	(0.021)	(0.022)	(0.022)	(0.004)	(0.004)
	[0.0115]	[0.0121]	[0.0121]		
R3*y2000	0.0282	0.0324	0.0324	0.0078+	0.0100*
	(0.021)	(0.022)	(0.022)	(0.004)	(0.004)
	[0.0053]	[0.0060]	[0.0059]		
R1	0.0872**	0.0044	0.0012	0.0002	0.0163**
	(0.018)	(0.034)	(0.029)	(0.006)	(0.005)
	[0.0188]	[0.0009]	[0.0003]		
R2	-0.0479**	-0.1065**	-0.1102**	-0.0232**	-0.0147**
	(0.016)	(0.024)	(0.021)	(0.004)	(0.003)
	[-0.0099]	[-0.0219]	[-0.0223]		
R3	-0.0794**	-0.1198**	-0.1237**	-0.0258**	-0.0216**
	(0.015)	(0.019)	(0.017)	(0.003)	(0.003)
	[-0.0162]	[-0.0242]	[-0.0247]		
y2000	-0.2534**	-0.2623**	-0.7174*	-0.1615*	0.0000
	(0.034)	(0.034)	(0.300)	(0.071)	(0.000)
	[-0.0530]	[-0.0538]	[-0.1553]		
Contr. individual		X	X	X	X
Contr. plant			X	X	X
OLS				X	X
Plant FE					X
Observations	423 831	423 831	423 831	423 831	423 831
Mean	0.296	0.296	0.296	0.296	0.296
(Pseudo) R2	0.003	0.020	0.030	0.036	0.225

Notes: Estimates reflect logodds coefficients with standard error in parentheses and marginal effects in brackets. ** and * denote significance at 1 percent and 5 percent levels. Dependent variable is an indicator taking the value 1 if the individual has separated from his/her job within two years from base year. Control variables include age, gender, education level, log of earnings, county of residence, number of peers in peer group, number of colleagues, and industry-year fixed effects. Multiple observations for each plant are corrected for by using the "robust cluster (.)" option in Stata.

Table 3: Effect of information shock on job separation, by earnings variation within peer group

	Model 1	Model 2	Model 3
Subsample:	Compressed wages	Medium variation	Flexible wages
R1*y2000	0.0733+	0.1578**	0.1614**
	(0.040)	(0.030)	(0.062)
	[0.0138]	[0.0327]	[0.0317]
R2*y2000	0.0299	0.0597*	0.1020*
	(0.037)	(0.028)	(0.046)
	[0.0053]	[0.0114]	[0.0188]
R3*y2000	0.0387	0.0179	0.0556
	(0.039)	(0.028)	(0.045)
	[0.0067]	[0.0034]	[0.0100]
R1	-0.0899*	-0.0887*	-0.1488*
	(0.038)	(0.042)	(0.061)
	[-0.0182]	[-0.0193]	[-0.0318]
R2	-0.1210**	-0.1684**	-0.2501**
	(0.030)	(0.030)	(0.046)
	[-0.0236]	[-0.0348]	[0.0516]
R3	-0.1175**	-0.1417**	-0.2648**
	(0.027)	(0.023)	(0.039)
	[-0.0224]	[-0.0287]	[-0.0537]
y2000	-0.3791	-1.0046**	-1.0342**
	(0.320)	(0.297)	(0.378)
	[-0.0735]	[-0.2249]	[-0.2300]
Observations	105 957	211 916	105 958
Mean	0.278	0.304	0.300
Pseudo R2	0.042	0.035	0.033

Notes: Estimates reflect logodds coefficients with standard error in parentheses and marginal effects in brackets. +, * and ** denote significance at 10 percent, 5 percent and 1 percent levels. Dependent variable is an indicator taking the value 1 if the individual has separated from his/her job within two years from base year. Control variables include age, gender, education level, log of earnings, county of residence, number of peers in peer group, number of colleagues, and industry-year fixed effects. Multiple observations for each plant are corrected for by using the "robust cluster (.)" option in Stata.

Table 4:
Effect of information shock on job separation. Controlling for labor market characteristics

	Model 1	Model 2
R1*y2000	0.1408**	0.1374**
	(0.027)	(0.028)
	[0.0283]	[0.0277]
R2*y2000	0.0650**	0.0554*
	(0.022)	(0.023)
	[0.0122]	[0.0104]
R3*y2000	0.0325	0.0269
	(0.022)	(0.023)
	[0.0059]	[0.0049]
Controls:		
Unemployment rate	X	X
Year-, region-, and industry-specific job separation rates		X
Observations	423,831	423,831
Mean	0.296	0.296
Pseudo R2	0.030	0.030

Notes: Estimates reflect log odds coefficients with standard error in parentheses and marginal effects in brackets. ** denotes significance at 1 percent level. Dependent variable is an indicator taking the value 1 if the individual has separated from his/her job within two years from base year. Control variables include age, gender, education level, log of earnings, county of residence, number of peers in peer group, number of colleagues, and industry-year fixed effects. Multiple observations for each plant are corrected for by using the “robust cluster (.)” option in Stata.

Table 5:
Effect of information shock on state in outcome year

	Model 1		Model 2
Dependent variable:	a. New job	b. Unemployed	Log earnings
R1*y2000	0.1215**	0.1760**	0.0481**
	(0.030)	(0.045)	(0.004)
	[0.0191]	[0.00835]	
R2*y2000	0.0510*	0.0990*	0.0194**
	(0.024)	(0.042)	(0.003)
	[0.00761]	[0.00485]	
R3*y2000	0.0252	0.0541	0.0086**
	(0.023)	(0.041)	(0.003)
	[0.00369]	[0.00265]	
Observations	423 831	423 831	423 831
Mean	0.229	0.070	1.942
(Pseudo) R2	0.038	0.038	0.489

Notes: Model 1 presents estimates of a multinomial logit regression. Dependent variable takes three values: Being in a new job, becoming unemployed, or remaining in the same job. The latter category serves as the base status. Estimates reflect log odds coefficients with standard error in parentheses and marginal effects in brackets. Marginal effects are calculated using the “mxf-command” in Stata. Model 2 presents OLS-estimates of a regression with log of earnings as the dependent variable. +, * and ** denote significance at 10 percent, 5 percent and 1 percent levels. Control variables include age, gender, education level, log of earnings, county of residence, number of peers in peer group, number of colleagues, and industry-year fixed effects. Multiple observations for each plant are corrected for by using the “robust cluster (.)” option in Stata.

Table 6: Effect of information shock on job separation and earnings. Placebo test

	Model 1	Model 2
Dependent variable:	Job separation	Earnings
R1*y2000	0.0061	-0.0009
	(0.023)	(0.003)
	[0.0013]	
R2*y2000	0.0089	0.0043
	(0.020)	(0.003)
	[0.0018]	
R3*y2000	-0.0224	0.0051
	(0.020)	(0.003)
	[-0.0046]	
Observations	463 475	461 256
Mean	0.294	1.927
(Pseudo) R2	0.039	0.452

Notes: Estimates in Model 1 reflect log odds coefficients with standard error in parentheses and marginal effects in brackets. Dependent variable is an indicator taking the value 1 if the individual has separated from his/her job within two years from base year. Model 2 presents OLS estimates of a regression with dependent variable as log of earnings. Control variables include age, gender, education level, log of earnings, county of residence, number of peers in peer group, number of colleagues, and industry-year fixed effects. Multiple observations for each plant are corrected for by using the “robust cluster (.)” option in Stata.

Table 7: Effect of information shock on job separation. Alternative reference group definitions

	Model 1	Model 2		Model 3		Model 4	
		a.	b.	a.	b.	a.	b.
	Plant	Industry	Industry + plant	Municipality	Municipality + plant	Country	Country + plant
Plant peer group interactions							
R1*y_2000	0.1390**		0.1409*		0.1786**		0.1563**
	(0.027)		(0.059)		(0.052)		(0.050)
	[0.0280]		[0.0284]		[0.0357]		[0.0314]
R2*y_2000	0.0648**		0.0595		0.0856*		0.0722*
	(0.022)		(0.042)		(0.035)		(0.034)
	[0.0121]		[0.0112]		[0.0159]		[0.0135]
R3*y_2000	0.0323		0.0265		0.0404		0.0346
	(0.022)		(0.029)		(0.026)		(0.026)
	[0.0059]		[0.0049]		[0.0074]		[0.0063]
Alternative reference group interactions							
R1*y_2000		0.0984*	-0.0006	0.0408	-0.0712	0.0540	-0.0421
		(0.041)	(0.075)	(0.051)	(0.076)	(0.055)	(0.078)
		[0.0202]	[-0.0001]	[0.0086]	[-0.0152]	[0.0112]	[-0.0088]
R2*y_2000		0.0710*	0.0128	0.0404	-0.0240	0.0647	0.0121
		(0.035)	(0.055)	(0.039)	(0.053)	(0.042)	(0.055)
		[0.0132]	[0.0024]	[0.0077]	[-0.0046]	[0.0121]	[0.0023]
R3*y_2000		0.0455	0.0177	0.0258	-0.0080	0.0160	-0.0117
		(0.028)	(0.037)	(0.032)	(0.037)	(0.034)	(0.039)
		[0.0083]	[0.0033]	[0.0047]	[-0.0015]	[0.0030]	[-0.0022]
Observations	423,831	423,831	423,831	423,831	423,831	423,831	423,831
Mean	0.296	0.296	0.296	0.296	0.296	0.296	0.296
Pseudo R2	0.029	0.029	0.030	0.029	0.030	0.029	0.030

Notes: Estimates reflect log odds coefficients with standard error in parentheses and marginal effects in brackets. * and ** denote significance at 5 percent and 1 percent levels. Dependent variable is an indicator taking the value 1 if the individual has separated from his/her job within two years from base year. Control variables include age, gender, education level, log of earnings, county of residence, number of peers in peer group (both at plant level and alternative peer group level), number of colleagues, and industry-year fixed effects. Multiple observations for each plant are corrected for by using the "robust cluster (.)" option in Stata.