Mandatory Notice of Layoff, Job Search, and Efficiency

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Abstract

In all OECD countries, Mandatory Notice (MN) policies require firms to inform workers in advance of a layoff. In our theoretical framework, MN helps workers to avoid unemployment and find better jobs by searching for a new job while still employed. This increases future production. The magnitude of this production gain depends on the relative effectiveness of search while employed versus unemployed. But on-the-job search and diminished work incentives reduce current production. If future gains outweigh production losses, MN extensions improve production efficiency. If not, Coasian bargaining predicts that firms offer larger severance instead of longer advance notice when MN is extended. With bargaining, the sole efficiency loss of MN is due to delayed separations of unproductive job matches. We test these predictions using novel Swedish administrative data on layoff notifications. Workers eligible for extended MN receive longer advance notice and larger severance, resulting in shorter non-employment spells and higher-paying jobs. We then disentangle the effects of longer notice versus severance. Longer advance notice causes workers to both engage in fewer job search activities while still finding better-paying jobs without delay, while larger severance delays job finding. Advance notice thus replaces job search while unemployed with more effective search while employed. We then gauge the production loss of MN by estimating the impact of notice on workers' productivity and the loss due to delayed separations. Finally, we evaluate the overall production efficiency implications of MN by combining the empirical estimates of production gains and losses with our theory. In our setting, the production gains of MN seem to outweigh the losses.

Keywords: Advance notice, Unemployment, Job search.
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A fundamental challenge for economists and policymakers alike is to design policies that assist displaced workers in finding new jobs. The policy that has received the most attention in this context is unemployment insurance (UI), which supports the unemployed while searching for a new job. Mandatory Notice (MN) constitutes an alternative policy. MN requires employers to notify workers in advance of a layoff, giving them the opportunity to search for a new job while continuing to work and staying on the payroll. MN thus encourages on-the-job search rather than search during unemployment.

MN is a prevalent, but understudied policy. It exists in all OECD countries (OECD, 2019). Yet the limited existing evidence is mostly related to the WARN Act of 1988, that introduced MN in the U.S. (see Addison and Blackburn, 1994 for a survey).[1] Many questions remain unanswered. Does MN assist displaced workers in finding jobs? If so, how does the policy achieve that? Which behaviors of worker and firm are affected? What are the costs of the policy? How does MN compare to other policy alternatives, such as UI?

This paper addresses these questions from both a theoretical and an empirical perspective. We model MN as a policy that mitigates a market failure due to an information asymmetry. Crucially, our theory allows firms to avoid the mandated information sharing by compensating the worker instead of providing Advance Notice (AN).[2] Such side-payments – which we refer to as severance – enhance production efficiency in our second-best setting. Our empirical analysis brings the theoretical insights to the data, and comes in four parts. First, we study how an extension of MN influences firms' decision to comply with the rule and provide actual AN or, instead, to pay severance. We also estimate the overall effects of the MN extension on workers' labor market outcomes. Second, we decompose the impact of MN into the effects of longer AN vis-à-vis larger severance. Third, we provide causal evidence on the higher effectiveness of job search while employed versus unemployed, thus complementing the evidence in Blau and Robins (1990) and Faberman et al. (2022). Fourth, we offer evidence on the production loss of the policy, complementing the evidence on the effect of employment protection legislation on firm productivity in Autor et al. (2007) and Bjuggren (2018).[3] Finally, we pull the pieces together by comparing the production efficiency gains and losses of MN.

Our theoretical model features an information asymmetry. Employers receive private information about an impending productivity decline that makes downsizing optimal. They have the choice of informing workers in advance. From an efficiency perspective, this choice incorporates a trade-off. Notified workers search for a new job and supply less effort on the current job (shirking). Both behaviors reduce current production, whereas intensified job search increases future production by reducing non-employment and improving subsequent job quality. The magnitude of the future production gains depends on the effectiveness of search while employed relative to search while unemployed. Firms never notify workers in the absence of a mandate, as they do not internalize the positive effect on worker's future production. MN prescribes information sharing and changes the property rights over information. However, employers can bypass MN by replacing AN

[1] The Worker Adjustment and Retraining Notification (WARN) act requires large employers to provide two months of advance notice. The U.S. has one of the least generous MN institutions in the OECD countries. However, some states extend the federal MN, however. For example, the MN period in New York and New Jersey is 90 days.

[2] This is in the spirit of Lazear, 1990 who pointed out that private contracts can counteract mandated severance.

[3] Autor et al. (2007) study the effect of dismissal costs and Bjuggren (2018) a reform of last-in-first-out rules.

with severance.[4] A key insight of the theory is that employers share information when workers' willingness to pay for notice exceeds firms' current losses; otherwise, they avoid information sharing using severance. Coasian bargaining thereby undoes the inefficiency created by the policy when losers can compensate the winners (Kaldor-Hicks compensation principle). Nevertheless, MN may still distort production by inducing firms to delay separation beyond what would be optimal. When firms can use severance to bypass the mandate, delays are the only production inefficiency associated with the policy.

We empirically examine these theoretical insights, using novel administrative data from Sweden on individual-level notice periods. Additionally, we devise a methodology for inferring severance payments from annual earnings and employment spell data, which we validate using survey data.

We divide our empirical analysis into four parts. The first part exploits a discontinuity in MN eligibility. Workers older than 55 at the time of notification receive an MN extension according to the Swedish collective bargaining agreements. We find that this extension lengthens AN *and* leads to larger severance. In light of the theory, the rise of AN implies that private contracts are not efficient, while the replacement of notice by severance constitutes evidence that side-payments mitigate inefficiencies associated with information sharing.[5] Longer MN yields smoother transitions across jobs for displaced workers: they experience shorter non-employment and obtain jobs with higher wages. Interestingly, the MN wage gain occurs mainly among workers who transition to a new job without experiencing unemployment. MN thus increases total post-notification earnings through several channels: shorter non-employment, larger severance, and higher subsequent wages contribute 59%, 27%, and 14% to the earnings increase, respectively.

The second part of our empirical analysis ventures beyond these reduced-form effects of MN and *separately* estimates the effects of extending AN and of larger severance. We introduce a second source of exogenous variation arising from spillovers across workers within the same layoff. In fact, displaced workers younger than 55 receive larger severance when laid off with coworkers who are older than 55. In practice, the individuals' own age relative to the MN discontinuity identifies the effect of advance notice, while the share of laid-off coworkers above age 55 identifies the effect of severance. This 2-IV design indicates that AN lowers the exposure to non-employment and increases future wages. By contrast, severance does not impact future wages and only lengthens non-employment duration.[6]

Two aspects of our results are striking. First, the timing of finding a new job is unaffected by an extension of the notice period. This result contrasts sharply with the evidence from the literature on UI, where more generous UI delays job-finding rates (e.g., Krueger and Meyer, 2002). Second, the wage effect – 1.7% higher wages for an additional month of AN – is an order of magnitude larger than the wage effect of UI duration estimated in prior work (Nekoei and Weber, 2017). Based on these findings, we hypothesize that job search is more effective while employed than while unemployed,

[4] Usage of bilateral agreements to replace notice with severance is legal in both Sweden and the U.S. See Lagen (1982:80) om anställningsskydd, 2§ for Sweden.

[5] The use of severance instead of MN implies that the employer's willingness to pay exceeds that of employees, whereas the opposite is true when MN increases AN.

[6] The positive severance effect on non-employment is dwarfed by the negative effect of notice on non-employment. As a result, the overall effect of MN is a reduction in the duration of non-employment.

and that this difference is a key component of the wage effect.

The third part of the empirical analysis tests this hypothesis. We measure job search activities from two complementary and novel data sources. The Labor Force Survey asks a sub-sample of individuals about whether and how they search for a job. The Public Employment Service provides population-wide daily records of job seekers' contacts with their assigned caseworker. We exploit our 2-IV design and estimate that one additional month of AN – compared to the counterfactual of one month of UI – reduces search, raises re-employment wages, but does not delay job finding. This finding implies that it is more effective to search from employment than from unemployment.

Employed job search is more effective than unemployed search for two reasons: it has a stronger impact on job finding – i.e., a higher "relative return to search" – and it enables workers to target high-quality jobs without experiencing significant declines in job finding. Because of the wage effect, the 2-IV strategy provides a lower-bound estimate of the relative return to search. Nevertheless, the estimated lower bound implies a higher relative return to employed search. We bolster this finding in two ways. First, we compare the difference in the relation between search and job finding rate for the same individual once employed and once unemployed. Again, we find a higher return to job search during employment than during unemployment. Second, we exploit two empirical strategies generating exogenous variation in the incentives to search: one for the unemployed and another for the employed. For the unemployed, we use a Regression Kink Design that exploits the ceiling of the UI scheme (Card et al., 2015; Kolsrud et al., 2018). For the employed, we leverage variation in the legal MN duration due to tenure and age. All together, these results show that an increase in search effort yields a larger increase in job finding among the employed than among the unemployed.

To summarize, we offer three pieces of evidence in support of the conclusion that job search during employment is more effective than job search during unemployment. Our estimates are closely aligned with one another: the ratio of the return to job search among the employed and the return among the unemployed ranges from 1.12 to 1.25, with the confidence intervals excluding unity.

The fourth part of our empirical inquiry centers on the costs of MN. We first estimate the productivity loss of notice. We compare revenue per worker among firms that are exposed to similarly sized layoff shocks but differ in the advance notice duration of their laid off workers. Because advance notice duration is endogenous, we instrument for it with the duration that would have occurred had the firm followed the MN rules as well as the last-in-first-out rules prescribing that workers should be notified by an inverse order of seniority. A causal interpretation of this analysis requires the assumption that the counterfactual productivity evolution is the same for firms with workforces differing in de jure notice duration.[7] The fact that the pre-notification evolution of the marginal revenue product is unrelated to subsequent average notice duration across firms supports this assumption. Our estimates suggest that productivity falls by roughly one third when the worker is notified of layoff. We then assess the production losses due to the delay of separations induced by MN. Our empirical analysis suggests that separations are indeed inefficiently delayed. To quantify the cost of delay, we estimate foregone earnings using variation from the last-in-first-out rule and

[7]Identifying the costs of MN requires stronger assumptions than for the benefits, since we cannot employ an age-based RD design at the individual level. Instead, we leverage the implied variation in MN from multiple discontinuities due to the firm's workforce composition.

consecutive within-firm layoff events. We conclude that the MN extension induces a loss due to delay corresponding to around a third of monthly earnings.

Finally, we combine all the empirical pieces using our theoretical framework in order to compare the size of the production gains and losses of MN. The production gains – equivalent to 1.2 months of earnings – consist of the positive wage effect and the shorter duration of non-employment, which should be compared with the productivity losses of notice and the cost associated with the delay of layoffs. We find a net economically significant positive impact, albeit statistically insignificant, of the MN extension on average production in our setting. Moreover, the positive impact on production contributes to a positive fiscal externality, which is further enhanced by a reduction in UI expenditure.[8]

Our paper contributes to several strands of the literature. Theoretically, we focus on a new mechanism: MN mitigates a market failure due to information asymmetries and induces firms to share private information with its employees. The previous literature has either focused on the insurance properties of the policy (e.g., Pissarides, 2001 and Ifergane, 2022) or modeled MN as a commitment device for firms (see Kuhn, 1992).[9] Our theory also allows firms to circumvent MN using side-payments. This idea relates to Lazear (1990), who argues that private contracts will fully undo mandated severance in the first best. In contrast, we consider a second-best setting where the policy addresses a market failure. In such settings, private contracts only undo the policy if it is Kaldor-Hicks inefficient. Therefore, Coasian bargaining increases the efficiency of the policy, and increases the scope for policy intervention in the second best, as opposed to being an argument against any policy intervention in the first best. Empirically, we offer direct evidence of side-payments being used to undo regulations. To the best of our knowledge, we are the first to theoretically emphasize the importance of side-payments for enhancing efficiency and to empirically document the occurrence of such transactions.

Previous empirical evidence on MN primarily relates to the introduction of the WARN act of 1988 (see, e.g., Ruhm, 1992, 1994, Burgess and Low, 1992 and Jones and Kuhn, 1995). Papers in this literature mainly compare workers in firms subject to the WARN act to workers who are not in such firms, controlling for a rich set of covariates. We extend this literature by providing new estimates of the benefits of MN, as well as the underlying mechanisms, using state-of-the-art quasi-experimental methods. We also contribute to a small literature that estimates productivity losses of MN. Kuhn and Yu (2021) find that team productivity falls during the notice period while Alfitian and Vogelsang (2022) document that laid-off workers' absenteeism rises sharply around the time of notification. By focusing on firm revenue, our approach estimates the overall costs of MN. Our findings on the effectiveness of on-the-job search complement Blau and Robins (1990) and Faberman et al. (2022). An advantage of their evidence is that they use surveys that ask about the offer arrival rate. In comparison to this literature, our estimates are arguably more robust to compositional differences

[8] Our estimation of the effect on total production ignores general equilibrium effects. For instance, we assume that the wage gains of MN are not at the expense of other workers or other production factors.

[9] Pissarides (2001) shows that advance notice can be a part of an optimal contract, as it effectively provides an alternative insurance mechanism for workers. Ifergane (2022) examines whether an optimal policy package involves advance notice and UI. Kuhn (1992) models a scenario where a promise to share information with workers is not credible without a mandate. A firm which is not hit by a negative shock thus signals its viability by raising wages. This is not necessary with mandated notice, and the mandate thus raises profits for firms.

across the employed and the unemployed.

The paper unfolds as follows. Section 1 provides a framework that guides our empirical analysis as well as an analysis of the optimal length of the mandate. Section 2 describes the institutional setting and the data. Section 3 provides our empirical analysis of the benefits of MN. It also separates the effects of advance notice and severance pay. Section 4 offers estimates of the relative effectiveness of searching from employment compared with unemployment. Section 5 contains our empirical analysis of the costs of MN. Section 6 connects the empirical results to the theory and calculates the net impact on production of MN. Section 7 concludes.

1 Conceptual Framework

This section theoretically examines how MN influences the behaviors of workers and firms, with a particular focus on the impact of MN on production efficiency. We conceptualize MN as a remedy for a market failure resulting from information asymmetry. MN encourages certain firms to share information, while others sidestep the mandate through bilateral agreements that compensate workers for the loss of AN. In this context, we demonstrate that MN has the potential to enhance efficiency.

That mandated information sharing can improve productive efficiency is an idea with broad applicability. We start with an abstract example to illustrate the essence of the idea – thus highlighting its relevance to other contexts, such as bankruptcy laws that mandate timely information sharing with investors.

Consider a one-shot relationship involving two agents, whose default payoffs are $y_1 > 0$ and $y_2 > 0$ and outside options are zero. With probability θ, the payoffs fall to $y_1' > 0$ and $y_2' < 0$. The first agent privately observes the payoffs fall, but chooses not to disclose this information as $y_1' > 0$. In the first-best, information is shared if $y_1' + y_2' < 0$. The second agent stays in the relationship by default, but walks away if informed as $y_2' < 0$.[10] Mandating information sharing gives the uninformed agent the property right over information, and presents the informed agent with two options: share the information or compensate the uninformed agent ex post with a payment of $S = -y_2'$. The informed agent shares the information when $y_1' - S < 0$ or $y_1' + y_2' < 0$. The mandate thus ensures information sharing if and only if it is efficient.

This section applies this general idea to the case where the firm has private information on an impending lay-off. We convey the main message as simply as possible, relegating the details and extensions to Appendix A.

1.1 The Setting

Consider a two-period model. At the beginning of the first period, every worker is matched with an employer. At that moment, wages are set and remain fixed for both periods with no possibility of renegotiation. There is free entry of employers, so a zero-profit condition determines wages. It is common knowledge that productivity in the second period may fall by a factor of z with probability

[10] We thus assume that expected utility is positive: $(1-\theta)\, y_2 + \theta y_2' > 0$.

θ. The combination of a fixed wage and stochastic productivity leads to the possibility of layoff in response to a productivity fall, as in Blanchard and Tirole (2008).

Just after setting wages, the employer receives private information about the productivity of the match in the second period. The information is verifiable, so the employer may share it with the worker by providing AN at the start of the first period.

A notified worker searches on the job during the first period. She applies to one job, knowing that the likelihood of getting the job is decreasing in the wage: $\lambda^e = \phi^e(w)$, where w denotes the "target wage". On-the-job-search of the notified worker, combined with the lower incentive to exert effort (shirking), reduce first-period productivity in the notifying firm by a factor of α. There is heterogeneity in α across firms. Before start of the second period, laid-off workers searches for a new job with the job-finding rate of $\lambda^u = \phi^u(w)$.[11]

In the following, we parameterize the difference in the likelihood of finding a job between the employed and unemployed as $\phi^u(w) = \phi(w, 0)$ and $\phi^e(w) = \phi(w, \eta)$, where $\frac{\partial \phi}{\partial w} < 0$. We assume $\frac{\partial \phi}{\partial \eta} > 0$, so that η captures the relative effectiveness of employed and unemployed search; there is heterogeneity in η across individuals.[12]

As our focus is on production efficiency, we assume that workers are risk neutral. Another assumption is that private contracts do not include AN.[13] This assumption is similar to those used in the UI literature (Baily, 1978 and Chetty, 2006), where private contracts do not include UI. In keeping with the UI literature, we take as given the existence of frictions that prevent the optimal provision of notice in private contracts.[14] We thus examine whether MN improves outcomes in the second-best. Our empirical results support this assumption as we show that MN does increase the notice period.

1.2 MN Effect on AN and Severance

This section studies an extension of MN. In the absence of MN, the equilibrium wage is $w = y$. Firms always lay off workers between the two periods in case productivity drops in the second period, and the separation is efficient. But no firm provides notice, and this decision is not always efficient. Firms choose not to notify the worker as they bear the entire cost of notification, and do not internalize the benefit to workers. The cost to firms is αy, i.e., the reduction in the productivity due to notification and the benefit to workers is the expected increase in utility, which we denote by $\sigma \equiv U^n - U^u$. The heterogeneity in the effectiveness of employed search (η) implies heterogeneous σ. The decision to not provide notice is inefficient when the benefit to the worker outweighs the cost to the firm, $\sigma > \alpha y$. This inefficiency may justify policy intervention.

[11] There is a close connection between the directed search model here and a model with random search. For example, a directed search model with a job finding rate that is linear in the target wage, $\phi(w) = 2(1-w)$ and a random search model with offers from a uniform distribution imply the same expected job quality and job finding rate. This equivalence is a general feature of the two models (Nekoei and Weber, 2017).

[12] For convenience, we restrict the space of heterogeneity so that an un-notified worker (given her prior) does not search in the first period.

[13] Our findings remain unchanged when the contract space includes severance. Since all agents in the economy are risk-neutral, we ignore UI . We introduce search effort later on in the paper.

[14] A way to rationalize this is to assume that workers over-estimate the stability of their jobs; while we lack such empirical evidence, the results in Spinnewijn (2015) suggest that unemployed are overly optimistic regarding their employment prospects .

In the presence of MN, the firm has three options. One option is to comply with the mandate and notify the worker. This reduces profit by αy due to the lower productivity of notified workers. Another option is to disregard the mandate and instead compensate the worker ex post by paying severance. The amount of severance is equivalent to the gain to the worker from notice, σ. The firm thus provides notice if $\sigma > \alpha y$, and sidesteps the mandate by using severance when $\sigma < \alpha y$, i.e., it shares information when it is efficient.

But there is an efficiency loss associated with the policy. A distressed firm also has the option of delaying the layoff to avoid the productivity loss of notice during the high-productivity period. Delayed separation is a loss, since it would be efficient to separate at the start of the second period if productivity falls.

In equilibrium, the firm's choice of layoff policy must be incentive compatible, conditional on the wage. The equilibrium wage, and the implied layoff policy, is pinned down by free entry. We show in Appendix Section A that the firm's optimization problem can be thought of as choosing the option which has minimum cost, where the cost associated with each course of action is: $\kappa^N = \alpha y$, $\kappa^S = \sigma$, and $\kappa^D = \left(1 - \frac{\theta}{2}\right) zy$, where the superscripts denote the three options: N for notification, S for paying severance, and D for delay.

The likelihood that the firm will delay notice is increasing in the productivity loss of notice, and decreasing in the size of the productivity shock.[15] We denote the set of firms that provide notice, pay severance, or delay separation by $\Omega^i = \left\{\min\left(\kappa^N, \kappa^S, \kappa^D\right) = \kappa^i\right\}$ and their share by $P^i = \mu\left(\Omega^i\right)$ for $i \in \{N, S, D\}$, where the distribution of types (α, σ) is a measure μ.[16]

1.3 MN Effect on Wages, Employment, Earnings, and Production

An unemployed worker chooses the target wage so as to maximize expected utility, $U^u = \lambda^u w^u + (1 - \lambda^u)\, b$, where b denotes home production.[17] Notified workers choose a target wage to maximize utility, knowing that if they do not find a job while employed, they can search again as unemployed, $U^n = \lambda^e w^e + (1 - \lambda^e)\, U^u$. The employment rate among notified workers in the second period equals $\lambda^n = \lambda^e + (1 - \lambda^e)\, \lambda^u$, while the expected wage in the new job is $w^n = \frac{1}{\lambda^n}\left[\lambda^e w^e + (1 - \lambda^e)\, \lambda^u w^u\right]$.

Wage effectThe introduction of MN increases the expected re-employment wage by $\Delta w \equiv w^n - w^u$ for workers who are notified, i.e., workers who belong to Ω^N. We decompose this wage increase, using linear approximation, into two components: one is due to the relative effectiveness of employed job search; the other is due to notified workers having an extended search period, that is,

$$\Delta w \simeq \frac{1}{\xi}\frac{\lambda^e}{\lambda^n}\left[\underbrace{\eta\frac{\partial \phi}{\partial \eta}}_{\textbf{Relative effectiveness}} - \underbrace{\left(U^u - b\right)\frac{\partial \phi}{\partial w}}_{\textbf{Extended search}}\right] \tag{1}$$

[15]Our RD estimates will not capture equilibrium wage adjustments. In partial equilibrium with exogenous wages, the cost associated with delay can be written as $\kappa^D = w - (1 - z)\, y$.

[16]Our main findings are unaffected by alternative sources of heterogeneity.

[17]We assume that layoff is optimal after a productivity drop, i.e., $U^u > (1 - z)\, y$.

where $\xi > 0$ denotes the negative of the second order condition for worker optimization. If employed search is more effective ($\eta > 0$), notification unambiguously improves outcomes. Otherwise, notification involves two countervailing forces: it induces workers to seek higher-wage jobs while employed, but reduces wages by increasing the likelihood of unemployment search.[18]

Employment effect For workers who are given notice, the introduction of MN decreases the exposure to unemployment by $\Delta\lambda \equiv \lambda^n - \lambda^u$, which can be decomposed in a similar fashion as above. One component is due to an to extended period of search, that is $\lambda^u(1-\lambda^u)$. The other component is due to the relative effectiveness of employed job search, $\lambda^e - \lambda^u$. We thus have

$$\Delta\lambda \simeq \underbrace{\lambda^u(1-\lambda^u)}_{\textbf{Extended search}} + \underbrace{\left[\eta\frac{\partial\phi}{\partial\eta} + (w^e - w^u)\frac{\partial\phi}{\partial w}\right](1-\lambda^u)}_{\textbf{Relative effectiveness}} \tag{2}$$

In contrast to the MN wage effect, the employment effect is ambiguous in sign, even if $\eta > 0$. The reason is that advance notice encourages workers to target higher quality jobs ($w^e > w^u$), thus reducing the job-finding rate.

Effect on earnings, welfare, and production MN affects earnings (Y) by increasing the income of those who receive notice, by severance payments, and by changing the income of workers with delayed separation, that is

$$\Delta Y = \underbrace{(\lambda^n w^n - \lambda^u w^u)\,P^N}_{\text{Increased notice}} + \underbrace{\sigma\Delta P^S}_{\text{Increased severance}} + \underbrace{\left(w^D - \lambda^u w^u\right)P^D}_{\text{Delayed layoff}} > 0 \tag{3}$$

where $w^D = \mathbb{E}(w|\Omega^D)$ is the average wage in firms that delay notice in response to the mandate. Using this equation, we can decompose the effect of MN on earnings into four intuitive components that we estimate separately: the change in non-employment duration, $\Delta NE = -\left(P^N\Delta\lambda + P^D(1-\lambda^u)\right)$; the change in the duration of the new job, $\Delta L = P^N\Delta\lambda - P^D\lambda^u$, multiplied by the change in wages due to displacement; the effect on the re-employment wage; and the severance pay effect.[19]

$$\frac{\Delta Y}{w^D} = \underbrace{-\Delta NE}_{\text{non-employment dur. effect}} - \underbrace{\left(\frac{w^D - w^u}{w^D}\right)\Delta L}_{\text{new job dur. effect}} + \underbrace{\lambda^n\frac{\Delta w}{w^D}P^N}_{\text{wage effect}} + \underbrace{\frac{\sigma}{w^D}P^S}_{\text{Severance effect}} \tag{4}$$

We then examine the effects of MN on aggregate production and social welfare. In our model, aggregate production equals labor income, while Utilitarian social welfare is the sum of aggregate (market) production and home production.[20] For this analysis, we extend our model in the Appendix

[18] This is similar to the UI effect on expected job quality. Here, the negative duration dependence comes from the drop in search effectiveness.

[19] As noted above, our RD estimates will not capture equilibrium wage adjustments. In a partial equilibrium setting, an analogous decomposition applies. The only difference is that the normalization is with respect to the wage in the notifying firm.

[20] This is because our model features risk-neutral agents, and abstracts from disutility of search and work.

to allow for a mixed strategy for firms. The firm chooses the probability of sharing information, given the mandated minimum probability (m). This extension also enables us to study a marginal MN extension and, thus, the optimal duration of the mandate.

A marginal MN extension has two effects on production. First, it extends AN for the sub-set of the population already receiving notice (infra-marginal effect). Second, since the cost of providing notice increases, marginal workers are transferred across states: some marginal workers are not receiving notice anymore, and are thus moved from state N to states S and D, while other marginal workers are moved from state S to D, i.e., from receiving severance to experiencing a delay in notification.

At the margin, moves from notification to severance are not relevant as severance compensates the worker exactly. The same is not true for the marginal workers who are pushed from notice or severance to delay, where there is a discontinuous reduction in welfare as delays are inefficient. The impact on social welfare (V) of a marginal extension of MN can be written as:

$$\frac{dV}{dm} = \underbrace{P^N \, \mathbb{E}\left(\sigma - \alpha y | \Omega^N\right)}_{\substack{\text{Net Production gain of info sharing} \\ \text{Infra-marginal}}} \quad \underbrace{- \frac{\partial P^D}{\partial m} \, \mathbb{E}\left(\widetilde{w} - w^D | \partial\Omega^D\right)}_{\substack{\text{Net Production loss of delaying} \\ \text{Marginal}}} \tag{5}$$

where $\widetilde{w}$ is post-displacement earnings for marginal workers and $\partial\Omega^D$ denotes matches at the boundary of delay. The first expression on the right-hand side is unambiguously positive since notice is given when $\sigma \geqslant \alpha y$. In other words, firms provide notice if and only if information sharing is production efficient. The ability of firms to pay severance is crucial: although the payment of severance has no effects on aggregate production, the possibility of paying severance implies that firms avoid inefficient notice. The second expression on the right-hand-side is negative since separation is efficient in the second period. In general equilibrium, the efficiency loss is proportional to the difference between the re-employment wage and the wage in jobs where separation is delayed. Intuitively, $\widetilde{w} \geqslant w^D$, since equilibrium wages must be low in firms that would delay when hit by a negative productivity shock.

In the remainder of the paper, we provide empirical content to this conceptual framework. One major objective of our empirical analysis is to examine whether firms respond to MN by paying severance – a key component in the efficiency case for MN (Section 3.1). Section 3.1 also provides estimates of the wage and employment impacts of MN (equations 1 and 2). Section 4 offers direct evidence on the relative effectiveness of searching from employment compared to unemployment. In Section 3.1, we estimate the overall earnings effect (equation 3) and decompose it by estimating all components of the right-hand side of equation (4). The final part of the paper (Section 6) is based on equation (5) (while assuming home production is zero); we compare the estimate of the expected production gain of giving advance notice with an estimate of the productivity drop associated with having workers on notice, as well as the production loss of delay.

2 Institutional Setting and Data

2.1 Institutional Setting

The Swedish Employment Protection Law (Lag 1982:80 om anställningsskydd, 6c§) stipulates that a firm intending to lay off a worker must give *written* notice to the worker in advance. The length of the MN period increases discontinuously with tenure, from a minimum of 1 month for employees with less than 2 years of tenure, to a maximum of 6 months for workers with at least 10 years of tenure. Alternative rules can be agreed upon in collective bargaining agreements. For instance, many white-collar agreements within the manufacturing sector stipulate that workers above age 55 with 10 years of tenure get an additional 6 months of notice.[21] Neither the law nor these agreements include mandatory severance. That said, notified workers may agree to compensation packages involving advance notice and severance if it is perceived as more generous than the default rules. Moreover, workers with at least six months of tenure are eligible for 60 weeks of UI, replacing 80% of their earnings up to a cap when unemployed.[22]

Ideally, we would match the information on notice periods from the collective agreements to notified individuals. However, our micro data do not include information on which collective agreement a worker belongs to at a given point in time.[23] In our main empirical analyses, we focus on the age-55 threshold for all white-collar workers in the private sector because age is more precisely measured than tenure in our data.[24] In auxiliary analyses, we also use variation coming from the tenure thresholds.

A firm planning to lay off at least five workers at the same plant within three months must report to the Public Employment Service (PES). First, the firm reports the number of workers that it intends to displace along with the reason for downsizing. Upon this initial layoff report, the firm enters negotiations with the labor unions regarding who to lay off, respecting the last-in-first-out (LIFO) principle that requires workers with shorter tenure to be laid off first conditional on occupation type (Cederlof, 2023). The result of these negotiations is a list containing the identities of the notified workers and their individual-specific displacement dates. This list must be submitted to the PES at least two months before the first displacement date, and on average, it arrives 2.5 months after the initial layoff report.

[21] We have collected information from all major collective agreements on the Swedish labor market for the relevant time period (2005-2016). Other age rules do exist but they are much less prevalent. The white-collar agreement in retail trade during 2004-2007, for instance, determines MN duration as a combination of age and tenure. For given tenure, notification periods are prolonged discretely at age 25, 30, 35, 40, and 45. This age proviso was later changed to the age-55 rule described in the main text.

[22] Unlike most other countries, Sweden has a workfare-type system where unemployed workers can continue receiving unemployment benefits beyond the 60 weeks conditional on participation in counseling and other labor market programs.

[23] In principle, imputing the collective bargaining agreement that a worker is covered by may be possible using industry and occupation codes. In practice, the match between occupation by industry cells and CBAs is not perfect, as those cells can be matched to multiple agreements.

[24] Sometimes the start and end dates of the employment spells are reported erroneously in the data, which leads to a measurement error in tenure.

2.2 Data and Estimation Sample

Our main data source is the administrative register of all notifications reported to the PES during 2005-2016. For each worker, we measure the de facto advance notice period as the duration between the notification date and the reported displacement date.[25] We assume that the notification date also corresponds to the date when the worker learns about her future displacement.[26]

We match the notification register with six other administrative data sets: (i) RAMS contains the universe of matches between employers and employees, and includes information on earnings and employment spells; (ii) LISA contains individual-level characteristics; (iii) the Wage Survey comprises the employer-provided full-time-equivalent monthly wage, measured in September-November for all workers in the public sector and around half of all private-sector workers. The wage measure includes all fixed-wage components, as well as piece rates, performance pay, and fringe benefits; (iv) the Unemployment Spell Register from the PES provides the duration of unemployment spells and information on contacts with the PES caseworkers; (v) the Income Statement and Balance Sheet Register contains information on employers' revenue; (vi) the Labor Force Survey (LFS) contains information on individuals' labor market status and job search for a 0.4% sample of the population aged 15-74. The availability of all of these data sources extends beyond the 2005-2016 period covered by the notification register, such that we can observe both pre- and post-notification outcomes.

Population and Descriptive Statistics Our population comprises all individuals who were Swedish residents at some point during 2005-2016. As our first research design exploits a discontinuity in MN at age 55, the baseline estimation sample restricts attention to white-collar workers who are present in the notification register and are aged 52-58 at the time of layoff. We also remove workers laid off in bankruptcy events, since the collective bargaining agreements do not apply in those cases.

Table 1 presents descriptive statistics for notified workers overall (Column 1), notified workers in the baseline estimation sample (Column 2), as well as for two comparison groups (all workers in Column 3 and all workers reweighting the industry shares to match notified workers in 4). Panel (a) shows individual-level characteristics, while panel (b) presents firm-level statistics.

Notified individuals differ substantially from employed workers in almost all dimensions (c.f. Columns 1 and 3). However, most of these differences are driven by notifications being more common in certain industries, such as manufacturing and construction. When we control for industry – c.f. Columns (1) and (4) – only two differences remain. First, firm size differs mechanically across the two columns since the data contain notifications involving at least five workers. Second, educational attainment is lower among the notified, in part because higher educated workers have a lower risk of being laid-off. The individuals in our estimation sample have higher wages, earnings, and educational attainment than the average notified individual (c.f. Columns 1 and 2).

[25] For the vast majority (84%) of workers, the individual notification date corresponds to when the list arrives at the PES.

[26] Figure 5 of Section 4 shows evidence in support of this assumption. While reported job search increases discontinuously upon the initial layoff report, the increase in search activity is only differential across individuals with different notice periods after the notification date. This suggests that individuals are unaware of the impending notification before the individual notification date.

3 Benefits of Mandatory Notice (MN)

3.1 Estimating the Effect of MN using a RD Design

Our first identification strategy exploits the discontinuity in the duration of mandatory notice at age 55 for white-collar workers in the private sector using the following equation:

$$y_i = \beta \mathbf{1}(A_i \geqslant 55) + g^0(A_i - 55) + \mathbf{1}(A_i \geqslant 55) \times g^1(A_i - 55) + \delta \mathbf{X_i} + \varepsilon_i, \tag{6}$$

where A_i denotes age at notification for individual i, and $g^k(.)$, $k = 0, 1$, are age control functions. Our main analysis uses data for individuals who are aged ± 3 years relative to the age-55 threshold. This corresponds closely to the optimal bandwidth according to Calonico et al. (2014) (see Appendix B.2). Since age is discretely measured in months we rely on a parametric control function.[27] Our default specification has a linear control function interacted with the threshold. In addition, $\mathbf{X_i}$ denotes baseline controls for a number of pre-determined covariates and month-by-year fixed effects to increase precision.

The estimated β of equation (6) represents the effect of longer MN on outcomes given the absence of (i) other laws featuring a discontinuity at age 55 at the time of notification (exclusion restriction) and (ii) manipulation of age at notification (random assignment). Appendix Section B.1 shows both that the distribution of notified individuals is smooth and that pre-determined covariates are balanced around the threshold.

3.1.1 MN Effect on AN and Severance

We first investigate the effect of the MN extension on AN or, in other words, the effect of de jure notice on de facto notice, respectively. Figure 1a shows that being just above age 55 at the time of notification increases AN by about 2.6 months from a base of 6.7 months. The observed positive effect of MN on AN means that private contracts do not incorporate efficient notice, suggesting that MN increases efficiency as the framework in Section 1 predicts.

Controlling for baseline covariates and displacement event fixed effects changes the point estimates only marginally (Appendix Table A6), suggesting that baseline characteristics are similar on either side of the threshold both within and across notifications (Appendix Table A1). However, a more flexible polynomial control function in age at notification reduces the point estimate. For instance, when controlling for a second-order polynomial, the discontinuity estimate falls by a month. Appendix C shows that this reduction is an artifact of measurement error in notification dates, which translates into measurement error in age at notification.[28] Appendix C also shows that the parametric

[27] Since we observe month of birth rather than day of birth, we cannot determine whether someone who turns 55 in the notification month is above 55 or just below 55. We therefore exclude observations exactly at the cut-off.

[28] Appendix C further analyzes the consequences of measurement error. It shows, inter alia, that measurement error in notification dates makes a discontinuity appear like a non-linearity (as observed in Figure 1a). Moreover, measurement error in the assignment variable complicates a non-parametric RD approach. Intuitively, as measurement error increases, the relationship appears to be more non-linear close to the threshold, which erroneously reduces selected optimal bandwidths. Non-parametric RD thus places greater emphasis on the portion of the data that is most affected by the measurement error. Finally, Appendix C also presents donut RD estimates, which, as intuition suggests, tend to be larger in absolute value than the conventional RD estimates because the donut RD discards the portion of the data that is most affected by measurement

RD is less susceptible to the measurement error problem. Because of these issues, we estimate a linear interacted control function.

The conceptual framework outlined in Section 1 suggests that MN will be substituted by severance pay when losses for firms of having workers on notice exceed the compensation demanded by workers. We test this prediction by examining the effect of longer MN on severance.

Measuring severance is a major challenge in any tax-based administrative data. As severance is taxed as labor earnings, it is reported together with other earnings. We overcome this challenge as follows. We initially predict the portion of annual earnings due to regular wage payments in the year of separation. Severance is then measured as annual earnings net of the predicted annual wage payments. For example, if a worker separates in April, we construct severance by subtracting four months of monthly earnings – imputed from the previous year adjusted for average growth in the economy – from total earnings received from the displacing employer.[29]

We validate the imputed severance by comparing it to survey data where firms report total severance made to their employees. The imputed and reported severance are strongly and positively correlated (see Appendix Figure A10). When regressing imputed severance on reported severance we obtain a coefficient of 1.07 (standard error 0.012). Imputed severance overestimates reported severance slightly, presumably because the imputed measure includes other components that are paid out at the end of the spell, such as accumulated overtime. When estimating the effect of MN on severance, this is not an issue since such other payments are balanced around the threshold.

Figure 1b shows that severance increases by 17 kSEK at the age-55 threshold.[30] This estimate corresponds to 53% of a monthly wage.[31] This evidence shows that agents make private agreements to undo public regulation, at least to some extent, as pointed out by Lazear (1990). To our knowledge, this is the first paper that documents the prevalence of such transactions.

3.1.2 MN Effect on Employment Status

Figure 2 examines how MN affects the transition process for workers, in general, and whether longer MN shields workers from non-employment, in particular. Panel (a) presents the overall labor market dynamics for workers aged 52-58 by showing the share in different states over time relative to notification. Workers begin to leave the firm upon notice, and around half of all notified workers are in new jobs or in non-employment after six months. Eleven percent of all notified do not leave the notifying firm within two years.[32]

Panel (b) focuses on the same labor market states but plots RD-estimates, i.e., the causal effects of longer MN on the likelihood of being in different states, along with 95% confidence intervals. Longer

error.

[29] Appendix Figure A9 illustrates our method by focusing on workers laid off in January. It shows that they receive total labor income of around 180 kSEK (SEK 180,000) in January, which is well above monthly earnings in the preceding year (around 30 kSEK).

[30] All amounts have been deflated to 2010 values. In February 2024, the conversion rates are 10.29 SEK/Dollar and 11.17 SEK/Euro.

[31] Appendix Figure A13 shows the result of a permutation test where we vary the age threshold. The exact p-value associated with the actual threshold at age 55 is 0.014.

[32] Notified workers may replace those who voluntarily left their jobs; also, market conditions may improve for the notifying firm.

MN increases the likelihood of being with the notifying firm during the first year after notice. The mirror image of this is that MN reduces the exposure to non-employment. After 12 months, these effects dissipate. Interestingly, longer MN also increases the likelihood of making an employment-to-employment transition. There is a corresponding fall in the likelihood of being employed with an intermediate spell of unemployment (an EUE-transition) due to longer MN. Note that MN has no impact on the probability of working at a new firm at any horizon after notice (see Appendix Figure A3b).

Table 2 summarizes the employment impacts over the first two years. As a result of longer MN, individuals stay in the notifying firm an additional 1.3 months. This corresponds to around half of the additional 2.6 months of AN (c.f. Figure 1a or Appendix Table A6, Column 2), suggesting that they leave the firm 1.3 months prior to their layoff date. As suggested by panel (b) of Figure 2, this effect is almost entirely offset by shorter time in non-employment (Column 3). In other words, more generous notice periods do not prolong the duration until a new job is found. This finding stands in sharp contrast with the UI literature, where more generous UI delays job finding (Krueger and Meyer, 2002). Table 2 also shows that 40% of the non-employment reduction comes from shorter unemployment duration and the remainder from spending less time out of the labor force (Columns 4 and 5).[33]

3.1.3 MN Effect on Job Quality: Wages and Beyond

The theoretical framework outlined in Section 1 posits that the length of the notification period may impact the quality of the subsequent job. To examine this hypothesis, we start by investigating the wage as a proxy for job quality. Our focus is the wage in the first new job, considering new jobs as those distinct from any job held within twelve months prior to notification, including the notifying one. To avoid selection bias, we follow the literature on the impact of UI on re-employment wages and restrict attention to the first new job within two years after notification (Card et al., 2007). The two-year time frame avoids selection because the employment effects of MN have subsided after two years (see Appendix Figure A3).

We have two distinct approaches to find the the first new job. In our first approach we use the Wage Survey to define the first new job. The resulting sample includes 3,932 individuals who are employed by a different firm than the notifying firm. Figure 3a shows that the MN extension increases wages by 2.9% (standard error 1.4%) on average (c.f. Column (1) in Table 3).

For our subsequent analysis it is vital to time job transitions precisely. In the analysis below, we estimate separate wage effects for those who make an employer-to-employer transition and for those who experience an unemployment spell in between. Moreover, we connect the wage effects to the hazard and search responses, requiring a consistent sample. These two objectives motivate our second approach to finding the first new job. We use the monthly workplace indicators available in the matched employer-employee data, and exclude observations where the worker has located another new job prior to the one recorded in the Wage Survey. The resulting sample measures employment dynamics precisely. The cost is that the number of observations fall by 30%. Despite the reduction

[33] Figure A12 in the Appendix provides graphical illustrations of the RD-estimates in Table 2.

in sample size, the wage impacts remain statistically significant and of about the same size as in the larger sample (Columns (2) and (3) in Table 3). The estimates in Column (3), for example, show that longer MN allows workers to avoid one-third of the wage loss associated with moving to a new job after notification. Whereas workers in the control group experience a wage loss of 9.3% as a result of displacement, longer notification limits the wage loss to 6.1%. The effect of being eligible for longer MN is therefore 3.2 percentage points.[34]

The wage effect of MN can stem from i) workers targeting higher wages when they search as employed due to longer notice or ii) a higher likelihood of finding a job while employed. Moreover, workers eligible for longer MN could be iii) more selective when unemployed, since they receive larger severance.[35] We fully investigate these factors in Sections 3.2 and 4. At this stage, we offer a simple decomposition to gauge the magnitude of each channel quantitatively.

The MN extension leads to 4.5% higher wages among those who make an EE-transition within six months after notice (Column 4 of Table 3); we examine this time period because longer MN has no effect on the likelihood of making an EE transition within this period (see Figure 2b). The share of the main wage effect that can be explained by a higher selectiveness while searching as employed is obtained by multiplying the Column-(4) estimate (4.5%) with the EE-transition probability for workers eligible for longer MN ($0.566 + 0.075 = 0.641$ according to Column 5):

$$\Pr(\text{EE})\,\mathbb{E}(\Delta \ln w|\text{EE}) = 0.641 \times 4.5\% = 2.9\% \tag{7}$$

The decomposition thus suggests that 91% (2.9/3.2) of the wage effect stems from the wage effect when making an EE-transition.[36] The other two channels are thus quantitatively unimportant.

Another question is whether the wage effect stems from matches with higher-paying firms or from higher wages conditional on a firm match. We examine this issue in Appendix Table A4 by studying the effect of MN on different firm characteristics in the new job, for example, the average wage, firm size, value added, and profits. For most of these dimensions, we do not detect statistically significant effects. However, we do observe a positive and statistically significant effect of MN on subsequent firm size (Appendix Figure A7). Being eligible for longer MN increases average firm size in the new job by around 1.7%. This indicates that part of the wage effect may be caused by new employment in better firms. Consistent with this, workers eligible for longer MN find a firm where average wages are 1.4% higher; albeit this effect is not statistically significant.

We also investigate the effect of MN on other characteristics of the new job (see Appendix Table A5). Although we do not have statistical power, all results suggest a positive impact of MN. In particular, eligibility for longer MN reduces the probability of changing industry or occupation, and increases tenure in the new job. Overall, the non-wage characteristics of the new job are positively

[34]The Appendix reports additional evidence regarding the MN wage effect. For example, Appendix Figure A5 (b) shows the result of a permutation test where we vary the age threshold. The exact p-value associated with the actual threshold at age 55 is 0.011. Appendix Figures A5 (c)-(d) show that workers eligible for longer notice avoid large wage losses and have a higher chance of experiencing a large wage increase. Appendix Figure A6 pursues the same theme by showing the distributional impact following Nekoei and Weber (2017).

[35]The change in log wages can be decomposed into: $\Pr(\text{EE})\,\mathbb{E}(\Delta \ln w|\text{EE})$, $\Delta \Pr(\text{EE})\,[\mathbb{E}(\ln w|\text{EE}) - \mathbb{E}(\ln w|\text{EUE})]$ and $\Pr(\text{EUE})\,\mathbb{E}(\Delta \ln w|\text{EUE})$.

[36]In the full decomposition, we conclude that 4% of the wage effect is tied to the increase in the probability of making an EE-transition, and 5% is due to a wage impact conditional on an EUE-transition.

but imprecisely affected by MN – a finding consistent with the UI literature (see Nekoei and Weber, 2017, for example).

Finally, we estimate the wage effects dynamically. Figure 3b depicts the evolution of wages for those just above and below the age-55 threshold, independently of whether they are at the notifying firm, the first subsequent job, or any other job. In general, the wage differences between the two groups have two sources: wage changes associated with a move to another firm, and differential rates of remaining at the notifying firm. The patterns convey two messages. First, most differences between the two groups manifest during the first year after notification. Second, wages converge subsequently, with no discernible disparities across the groups after 24 months. These patterns point to the importance of finding a new job during the notice period – consistent with the results in Table 3 – and to stronger incentives for onward mobility among workers eligible for short notice, such that they climb the wage ladder faster after obtaining the first job.[37]

3.1.4 MN Effect on Total Earnings and its Decomposition

Figure 4a shows RD estimates for earnings in the calendar year after notification. Individuals just to the left of the threshold earn 361 kSEK, while individuals just to the right earn 399 kSEK. Extended MN thus increases annual earnings by 11% (38 kSEK).

Figure 4b illustrates the evolution of annual earnings in different calendar years relative to the notification event for those just above the age-55 threshold (black-circled line) and those just below (hollow-circled dashed line). It conveys several insights. First, the treatment and control groups are strongly balanced during four years prior to notification. Second, there are no discernible effects of extended MN two years after notification and beyond. A comparison of earnings at event-years 2 and -1 suggests that the earnings losses associated with displacement amount to 26.5% of pre-displacement earnings. These losses are similar in magnitude to those typically documented in the job displacement literature (see, e.g., Jacobson et al., 1993). Third, there is an increase in annual earnings during the year of notification, which is striking given that we expect earnings to fall for notified individuals. This is consistent with the existence of severance, as predicted by theory.[38]

What are the proximate drivers of this overall earnings effect? We decompose earnings according to equation (4) and focus on the first two calendar years after notification since all employment adjustments have subsided by then (see Figures 2b and Appendix A3). The earnings effect of longer MN corresponds to 1.60 months of additional pay (LHS). Most of this effect can be attributed to the non-employment effect (59% of the overall effect). Severance contributes to 27% of the earnings difference, while higher wages in the new job contribute to 14%.[39]

$$\underbrace{1.60\text{ month}}_{\text{Earnings effect of MN}} \simeq \underbrace{59\%}_{\downarrow\text{non-emp. dur.}} - \underbrace{0.4\%}_{\uparrow\text{disp. eff.}} + \underbrace{14\%}_{\uparrow\text{wage}} + \underbrace{27\%}_{\uparrow\text{ severance pay}} \tag{8}$$

[37]Consistent with this finding, Appendix Table A3 shows higher wage growth 3 years post notification.

[38]In fact, this pattern inspired as to develop the novel measure of severance; see Figure 1b.

[39]Appendix Table A7 presents additional detail regarding the decomposition.

3.2 Separating the Effects of AN and Severance

We have shown that longer MN helps workers avoid non-employment and find new jobs that pay higher wages. These reduced-form effects are due to both longer AN *and* larger severance. This section disentangles these two channels, thus isolating the causal impact of each factor on outcomes of displaced workers.

Separating the effect of longer AN from larger severance requires two instruments. In addition to the discontinuity used in Section 3.1, we leverage exogenous variation due to spillovers across individuals within layoff events. As layoff events entail bargaining between the firm and union representatives, we expect layoff packages to be extended to all workers involved in a layoff. In particular, we hypothesize that severance offers are more generous if many coworkers older than 55 are part of the layoff. Appendix Figure A14 (a) supports this intuition by showing that severance is more generous for workers aged below 55 who are laid off with a larger share of coworkers aged above 55 (and thus eligible for longer MN). Reassuringly, a falsification test suggests that these spillovers only exist for workers below age 55 (see Figure A14 (b)).

Building on this finding, we introduce the share of notified (white-collar) coworkers older than 55 as an additional instrument. This analysis assumes that the share is quasi-randomly assigned, conditional on observables. Appendix Table A9 supports this assumption and shows that pre-notification earnings do not vary with the share of displaced coworkers older than 55. The exclusion restriction prescribes that the share of coworkers older than 55 should only affect a notified worker through spillovers in terms of the layoff package.

Table 4 shows the IV estimates, along with the associated first-stages and reduced-form estimates. The estimation sample contains white-collar workers aged 52-58 that we use in the previous analyses and all white-collar workers who are notified with them. Adding the latter group increases statistical power.

The IV estimates imply that 2.6 months longer AN reduces non-employment by 1.6 months over a two-year horizon and increases wages in the first new job by 4.4%. Moreover, an increase in severance pay by 30 kSEK – around one month of salary – increases non-employment by 1.5 months but has no significant impact on wages – neither economically nor statistically.

How does the wage effect compare with prior evidence in the literature? The closest comparison comes from the literature on UI. Our wage estimates are an order of magnitude larger than the estimates of the UI wage effect. A major difference between the wage effect of advance notice and UI is that workers on notice search on the job. Any fundamental differences between search efficiency as employed and unemployed will translate into differences in the wage effect of AN compared with the UI wage effect. Moreover, workers on notice avoid the negative duration dependence associated with unemployment. UI induces workers to seek jobs with higher wages, but reduces wages by lengthening unemployment.[40] These two counteracting forces lead to an ambiguous net effect of UI on wages in line with the varying results in the empirical literature (Card et al., 2007, Lalive, 2007, Schmieder et al., 2016, Nekoei and Weber, 2017). The absence of the latter force is an additional reason why there is a larger wage effect of AN, relative to UI.

[40]See Nekoei and Weber (2017). For empirical evidence on the effect of unemployment duration on the probability of finding a job, see Kroft et al. (2013), Eriksson and Rooth (2014), Marinescu and Skandalis (2021).

Our finding that severance has no significant impact on re-employment wages lines up with our finding that the estimated wage effect is mainly associated with EE transitions (Section 3.1). The absence of an effect of severance on wages aligns with the findings in Card et al. (2007). However, our estimate of the effect of severance on non-employment is substantially larger than those in Card et al. (2007). They find a 10-day increase in non-employment following a 2-month increase in severance pay. It is difficult to pinpoint the exact reasons for the difference in results. An interesting difference is that Card et al. (2007) examine the effect of a mandatory severance program. By contrast, our estimates reflect the causal impact of severance for the sub-population who accept such an offer. According to our theory, this sub-population consists of individuals with a low value of conducting on-the-job search relative to search as non-employed.

An important result, which we have seen no counterpart of in the literature, is that an increase in the advance notice period by 2.6 months leads to a reduction in non-employment duration by 1.6 months, after accounting for the impact of severance pay. This finding aligns well with our earlier result that higher wages are primarily attributed to longer notice periods rather than to severance pay. Again, our theory sheds additional light on the magnitude of these effects. The causal impact of AN is identified in the sub-population whose value of conducting on-the-job search is particularly large. That the law allows for bilateral agreements that undo the mandate, effectively identifies the population that has most to gain from advance notice.

Longer AN thus improves the labor market outcomes of notified individuals. One hypothesis is that this is because job search while employed is more efficient than job search while unemployed (see Section 1). It is to this issue we turn next.

4 Search Effectiveness by Employment Status

This section compares the effectiveness of job search of employed versus unemployed. We first introduce two measures of job search and investigate descriptively how search responds to the news of an upcoming layoff. We then use our 2-IV strategy to estimate the relative effectiveness of job search as employed versus unemployed. We also probe the robustness of this exercise using two alternative strategies.

We use two complementary sources of information about job search activities. The Labor Force Survey (LFS) collects information from individuals regarding their job search activities. It asks about the use of particular search channels, for example visiting PES, scanning job databases, or directly contacting firms. Additionally, we leverage data from the PES, which records a job seeker's interactions with the assigned caseworker. The LFS offers detailed insights into job search patterns, while the PES data provide comprehensive coverage of the population and relies on third-party reports.

We start by introducing these data descriptively and comparatively in an investigation of how job search responds to notification. Using the LFS, Figure 5a shows that around 10% of employed individuals report searching actively for a job (line labeled "Extensive search (LFS)"). Search effort increases after the initial layoff report, and surges even further at the time of the individual notification to a level of around 30%. Search then remains persistently high.[41] All other series in Figure 5a show that individual notification yields additional search activity. However, the impact on the PES-measure is only visible after notification and not after the initial layoff report, and the magnitude of the response is muted relative to the more comprehensive LFS-measure.[42]

Figure 5b compares search intensity for workers with long and short AN periods, below and above the median of 3 months.[43] Search intensity in both groups evolve similarly until notification, suggesting that the increase in search is driven by a common perception of increased layoff risk and that individuals are unaware of the impending notification before receiving individual notice. Only after individual notification, workers with long notice search comparatively less intensely. This difference persists during the first 6 months. Extended AN thus reduces job search incentives.

Now we turn to the main goal of this section: estimating the relative effectiveness of search. For this purpose, exogenous variation in AN provides an ideal conceptual experiment. Over a fixed time frame, the alternative to receiving an additional month of search time during the notice period (i.e., while still being employed) is one month of search as unemployed. We therefore use our 2-IV approach from Section 3.2 to identify the effect of AN separately from severance. Table 4 shows that the number of contacts with the PES caseworker is reduced by 8.7% due to longer AN. [44] The estimated reduction in the duration until a new job (-0.205 on a base of 8.28 months) suggests a slight (and insignificant) increase in the job finding rate by 2.5%.[45] The striking fact is that the hazard is barely affected despite the fact that longer notice reduces search *and* increases wages. This finding implies that it is more effective to search from employment compared to unemployment.

Conceptually, job search strategies vary based on the individual's diligence in seeking a job (search intensity) and the quality of the jobs they seek (i.e., the target or reservation wage). However, the small literature on the topic (see Blau and Robins, 1990, and Faberman et al. (2022)), focuses solely on effectiveness of search intensity and studies the relationship between the job finding rate and search activity – which we refer to as the return to search – and how this relationship varies by employment status – the relative return to employed search. Our estimates from the 2-IV design suggest a relative return to employed search of $(1 + 0.025) / (1 - 0.087) = 1.123$. However, this is a downward biased estimate as it ignores that target wages increase due to longer AN (see equation 2).

To probe the robustness of this result, we begin by regressing the hazard rate on reported search effort in the LFS separately for the employed and the non-employed, adjusted for observed differences across the two groups (see columns 1 and 3 in Table 5). While simple, this OLS approach suffers from the problem that search effort is endogenously chosen. We confront this problem by exploiting that the LFS is a rotating panel where each individual is surveyed eight consecutive quarters. We can thus estimate the relation between the job-finding rate and search in a sample that is restricted

[41] The persistent increase in search intensity is most likely tied to the negative earnings impact of displacement (e.g., Jacobson et al., 1993). The literature shows that displacement causes a reduction in future employment prospects (e.g., Cederlof, 2023) as well as wages (e.g., Lachowska et al., 2020; see also Column 2 of Table 3).

[42] Registering with the PES is a precondition for collecting UI.

[43] The average duration of AN periods in these groups are 1.6 and 7.0 months, respectively.

[44] A caveat of the 2-IV design is that it uses the less granular PES search measure as we lack power using LFS measure around the age-55 threshold.

[45] Appendix Figure A16 shows direct estimates of the hazard. There is little variation in the effect of MN on the hazard over all time horizons post notice. It also shows how MN affects search over time.

to individuals who experience both states of employment and unemployment during the panel. This ensures that the composition of the two groups is held constant. In addition, we include individual fixed effects that are allowed to vary by employment status.[46] The return to search from employment is uniformly higher than the return to search from unemployment (Table 5). Our preferred specification, presented in Column (4) holds composition constant and suggest that the return to searching from employment is 25% higher than searching from unemployment.

A potential concern with the estimates in Table 5 is that they are confounded by variation in the target wage. To tackle this issue, we use two exogenous shifters of search among unemployed and employed workers where we a priori expect wage effects to be small or non-existent.[47] For the unemployed, we leverage that UI benefits are capped in a regression-kink design (Kolsrud et al., 2018). Under the exclusion restriction that target wages are unaffected by UI benefits, this provides an estimate of the causal relationship between the hazard and search activity. For the employed, we leverage variation in MN driven by the tenure and age rules among notified individuals. In particular, we use variation during the MN period. We focus on job search during the period from the initial layoff report (around -2) to $+2$ for those who were notified at $t = 0$, and were eligible for 2-6 months of MN. This population is much broader than the one considered in the main analysis. As a result, their observed characteristics are comparable to the average notified individual described in Column (1) in Table 1.

Table 6 presents the results.[48] Since the LFS is available for a small sub-sample, we increase the statistical power of the first stage by implementing a two-sample IV approach (see Inoue and Solon, 2010) and estimating the impact on the hazard in the population. For the employed, Column (1) of Panel (a) shows that being eligible for longer MN lowers search as well as the hazard to a new job. For the unemployed, Column (2) shows that a lower UI replacement rate increases search and the hazard to a new job. It implies that a one percent increase in the replacement rate reduces search by 0.4%.[49] The impact on the hazard is consistent with, e.g., Kolsrud et al. (2018). According to panel (a), the effect of these shifters on the wage in the new job is insignificant, supporting the exclusion restriction.[50] The IV estimates show that the impact of search activity on the job-finding rate as employed (unemployed) amounts to 20.7 (16.1) percentage points. These causal estimates are new to the literature. The return to searching from employment is higher than the return to searching from unemployment with an implied relative return of 1.285. We obtain the same conclusion if we replace the search indicator with a measure of search intensity. The relative return to employed search in this case is 1.190. Both estimates are statistically significant.

[46]The addition of these fixed effects do not change the estimates much. The important point is that the composition of the sample is held constant.

[47]The second row of Table 6 shows that the wage impacts are small and statistically insignificant.

[48]Appendix Figure A17 illustrates the reduced-form estimates for the RKD-design. Miika Päällysaho kindly provided the code implementing the RKD-design.

[49]We obtain the elasticity as follows. We divide the change in slopes for search (0.222 from Column (2) of Table 6) with the level at the kink (0.65 as shown in Appendix Figure A17b). We divide this estimate by the change in slopes of the replacement rate (-0.64) divided by the replacement rate at the kink (0.76).

[50]The lack of a wage effect in the analysis of the unemployed is in line with most papers in the UI literature. The lack of a wage impact of de jure MN in Column (1) of Table 6 is not entirely in line with the evidence reported in Table 3. Notice, however, that the treatment, which is driven by the tenure rules, is much smaller, and that the population is entirely different. Moreover, our analysis here focuses on search early on during the notice period.

Figure 6 summarizes the results across the three designs. Despite potential differences in the complier populations across the three designs, the results provide a consistent message: the job *finding* rate per unit of search for the employed relative to the unemployed ranges from 1.123 to 1.285. These estimates complement the survey evidence in Faberman et al. (2022) who find that the job *offer arrival* rate per unit of search for the employed relative to the unemployed is 3. While qualitatively aligned, our estimates are an order of magnitude lower. One reason for this discrepancy is that our estimates are not confounded by compositional differences between the employed and unemployed. To conclude, the beneficial impacts of mandatory notice on wages and non-employment are at least partly driven by the return to job search being higher in employment than in unemployment.

5 Costs of Mandatory Notice

While the focus of Section 3 is on the benefits of MN, this section considers the costs of the mandate. Section 5.1 exploits variation in notification times across firms to provide an estimate of the average productivity loss of notice – α in the framework of Section 1. In Section 5.2 we estimate the production loss associated with the delay of separation due to MN.[51] The analysis of the costs of MN requires stronger assumptions than our previous RD analysis, since we generally cannot rely on the age-threshold for identification.

5.1 MN Effect on Productivity

The goal of this section is to estimate the production loss due to notification, which corresponds to α as defined in Section 1.2. The productivity of notified workers declines as their lack of concern about their current career and performance leads them to work less diligently and search for alternative employment. We start by considering firms that give notice to some employees and we leverage variation across firms in how long notified workers stay at the firm.[52] A summary of the analysis is provided here, while additional details of the analysis and the construction of the firm sample can be found in Appendix Section D.

Following Section 1, a worker's productivity falls by a factor α once notified. The production at firm i at time t is therefore $Y_{it} = A_{it}(1-\alpha\chi_{it})L_{it}$, where χ_{it} is the share of labor (months worked) provided by notified workers, A denotes total factor productivity (TFP) and L is the number of employees. The change over time in the log of average worker productivity at the firm is $\Delta \ln y_i = \Delta \ln A_i + \ln(1-\alpha\chi_{it})$, where $y_i = \frac{Y_i}{L_i}$.

The share of labor provided by notified workers is $\chi_i = s_i \times m_i$, where s_i denotes the share of notified employees and m_i the average months worked among workers on notice relative to the overall average. As such, both s_i and m_i are likely endogenous to y_i, s_i is directly related to the size of the TFP shock and m_i partly reflects the choices of workers and firms.

[51] Appendix Section B.4, provides evidence that cash-constrained firms do not pay severance. This suggests that some provide notice even though it is inefficient to do so.

[52] This period is distinct from both MN and AN as it corresponds to the time a notified worker stays with the notifying firm. It is causally affected by MN as shown in Section 3.1.

As a rudimentary starting point, we use an OLS approach to estimating α. It addresses the issue of endogeneity of s_i and m_i by controlling for *inter alia* fixed effects for the percentiles of the s and m distributions, respectively, and identifies α from their interaction. This method compares firms hit by equally-sized shocks (s), and ask whether the impact of this shock depends on how many months notified workers stay at the firm (m). The resulting estimate of α may be biased downwards as firms with a larger productivity drop due to notice – high α – shorten the AN period by paying severance to arrive at a lower m.

An instrumental variables approach is used to deal with this endogeneity problem. We construct an instrument for m based on two institutional features of our setting. First, we exploit variation in MN that is due to age and tenure (see Section 2.1). Second, we employ the LIFO rules that determine how redundancies should be made (See Section 2.1 for details). The instrument – denoted $\widetilde{m_i}$ – takes the number of notified workers in each occupation as given, and calculates the MN period for workers who would have been notified according to the LIFO-rules. This instrument addresses endogeneity concerns regarding selection into displacement and that the actual time spent at the notifying firm may be driven by unobserved heterogeneity.

The IV-approach compares firms that are exposed to equally-sized shocks and quantifies the productivity drops due to longer notice induced by MN. Concretely, we implement the following two-stage least squares approach

$$\chi_i = \gamma_1 \left[s_i \times \widetilde{m_i}\right] + \gamma_2 \left[s_i \times (\widetilde{m_i})^2\right] + \delta_t + \delta_{j(i)} + f_\chi(s_i) + g_\chi(\widetilde{m_i}) + hX_i + \epsilon_i \tag{9}$$

$$\Delta \ln y_i = \beta\chi_i + d_t + \delta_j + f_y(s_i) + g_y(\widetilde{m_i}) + \eta X_i + \varepsilon_i \tag{10}$$

where i denotes a notification event and $\widetilde{m_i}$ the instrument, δ_t and $\delta_{j(i)}$ are notification time and industry fixed effects, respectively. X_i denotes mean age and tenure of workers at the firm, which we also interact with s.[53] Equation (9) is the first stage, and equation (10) the structural equation in our instrumental variables setting. The IV-estimate of β is identified by the interaction terms $s_i \times \widetilde{m_i}$ and $s_i \times \widetilde{m_i}^2$. The second-order interaction term improves the fit of the first-stage regression considerably.[54]

Throughout the IV-analysis, we hold s constant since $\widetilde{m_i}$ increases with s because of the LIFO rule that prioritizes low-tenure workers (low-notice workers) over high-tenure workers in a layoff event. We also control flexibly for $\widetilde{m_i}$. One reason for this is that the instrument depends on the composition of the workforce, which in turn may depend on changes in firm productivity. In firms where productivity has been sluggish, hiring may have been lower, for example, implying that the composition of the workforce is skewed towards high-tenure and old-age workers (and thus higher $\widetilde{m_i}$). In addition, the composition of the workforce may of course have a direct impact on productivity, beyond the impact of average age and tenure that we control for in the regression.[55]

[53] The interaction terms alleviate the worry that the impact of the shock on productivity varies with average age and tenure for reasons not related to advance notice.

[54] As shown by Table 7, the value of the first-stage F-statistic is 222 compared to the first-stage F of 79.4, obtained with a linear interaction term. We illustrate the first-stage relationship in Appendix Figure A25.

[55] Another reason for controlling for $\widetilde{m_i}$ is that firms may postpone layoffs in response to MN. When the firm keeps the

The identification assumption is that a layoff shock of given size would have affected firm productivity in the same way absent the MN rules. Alternatively, productivity in firms with different exposure to MN would have evolved in the same way absent the layoff shock. In addition to the relevance condition, our IV-analysis thus requires that

$$E(\varepsilon_{ij} \,|\, s_i \times \widetilde{m_i}, s_i, \widetilde{m_i}, \Gamma) = E(\varepsilon_{ij} \,|\, s_i, \widetilde{m_i}, \Gamma)$$

where Γ denotes the vector of other variables held constant in equation (10). We validate this assumption by relating productivity prior to the layoff event to the share of work conducted by individuals on notice. These validation exercises do not refute our specifications (see the rows pertaining to outcomes $t-1$ and $t-2$ in Table 7).

The first rows of Table 7 presents our main results. The unit of observation is a layoff-event, defined as a calendar year when the firm notifies some employees of a layoff. To avoid notification times extending across calendar years, we restrict attention to layoff events that are preceded by at least two years without notifications.[56]

The dependent variable in Columns (1)-(2) is the change in log annual revenue per worker between the event year and one year prior to the event. The dependent variable in Columns (3)-(4) is the log of annual revenue per worker during the event year relative to average log revenue per worker during the preceding three years. The estimates of the productivity loss, α, are calculated as $\hat{\alpha} = (1 - \exp(\hat{\beta}\bar{\chi}))/\bar{\chi}$. The OLS-estimates in Columns (1) and (3) deliver estimates of around 0.28 whereas the IV estimates in Columns (2) and (4) – suggest that $\hat{\alpha} \simeq 0.46$. The average of the estimates in Table 7 is 0.37, and thus suggests that a worker's productivity falls by around one third during the notification period.

5.2 The Production Loss due to Delay in Separations

Section 1 points out that the efficiency loss of MN comes from delay – that is, some matches are destroyed later than would be optimal from a production efficiency point of view. This section addresses two questions. First, to what extent does an extension of MN cause a delay in separation? Second, what is the magnitude of the production loss of such delay?

Before turning to these two questions, we briefly discuss the determinants of delay. As emphasized in Section 1, a firm that notifies the worker when the match is still viable incurs the production cost, αy. The firm trades off this cost against the cost of delaying separation. In partial equilibrium, the cost equals: $w - (1-z)(1-\alpha)y$. Thus, the likelihood of delay increases with α. Appendix Section 5.2 extends this result to a continuous time setting. In general, MN affects AN by advancing the timing of notification and postponing the timing of layoff. Indeed, under some circumstances, the duration of delay is proportional to α.[57] This result is derived in a setting

worker longer than is optimal – because of its inability to predict the productivity decline or because MN creates incentives to delay layoffs (see Section 5.2) – a larger portion of the work spell has lower productivity. This, however, is not due to the behavior of workers.

[56]A firm may appear multiple times in case it has multiple events. We cluster standard errors on firms to allow for correlation across events within firm.

[57]This is the result when productivity declines linearly over time. In general, an increase in α makes the timing of layoff

where the firm can time individual layoffs to MN. In practice, however, the layoffs in our setting are collective, typically involving workers with differential eligibility for MN. If the firm wants to lay off some of its workers at the same point in time, it would have to notify workers eligible for long MN earlier than other workers. However, because LIFO prescribes that workers are laid off in inverse order of seniority and because seniority and MN are highly correlated, it will be hard for the firm to notify long-MN worker before those with short MN.[58] Empirically, we find a negligible adjustment of the timing of notification in response to extended MN (see Table A1). This suggests that we can approximate the duration of delay using the increase in the duration at the notifying firm (1.32 according to Table 2).

Next, we examine the cost of delay. Intuitively, this cost approximately equals the expected amount of earnings the worker would have had in an alternative job, had the layoff not been delayed. To measure this counterfactual earnings stream for marginal employees – those most likely to get delayed – we conduct the following empirical exercise. We focus on establishments involved in two consecutive layoff events that are 3-6 months apart.[59] We then sort workers by seniority within layoff event.[60] When workers are laid off in inverse order of seniority according to the LIFO-rule, the least senior worker in the second event could have been laid off as the most senior worker in the first event had the first event been marginally larger. To measure counterfactual earnings, we thus cumulate earnings from the initial layoff report (of the first event) for all workers involved in these two events. We then relate cumulative earnings to annual earnings prior to the layoff to get a convenient metric.[61] This measure is upward biased relative to the production loss associated with delay to the extent that "residual productivity" in the notifying firm ($(1-z)(1-\alpha)y$) is greater than the value of home production (b).

Appendix Figure A18 shows that, relative to prior earnings, the least senior worker in the second layoff lost 1.21 months of earnings in comparison to the marginal worker in the first layoff. Taking into consideration that the two layoff events are on average 4.14 months apart, the earnings loss corresponds to 0.29 of monthly earnings per month of delay. We thus conclude that the cost of the delay induced by the MN extension is equivalent to 0.38 ($1.32 \times 0.29 = 0.38$) months of earnings.

6 Production Efficiency

The aim of this section is to investigate the impact of MN on production efficiency. We calculate the net effect of MN on total production by inserting estimates from Sections 3 and 5 into equation (5). We assume that workers are paid their marginal product and calculate the production gain based on the estimated earnings effect. We deduct the severance effect of MN – 27.4% of the total earnings effect –

more responsive to MN than the timing of notification. We also show that there is no delay in the extreme case where $\alpha = 0$.

[58] For workers with the same tenure, LIFO protects those that are older.

[59] According to legislation, workers that are notified within 3 months of one another belong to the same layoff event (see Section 2). Thus, 3 months is a natural lower limit.

[60] In this analysis, we focus on all workers involved in the layoff. In contrast to our RD approach, the analysis thus uses blue-collar as well as white-collar workers, and there is no age restriction.

[61] Notice that the amount of prior earnings is balanced across the threshold.

since severance is a transfer from the firm to the worker.[62] The estimates from equation (8) then yield total production gains of crossing the age-55 threshold of 1.16 (standard error 0.26) of the average monthly wage per notified worker.[63] The production losses due to MN have two components. We first combine the estimated production loss factor, α, from Section 5.1 (we use $\alpha = 0.37$, i.e., the average of the estimates in Table 7) with the additional number of months a notified worker stays in the notifying firm (1.32 according to Table 2) to obtain a production loss due to the productivity drop among notified workers that equals 0.49 (standard error 0.21). We thus find a net production gain of notice that corresponds to 0.67 of a monthly wage.

The second component of the production losses come from delay. According to Section 5.2, the production loss due to delay is 0.38 of a monthly wage (standard error 0.10). When we pull the different pieces together (equation 11), we conclude that extending MN yields a net production gain of 0.29 of a monthly wage. While positive, this estimate is not statistically significant – the standard error equals 0.35.

$$\underbrace{\underbrace{1.60 \times (1 - 0.274)}_{\text{Earnings gain}} - \underbrace{0.37}_{\text{Productivity drop}} \times \underbrace{1.32}_{\text{Addtional tenure}}}_{\text{Net Production Gain of Notice}=0.67} - \underbrace{\underbrace{0.29}_{\text{Delay Cost}} \times \underbrace{1.32}_{\text{Delay}}}_{\text{Production Loss of Delay}=0.38} \simeq \underbrace{0.29 \,\text{Monthly wage}}_{\text{Net Production Effect}} \tag{11}$$

We interpret this result as suggestive evidence that the MN extension improved production efficiency in our setting. Two main concerns may be raised regarding the external validity of this result. First, it is key in our context that it is possible to replace AN with severance. Since we observe a significant impact of MN on severance, the mandate was not efficient in some cases. Second, the MN extension applies to a sub-set of the population – high-age, long-tenured workers – whose gains from advance notice may be particularly large.

We now turn to the fiscal implications of the MN extension, which we have ignored thus far. To estimate the fiscal externality of MN, we first note that the average sum of the tax rate on labor income and the payroll tax amounts to around 60%, $\tau \simeq .6$. Coupled with the above estimate of the net production gain, MN thus raises tax revenue by 17% of a monthly wage. In addition, MN reduces the duration of unemployment, creating another positive fiscal externality. With an average replacement rate of 50%, $\rho \simeq .5$ and our estimate of the unemployment duration effect of MN from Table 2 (-0.472), we find a fiscal externality corresponding to 0.24 of the average monthly wage.[64] Putting these two externalities together, the total fiscal externality, resulting from the extension of MN at age 55, corresponds to 0.41 of a monthly wage, with a standard error of 0.24.

[62] Our production calculation only accounts for the direct effect of MN. We thus ignore equilibrium effects of MN– see Hagedorn et al. (2013), Lalive et al. (2015), Marinescu (2017), and Landais et al. (2018) for evidence in the case of UI – and spillovers. As shown in Section 3.2, spillovers are relevant since other workers involved in the same layoff receive higher severance when they are laid-off with workers eligible for longer MN.

[63] We use the delta method to calculate all standard errors in this section.

[64] A replacement rate of 50% corresponds well with reality, since the earnings cap in the public UI system was low during the time period considered and the population we consider is a high-income group.

$$\underbrace{\tau \times \underbrace{0.29}_{\text{Production gain}}}_{\uparrow \text{Taxes } 0.17} \underbrace{- \rho \times \underbrace{-0.47}_{\text{Unemp. dur.}}}_{\downarrow \text{UI benefit } 0.24} \simeq \underbrace{0.41 \,\text{Monthly wage}}_{\text{fisc. externality}} \tag{12}$$

While suggestive, these calculations should be taken with a due grain of salt. In addition to the external validity concerns we raised above, we should also note that we have ignored the potential effect that MN might have on hiring. Hiring will be adversely impacted if firms cannot shift the average cost of the policy onto workers in the form of a reduction in wages.

7 Concluding Remarks

Mandatory Notice (MN) policies require employers to inform employees in advance of layoff. Despite the widespread prevalence of these policies, the literature on MN is limited. Our paper analyzes the positive and normative aspects of the requirement to inform workers in advance of layoff.

One main lesson of our analysis is that some MN is generally optimal. By inducing firms to provide advance notice (AN), workers are encouraged to search while still employed, helping them to find higher-quality jobs upon reemployment. There is more uncertainty regarding the length of MN, however. A concern – that further research should engage with – is that MN puts financial pressure on firms in instances when they may be in trouble. Another concern is general equilibrium effects of MN on hiring. Such consequences are relevant if firms cannot reduce wages to shift the cost of the policy onto workers. While we have offered a framework that sheds light on the tradeoff involved in determining the optimal duration of the mandate, we have left other questions regarding the detailed design of the policy for future work.

We have provided novel empirical analyses on several accounts. We believe that two empirical findings are particularly fundamental. First, we have shown that severance is used to sidestep the law. Lazear (1990) points out that private contracts undo policies, limiting their impact on the real economy. But private contracts would only undo policies when they are inefficient. Thus studying why and how labor laws are circumvented using private contracts teaches us about the efficiency of the public mandate in question. When the law is not undone using private contracts, this may indeed signal that it improves allocations and efficiency.

Second, we find that AN replaces job search while unemployed with more effective search while employed. Despite reduced search activity and higher re-employment wages, extending AN does not delay job-finding. Employed job search offers a higher return to search – in the sense of a higher job-finding rate – and workers can target higher quality jobs without a decline in job-finding rates. We have offered new evidence on the return to search activity across employment states, and find that it is indeed higher during employment. However, we cannot gauge the extent that targeting a better job is easier for an employed worker. Hopefully, further research will shed light on this key issue.

At several stages of our analysis we have pointed to the similarities and differences of MN compared with regular Unemployment Insurance (UI). An additional point is that MN has the advantage of forcing firms to internalize part of the layoff cost relative to a non-experience-rated UI system. We think further analysis of the advantages or disadvantages of MN and UI, and the potential interactions between these two systems, is an interesting avenue for further research.

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Table 1: Descriptive Statistics

	All Notified	Notified aged 52-58	Employed workers	Employed workers in same industry
	(1)	(2)	(3)	(4)
		Panel (a): Individual-level characteristics		
Female	0.35	0.44	0.50	0.35
Immigrant	0.17	0.11	0.14	0.14
Age (years)	40.99	55.00	41.23	41.18
Tenure (years)	5.71	7.93	7.45	6.98
Earnings_{t-1} (1,000 SEK)	260.29	377.57	242.30	265.43
Wage_{t-1} (1,000 SEK)	24.77	31.98	23.47	24.57
Educational attainment				
Compulsory	0.16	0.11	0.15	0.17
Upper-secondary	0.60	0.49	0.47	0.52
College	0.24	0.38	0.37	0.30
		Panel (b): Firm-level characteristics		
Firm size (number of employed)	593.01	1,056.66	76.59	60.72
Industry shares:				
Manufacturing	0.36	0.34	0.13	0.36
Construction	0.08	0.03	0.06	0.08
Wholesale and retail	0.11	0.10	0.12	0.11
Transport	0.12	0.24	0.08	0.12
Financial Services	0.01	0.01	0.02	0.01
Non-Financial services	0.15	0.15	0.13	0.15
Public administration	0.02	0.01	0.06	0.02
Education	0.02	0.02	0.11	0.02
Health care	0.04	0.04	0.18	0.04
Entertainment	0.02	0.02	0.05	0.02
Other	0.08	0.02	0.06	0.08
Number of observations	438,413	10,275	4,940,447	4,940,447

Notes: The table presents summary statistics (means) for different samples over the years 2005-2016. Column (1) considers all notified individuals and Column (2) notified individuals in our main analysis sample – white-collar workers in the private sector aged 52-58 at the time of notification. Column (3) shows characteristics for a stratified random sample of employed workers. We create the sample as follows. First, we compute the share of notified workers in each calendar year from the population in Column (1). We then extract a random sample of workers from the matched employer-employee data using the shares across years from the first step as weights. In Column (4), we further re-weight observations so that the industry distribution of employed workers matches that of notified workers in Column (1) (using the 11 industry-categories in the table). Immigrant is an indicator for being born outside Sweden. Age and tenure are measured at notification in Columns (1) and (2) and at the end of the year for Columns (3) and (4). Earnings in $t-1$ correspond to annual earnings in the previous calendar year. The wage is the full-time equivalent monthly wage, taken from the Wage Survey, and is observed in September-October each year for all public-sector workers and roughly 50% of all private-sector employees. Firm-level characteristics are computed at the individual level, except firm size where the unit of observation is a firm.

Table 2: Effect of MN on Employment Status Within Two Years

	Cumulated duration (months) within two years after notification				
	Notifying firm	New firm	Non-employment	Unemployment	Out of the labor force
	(1)	(2)	(3)	(4)	(5)
Above Age-55	1.322***	-0.145	-1.177***	-0.472*	-0.705***
	(0.276)	(0.333)	(0.288)	(0.246)	(0.214)
Control mean	7.859***	9.372***	6.769***	4.668***	2.100***
	(0.217)	(0.253)	(0.212)	(0.178)	(0.147)
Number of clusters	4,158	4,158	4,158	4,158	4,158
Number of observations	10,275	10,275	10,275	10,275	10,275

Notes: The table shows estimates of equation (6) with the outcomes being the number of months spent in various labor market states during the first two years after notification. The outcomes in Columns (1)-(3) are mutually exclusive. Notifying firm in Column 1 is the number of months worked in the notifying firm while not being in a new job. New firm in Column 2 is defined as the number of months in a new employer-employee spell that i) pays more than 10 kSEK per month; and ii) the worker has not derived any income from during the 12 months preceding notification. Non-employment in Column 3 refers to the number of months in the residual category. Columns (4) and (5) decompose the non-employment outcome in Column (3) into months registered as unemployed with the PES and months out of the labor force. Regressions include individuals aged 52-58 at the time of notification. The regressions control for a linear age polynomial interacted with the threshold indicator, individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs), and month-by-year FE:s. Appendix Figure A12 illustrates the RD estimates graphically. Standard errors are clustered at the level of the notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of MN on Wages and EE-transitions

	Re-employment wages				Pr(EE)
	$\ln(w)$	$\ln(w)$	$\Delta\ln(w)$	$\Delta\ln(w)$ $EE = 1, t \leqslant 6$	
	(1)	(2)	(3)	(4)	(5)
Above Age-55	0.029**	0.034**	0.032**	0.045*	0.075**
	(0.014)	(0.016)	(0.016)	(0.027)	(0.037)
Control mean	10.201***	10.200***	-0.093***	-0.077***	0.566***
	(0.010)	(0.011)	(0.011)	(0.019)	(0.027)
Number of clusters	2,229	1,713	1,353	561	1,713
Number of observations	3,932	2,752	2,276	749	2,752

Notes: The table shows regression estimates of equation (6). In Column (1) the wage in the new job is defined as the log of the first monthly wage that we observe within two years after notification. The sample contains all individuals who have found a new job according to the wage survey. Column (2) restricts the sample by excluding observations where the worker has located another new job prior to the one recorded in the Wage Survey according to the monthly workplace indicators available in the matched employer-employee data. In Column (3) we construct the outcome as the difference between log wage in Column (2) and the log wage from the notifying firm. In Column (4), we condition on the worker finding the new job directly from employment (EE-transition) within 6 months after notification. In Column (5), the outcome is the indicator of making an EE-transition within two years after notification. We include a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification as controls. The regressions also include individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs) and month-by-year FE:s. Standard errors are clustered by notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: 2-IV Estimates: Separating the Effects of Advance Notice and Severance

Panel (a):	**First-stage estimates**		**Reduced-form (RF) estimates**			
	Notification time (months)	Severance (1,000 SEK)	Search intensity	Months until new job	Non-employment (months)	$\Delta\ln(w)$
	(1)	(2)	(3)	(4)	(5)	(6)
Above age-55	2.593***	18.458**	-0.222***	0.112	-1.176***	0.035**
	(0.193)	(7.307)	(0.066)	(0.319)	(0.283)	(0.016)
Share coworkers above 55	0.776	30.428***	-0.064	1.500***	1.813***	-0.002
	(0.678)	(11.197)	(0.073)	(0.378)	(0.560)	(0.014)
Panel (b):			**2-IV estimates**			
Notification time			-0.087**	-0.205	-0.621***	0.017**
(months)			(0.038)	(0.241)	(0.161)	(0.008)
Severance			-0.001	0.035***	0.051***	-0.0001
(1,000 SEK)			(0.002)	(0.013)	(0.015)	(0.001)
Joint F-statistic	90	8	21	26	29	5
Number of clusters	4,285	4,212	4,011	4,060	4,285	2,564
Number of observations (RF)	55,987	49,340	35,515	36,689	56,531	12,590

Notes: The table shows results from our 2-IV strategy, outlined in Section 3.2. Panel (a) shows first-stage and reduced-form coefficients, while panel (b) shows instrumental variables estimates. Search intensity in Column (3) is defined as the inverse hyperbolic sine function (arcsinh) of the number of interactions with a PES caseworker up until the worker has found a new job. The dependent variables in Column (4) is the duration of the period between notification and the next job, Column (5) is the duration of the period between end of notifying job and the next job (zero if EE), and Column (6) is the difference in the log wage between the first new job and the notifying job (c.f. Column (2) in Table 3). The regression specifications are such that the effect of the above-55 indicator is identified within the sample of white-collar workers aged 52-58, while share coworkers above 55 is identified across all white-collar workers. All regressions include (i) a linear age-polynomial interacted with age-brackets FEs (the age brackets are 6 years wide, consistent with the 52-58 bracket), individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs), month-by-year FE:s. Individual covariates are interacted with a dummy for being close to the threshold (age 52-58). (ii) Firm covariates: average age of workers, average age squared, average earnings of workers, share female workers, share college educated and firm size. (iii) Layoff characteristics: size of layoff and flexible controls for average tenure within layoff. (iv) 2-digit industry FEs. Standard errors are clustered by notification event. The sample comprises all white-collar workers in notification events where a white-collar worker aged 52-58 was notified. The sample drops in Column (3), (4), and (6) as we condition on workers having found a new job within two years after notification (Section 3.1.3); the additional drop in Column (6) is due to sampling in the Wage Survey (Section 2.2). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Job Search and the Hazard Rate for Employed and Unemployed

	Extensive search		Search intensity	
	(1)	(2)	(3)	(4)
(a) Search	0.201***	0.149***	0.113***	0.084***
	(0.002)	(0.004)	(0.001)	(0.002)
(b) Search $\times$Employed	0.031***	0.019***	0.046***	0.021***
	(0.003)	(0.005)	(0.002)	(0.003)
Employed	-0.014		-0.022*	
	(0.013)		(0.013)	
Intercept	0.075***		0.083***	
	(0.011)		(0.011)	
Relative return to search:	1.157***	1.131***	1.408***	1.252***
[(b)+(a)] /(a)	(0.013)	(0.035)	(0.015)	(0.04)
Individual FE		√		√
R^2	0.139	0.518	0.137	0.518
Number of observations	2,017,164	374,585	2,017,164	374,585

Notes: The table uses the Labor Force Survey (LFS) and reports the results of OLS regressions where the outcome is a binary indicator of finding a new job in the next quarter. The search measure in Columns (1)-(2) is an indicator for those self-reporting the use of any of eight search channels (visiting the Public Employment Service (PES), using a PES job coach, searching jobs databases, searching via recruitment firms, searching by directly approaching firms, applying to posted ads, reading ads, asking friends for job tips). The mean is 0.200 for the unemployed vs. 0.070 for the employed. The search measure in Columns (3)-(4) is the inverse hyperbolic sine of the number of search channels used. All regressions control for gender-by-education FEs interacted with a third-order age polynomial as well as calendar month and year fixed effects. All controls, including individual FEs, are interacted with employment status. Significance asterisks for the relative search effectiveness estimates refer to the null of the ratio being equal to one. Standard errors are clustered at individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: The Return to Search for Employed and Unemployed

	Employed (De jure notice) (1)	Unemployed (RKD) (2)	Relative return to search (3)
Panel (a): Reduced-form			
Hazard	-0.005***	0.040***	
	(0.0002)	(0.002)	
ln(wage)	-0.003	-0.002	
	(0.003)	(0.013)	
Search			
Extensive search	-0.022*	0.222***	
	(0.013)	(0.083)	
Search intensity	-0.054***	0.558***	
	(0.021)	(0.164)	
Panel (b): IV			
Effect of search on the hazard			
Extensive search	0.207***	0.161***	1.285***
	(0.012)	(0.005)	(0.086)
Intensive search	0.083***	0.070***	1.190**
	(0.005)	(0.002)	(0.076)
Number of observations	840,124	9,988,274	

Notes: Each coefficient derives from separate regressions where the outcomes are reported in each row. Panel (a) shows the reduced-form effects exploiting exogenous variation in search for employed and unemployed, respectively. The independent variable in Column (1) is de jure notice time (in months) while Column (2) exploits the cap in the UI-benefit schedule in an RKD design. Panel (b) show the effect of search on the hazard to a new job in the next month using a two-sample IV-strategy. For Column (1), the sample includes notified workers between 2005-2016 with 2-6 months de jure notice time while Column (2) includes all unemployed workers eligible for UI between 2005-2015. The outcomes in Column (1) are measured between the initial layoff report and two months after individual notice, i.e., in close vicinity of individual notification (on average a 4 month period). Regressions in Column (1) control for individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, age, tenure, educational attainment FEs), and month-by-year FE:s. Column (3) shows the ratio of the estimates in panel (b) where standard errors are calculated using the delta method. Standard errors in Columns (1) and (2) are clustered at the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

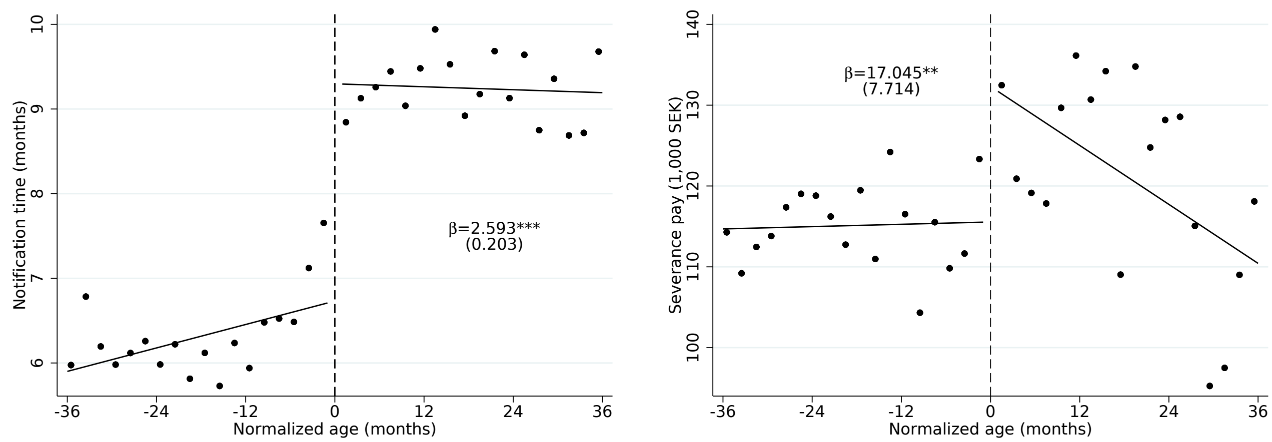
Table 7: The Productivity Loss of Notice

	Dependent variable			
	$\Delta \ln y$		$\ln y - \sum_{t=-3}^{-1} \ln y_t/3$	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Share of workers on notice (χ)	-0.275**	-0.469***	-0.290**	-0.465***
	(0.111)	(0.161)	(0.118)	(0.162)
Estimate of productivity loss (α)	0.272**	0.461**	0.287**	0.458***
	(0.110)	(0158)	(0.116)	(0.160)
First stage				
First-stage F		221.7		221.7
Specification check (outcomes in $t-1$)				
Share of workers on notice (χ)	0.078	0.062	0.021	0.003
	(0.088)	(0.121)	(0.060)	(0.081)
Specification check (outcomes in $t-2$)				
Share of workers on notice (χ)	-0.033	-0.169	-0.055	-0.048
	(0.100)	(0.135)	(0.048)	(0.065)
Number of observations	3,218	3,218	3,218	3,218

Notes: The table shows estimates of productivity losses from having workers on notice, i.e. α of Section 5.1. For the dependent variables we use $y =$ revenue per worker. Columns (1) and (3) present OLS estimates, while Columns (2) and (4) present instrumental variables estimates. The first stages regress χ on $s \times \widetilde{m_i}$ and $s \times (\widetilde{m_i})^2$, where s_i denotes the share of notified employees and $\widetilde{m_i}$ denotes the de-jure instrument. We include mean age and mean tenure at the firm as controls, as well as the interactions between these two variables and s. We also include fixed effects for each percentile rank of the s-distribution (share notified) as well as s entered linearly. Along the same lines, we include fixed effects for each percentile rank of the $m(\widetilde{m_i})$-distribution as well as m $(\widetilde{m_i})$ entered linearly. Finally, all regressions include calendar year and month fixed effects. Appendix Section D describes the firm analysis in greater detail. Mean and variance of χ are 0.069 and 0.103. Standard errors are clustered on firms. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: The Effect of Mandated Notice (MN) on Actual Notification Times and Severance Pay

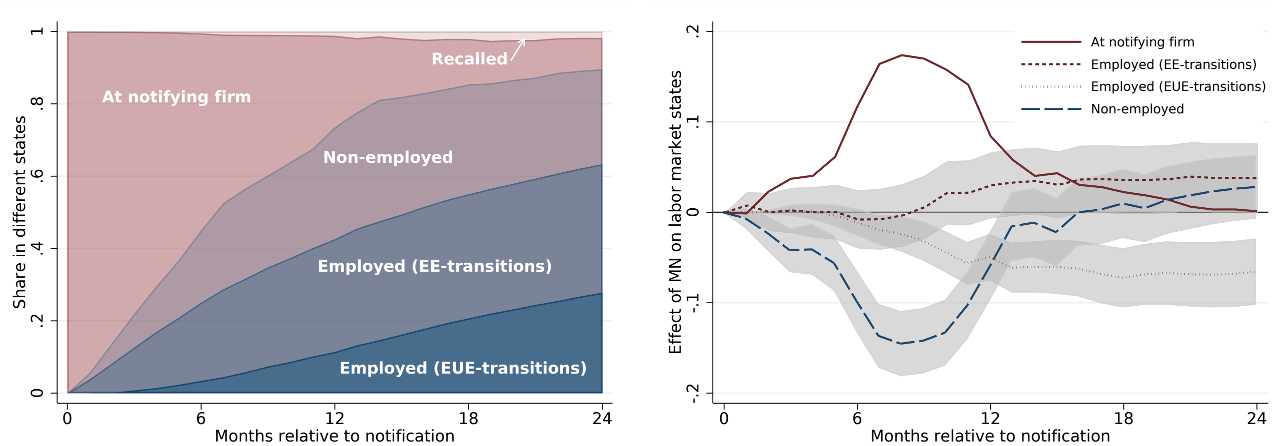
(a) Notification Times (b) Severance



Notes: The figure plots (a) the actual advance notification (AN) period, (b) severance on the y-axis against age at notification relative to the 55-threshold in 2 month bins on the x-axis. Severance pay is measured as excess earnings in the year of displacement (see Section 3.1). The regression lines come from estimating equation (6) with a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification. The regressions also include individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure at notification, educational attainment FEs), and month-by-year FE:s. The estimated discontinuity at the threshold and its standard error are reported. Standard errors are clustered at notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 2: Dynamic Employment Transitions

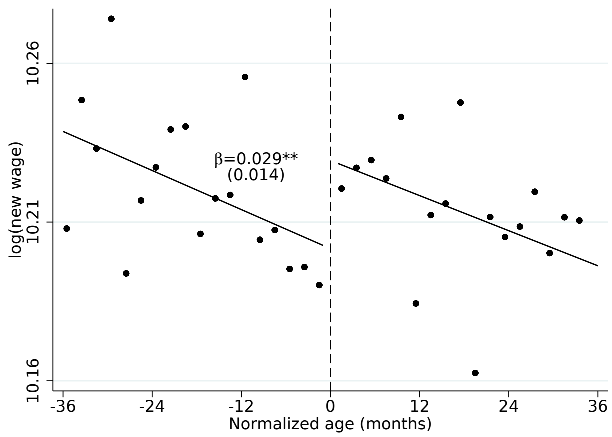
(a) Employment Transitions Over Time (b) Effect of MN on Employment Transitions



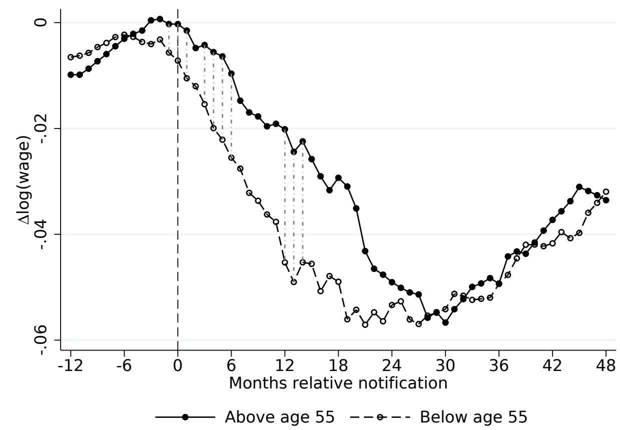
Notes: The figure presents employment outcomes over time relative to notification for individuals aged 52-58 at the time of notification. Panel (a) shows the employment dynamics after notification where we treat new employment as an absorbing state. Notified workers who are employed are split into those who made an employment-to-employment transition (EE) and those who had a period of non-employment between the spells (EUE). An individual who experiences a period of non-employment and returns to the notifying firm is coded as a recall. Panel (b) plots RD-estimates and 95% confidence intervals (in gray) corresponding to equation (6) at each point in time after notification. Standard errors are clustered by notification event. Regressions include a linear age control function where the slope is allowed to differ above and below the threshold, baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs), and month-by-year FE:s.

Figure 3: Effect of MN on Wages

(a) Effect of MN on the Re-employment Wage

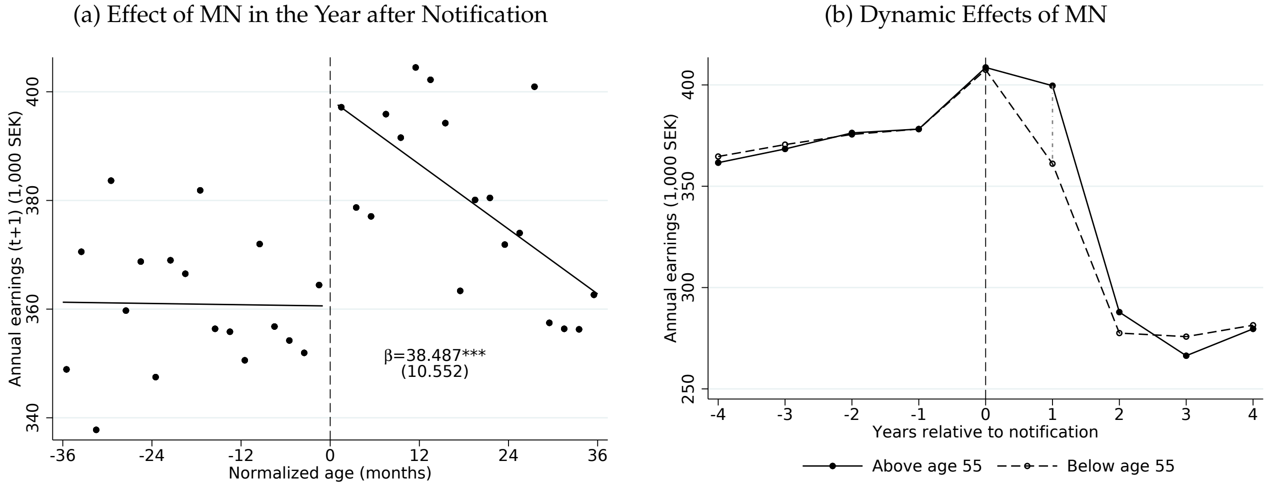


(b) Dynamic Effect of MN on Wages



Notes: Panel (a) shows the log of the wage from the new job after notification by age at notification relative to the 55-threshold. The estimated jump at the threshold and its standard error are also displayed. The wage refers to full-time equivalent monthly wage reported by the employer. The wage from the new job is the first wage that we observe in the Wage Survey within two years after notification. Panel (b) shows the RD-estimates of the difference between the log of the wage in a new job and the wage from the notifying firm observed prior to notification. The hollow circles represent the constant just below the threshold while the filled circles represent the constant plus the discontinuity at age 55. Dashed lines indicate when the discontinuity is significant at the 5% level. In contrast to panel (a), where we only consider the wage in the new job, we include all wage observations after notification in panel (b), including those from the notifying firm and wages obtained from job mobility. The estimates are based on equation (6) controlling for age linearly interacted with the threshold indicator for individuals aged 52-58 at the time of notification. The regressions also include individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs) and month-by-year FE:s. Standard errors are clustered by notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 4: Effect of MN on Annual Earnings

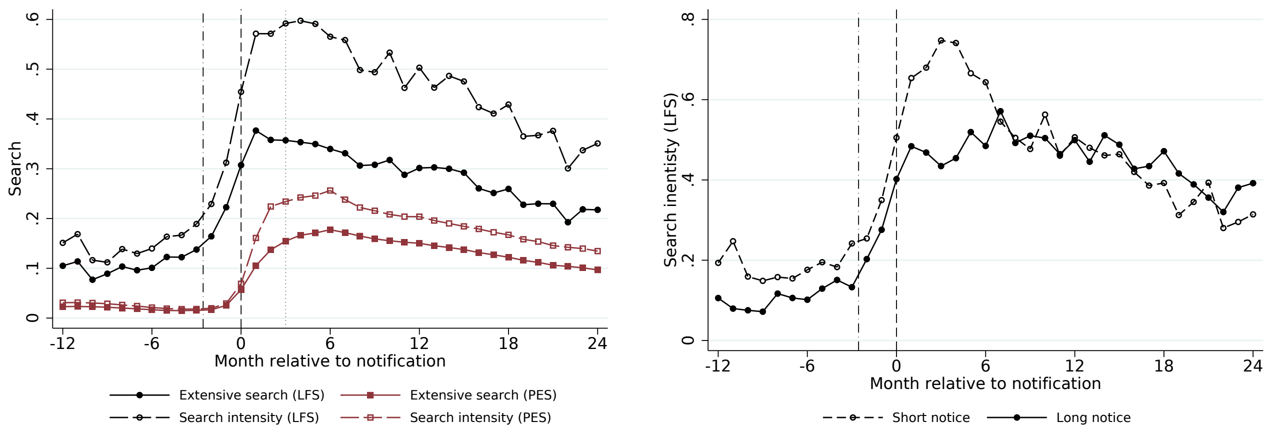


Notes: Panel (a) plots annual earnings in 1,000 SEK in the calendar year after notification against age at notification relative to the 55-threshold. The estimated jump at the threshold and its standard error are displayed in the figure. In panel (b) we plot the RD-estimates of earnings effects over time. The hollow circles represent the constant just below the threshold while the filled circles represent the constant plus the discontinuity at age 55. A dashed vertical line indicate when the discontinuity is significant at the 5% level. The regressions in both panels are based on equation (6) including a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification. The regressions also include individual-level baseline covariates (gender, immigrant status, tenure, educational attainment FEs) and month-by-year FE:s. Standard errors are clustered by notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 5: Search by Month Relative to Notification

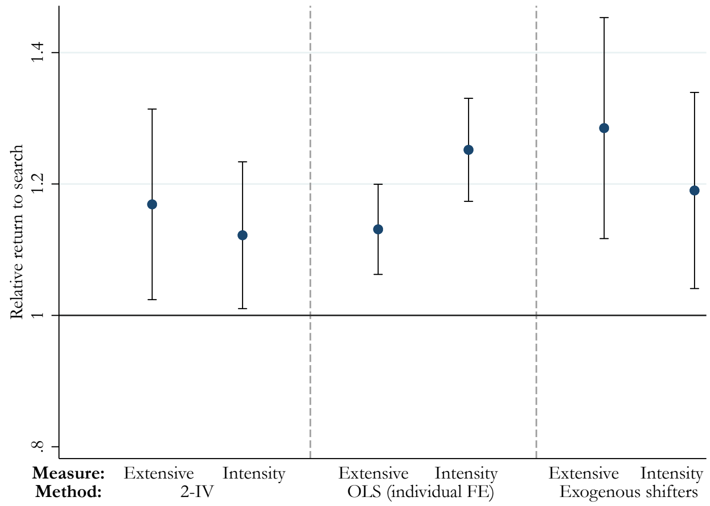
(a) Search in the LFS and PES

(b) Search intensity by notification time



Notes: The figure shows the evolution of job search around notification during 2005-2016. In panel (a) we present four measures of search. The solid and hollow black series correspond to self-reported search in the Labor Force Survey (LFS). The former shows the probability of search (extensive margin) whereas the latter show the intensity of search, defined as the inverse hyperbolic sine function (arcsinh) of the number of channels used to look for a job. The channels are: visiting the Public Employment Service (PES), using a PES job coach, searching jobs databases, searching via recruitment firms, searching by directly approaching firms, applying to posted ads, reading ads, asking friends for job tips. The red solid and hollow squared series use administrative data from PES. The former shows the probability of being registered at the PES (extensive search) and the latter search intensity defined as the inverse hyperbolic sine function (arcsinh) of the number of interactions with a PES caseworker within a month. The vertical dotted line at $t+3$ indicates average de jure notice time while the dash-dotted line at t-2 indicates when on average the firm reports the notice to the PES, relative to a workers individual notice at $t = 0$, respectively. Panel (b) shows search intensity as measured by the LFS for notified workers with short (dashed line) and long (solid line) AN, defined as below and above the median (90 days), respectively. Average AN among short and long notice workers is 1.6 and 7.0 months, respectively. The average time between firm reporting the notice and individual notification is almost the same for short notice workers and for long notice workers, demarcated by the vertical line in the figure at -2.55.

Figure 6: Relative Return to Job Search for Employed Versus Unemployed



Notes: The figure summarizes our estimates of the relative return to job search among employed versus unemployed using three empirical designs and two measures of search activities: the extensive margin and search intensity (arcsinh). The left-most two dots correspond to the estimated return using the 2-IV strategy as in Table 4 Column (3). These are lower-bound estimates of the return to search given the higher target wage. The next two dots show the relative returns obtained using the OLS-strategy presented in Columns (2) and (4) of Table 5. The final two dots use the exogenous shifters as in Table 6 Column (3). Each dot is surrounded by 95% confidence intervals.

A Online Appendix: Theoretical Part

This section provides a more detailed description of the model in Section 1. It also contains some analysis that we omitted from the main text for the purpose of brevity. Throughout, we consider a case where it is efficient for the firm to layoff the worker: $U^u > (1-z)y$ (the notation is the same as in the main text). For the sake of simplicity, the model assumes no discounting over time.

A.1 MN effect on target wages & job finding rates

In general, let $\lambda^j = \phi^j(w)$ denote the job-finding rate for an unemployed ($j = u$) or an employed worker ($j = e$). The job-finding rate is decreasing in the target wage (w). We define the η as the efficiency of employed search relative to unemployed search. With this definition, we parameterize the difference in the job-finding rate across an unemployed and an employed worker as $\phi^u = \phi(w, 0)$ and $\phi^e = \phi(w, \eta)$, respectively.

The optimal choice of the target wage for an unemployed individual is given by

$$U^u = \max_w \phi(w, 0)\, w + (1 - \phi(w, 0))\, b$$

The first order condition is

$$\phi(w^u, 0) + \frac{\partial \phi(w^u, 0)}{\partial w}(w^u - b) = 0 \tag{A1}$$

and the second order condition:

$$-\xi \equiv \frac{\partial \phi}{\partial w} + \frac{\partial^2 \phi}{\partial w^2}(w - b) < 0$$

We evaluate how the choice of the target wage of the unemployed agent responds to exogenous changes. To do so, we look at the comparative statics of the target wage with respect to η and b. We have:

$$\frac{\partial w}{\partial \eta} = \frac{1}{\xi}\left[\frac{\partial \phi}{\partial \eta} + \frac{\partial^2 \phi}{\partial \eta \partial w}(w - b)\right] \tag{A2}$$

and

$$\frac{\partial w}{\partial b} = -\frac{1}{\xi}\frac{\partial \phi}{\partial w} \tag{A3}$$

The optimal choice of the target wage for an employed individual on notice is defined by

$$U^n = \max_w \phi(w, \eta)\, w + (1 - \phi(w, \eta))\, U^u$$

The first order condition is

$$\phi(w^e, \eta) + \frac{\partial \phi(w^e, \eta)}{\partial w}(w^e - U^u) = 0 \tag{A4}$$

The two first order conditions, equations (A1) and (A4), implicitly define a wage function, which we with slight abuse of notation denote by w. Thus, $w^e = w(\eta, U^u)$ and $w^u = w(0, b)$.

Using linear approximation as well as equations (A2) and (A3), the difference between two wages can be written as

$$
\begin{aligned}
w^{e}-w^{u} &\simeq \eta\frac{\partial w}{\partial \eta}+\left(U^{u}-b\right)\frac{\partial w}{\partial b} \\
&=\frac{1}{\xi}\left[\eta\frac{\partial \phi}{\partial \eta}-\left(U^{u}-b\right)\frac{\partial \phi}{\partial w}\right]
\end{aligned}
\tag{A5}
$$

The expected wage for a notified worker is:

$$
w^{n}=\frac{\lambda^{e}w^{e}+\left(\lambda^{n}-\lambda^{e}\right)w^{u}}{\lambda^{n}}. \tag{A6}
$$

Using equations (A5) and (A6), we can write the wage change due to notice as

$$
\begin{aligned}
\Delta w &= w^{n}-w^{u} \\
&=\frac{\lambda^{e}}{\lambda^{n}}\left(w^{e}-w^{u}\right) \\
&\simeq \frac{1}{\xi}\frac{\lambda^{e}}{\lambda^{n}}\left[\eta\frac{\partial \phi}{\partial \eta}-\left(U^{u}-b\right)\frac{\partial \phi}{\partial w}\right]
\end{aligned}
$$

The change in the employment rate in the second period due to notice is

$$
\begin{aligned}
\Delta\lambda &= \lambda^{e}+\left(1-\lambda^{e}\right)\lambda^{u}-\lambda^{u} \\
&=\lambda^{e}\left(1-\lambda^{u}\right)
\end{aligned}
$$

which can be rewritten as a decomposition to two parts: One component is due to an to extended period of search that is $\lambda^{u}\left(1-\lambda^{u}\right)$. The other component is due to the relative effectiveness of employed job search, $\lambda^{e}-\lambda^{u}$. That is

$$
\Delta\lambda=\lambda^{u}\left(1-\lambda^{u}\right)+\left(\lambda^{e}-\lambda^{u}\right)\left(1-\lambda^{u}\right)
$$

and using linear approximation

$$
\Delta\lambda \simeq \underbrace{\lambda^{u}\left(1-\lambda^{u}\right)}_{\text{Extended search}}+\underbrace{\left[\eta\frac{\partial \phi}{\partial \eta}+\left(w^{e}-w^{u}\right)\frac{\partial \phi}{\partial w}\right]\left(1-\lambda^{u}\right)}_{\text{Relative effectiveness}}
$$

We can replace $w^{e}-w^{u}$ with its linear approximation, as in (A5), but this does not add intuition.

A.2 Equilibrium profits, wages, and the choice of layoff policy

Without a mandate, the expected profit over both periods for the employer is

$$
\underbrace{y-w}_{\text{1st period profit}}+\underbrace{\left(1-\theta\right)\left(y-w\right)}_{\text{2nd period profit}}=\left(2-\theta\right)\left(y-w\right)
$$

since with probability θ productivity drops in the second period, which implies that separation occurs.

Under the mandate, the expected profit under each layoff policy – Notice (N), Severance (S), and Delay (D) – can be written as

$$\begin{aligned} \pi^N(w) &= (2-\theta)(y-w) - \theta\alpha y \\ \pi^S(w) &= (2-\theta)(y-w) - \theta\sigma \\ \pi^D(w) &= (2-\theta)(y-w) + \theta[(1-z)y - w] \end{aligned} \tag{A7}$$

For simplicity, we assume that delayed layoff does not require notice, so productivity does not fall due to notice in the second period. All our results are robust to this assumption, but the expressions would be more complex.[65]

Equilibrium wages are determined by zero-profit conditions, $\pi^i(w_0^i) = 0$. They are given by

$$\begin{aligned} w_0^N &= y - \frac{\theta}{2-\theta}\alpha y \\ w_0^S &= y - \frac{\theta}{2-\theta}\sigma \\ w_0^D &= y - \frac{\theta}{2}zy \end{aligned}$$

The utility associated with each layoff policy is:

$$\begin{aligned} U^N(w) &= (2-\theta)w + \theta U^n \\ U^S(w) &= (2-\theta)w + \theta(U^u + \sigma) \\ U^D(w) &= 2w \end{aligned}$$

Evaluated at equilibrium wages, the utilities are given by:

$$\begin{aligned} U^N(w_0^N) &= (2-\theta)y + \theta(U^n - \alpha y) \\ U^S(w_0^S) &= (2-\theta)y + \theta U^u \\ U^D(w_0^D) &= (2-\theta)y + \theta(1-z)y \end{aligned}$$

With very low wages, delay is the only credible layoff policy as the intercepts of the profit functions is always larger for the policy of delaying notice, i.e., $\pi^D(0) > \max(\pi^N(0), \pi^S(0))$. In addition, if $w_0^D = \max(w_0^N, w_0^S, w_0^D)$, then delay is the only incentive compatible policy independently of the wage level. This condition is equivalent to $\left(1-\frac{\theta}{2}\right)zy \leqslant \min(\alpha y, \sigma)$. By defining $\kappa^N = \alpha y$, $\kappa^S = \sigma$, and $\kappa^D = \left(1-\frac{\theta}{2}\right)zy$, we can write this condition as $\min(\kappa^N, \kappa^S, \kappa^D) = \kappa^D$.

Another way of showing this, which is also helpful to characterize other equilibria, is as follows. Delay is optimal for firms if $\pi^D(w) \geqslant \max(\pi^N(w), \pi^S(w))$. The highest wage that is incentive

[65] This simplifying assumption is consistent with the two period set up, since firms cannot be compelled to provide notice at the start of the 2nd period if it is optimal to delay layoff.

compatible with delay is given by:

$$w_1^D = (1-z)\,y + \min(\alpha y, \sigma)$$

Delay is thus incentive compatible in equilibrium if $w_0^D \leqslant w_1^D$. Inserting the expressions for the wages, we see that this condition can again be written as $\left(1-\frac{\theta}{2}\right) zy \leqslant \min(\alpha y, \sigma)$.

Giving notice is incentive compatible in equilibrium if two conditions hold. First, the firm can afford to pay higher wages under notice than under the other two options: $w_0^N = \max\left(w_0^N, w_0^S, w_0^D\right)$. This is equivalent to $\alpha y \leqslant \min\left(\left(1-\frac{\theta}{2}\right) zy, \sigma\right)$ or $\min\left(\kappa^N, \kappa^S, \kappa^D\right) = \kappa^N$. Second, the equilibrium wage with notice should provide higher utility than w_1^D: $U^N\left(w_0^N\right) > U^D\left(w_1^D\right)$. Here we used the fact that the worker prefers a higher wage for given layoff policy and the profit functions of N and S have the same slopes with respect to the wage, see equations (A7). We can also verify that the second condition is implied by the first, given the efficient separation condition $(1-z)\,y < U^u$.

In the same vein, there are two conditions guaranteeing that severance is incentive compatible in equilibrium. First, there exist a wage level where severance is incentive compatible for the employer, that is, $w_0^S = \max\left(w_0^N, w_0^S, w_0^D\right)$. This is equivalent to $\min\left(\kappa^N, \kappa^S, \kappa^D\right) = \kappa^S$. Second, w_0^S should provide higher utility than w_1^D: $U^S\left(w_0^S\right) > U^D\left(w_1^D\right)$. Again, the second condition is implied by the first, given the efficient separation condition $(1-z)\,y < U^u$.

To summarize, the equilibrium choice of layoff policy can be characterized by choosing the minimum cost among: $\kappa^N = \alpha y$, $\kappa^S = \sigma$, and $\kappa^D = \left(1-\frac{\theta}{2}\right) zy$.In partial equilibrium with exogenous wages, on the other hand, the last cutoff is different: $\kappa^D = w - (1-z)\,y$, see equations (A7).[66]

A.3 The optimal mandate

To study the optimal duration of the mandate, we extend our model by introducing a mixed strategy for the firm's information sharing decision. Rather than making a binary choice, the firm now determines the probability of sharing information, denoted p. The mandate imposes a minimum requirement for the probability of information sharing, denoted by m. Probabilistic MN duration is a convenience assumption that allows us to abstract from the complications arising because of fixed duration; see Pissarides (2001) for a similar approach.

In cases where the notice period is shorter than the mandate, severance pay is given by $S(p, m) = (m-p)\,\sigma$; severance thus compensates for the deviation from the mandate, and captures the value for workers of receiving notification.

The zero-profit condition when adhering to the mandate is

$$(2-\theta)\left(y - w_0^N(m)\right) = \theta m \alpha y$$

and if the firm is paying severance

$$(2-\theta)\left(y - w_0^S(m)\right) = \theta S(0, m)$$

[66] If we factor in the productivity loss of notice, this would be $\kappa^D = w - (1-\alpha)(1-z)\,y$.

The wages consistent with zero-profits are determined by $\pi^i\left(w_0^i(m)\right)=0$. Thus

$$\begin{aligned}
w_0^N(m) &= y-\frac{\theta}{2-\theta}m\alpha y \\
w_0^S(m) &= y-\frac{\theta}{2-\theta}m\sigma \\
w_0^D(m) &= y-\frac{\theta}{2}zy
\end{aligned}$$

Thus, the firm's decision rules remain largely unchanged, with the only difference being that compliance with the law entails giving notice in accordance with m. Severance compensation and the productivity loss are thus proportional to the mandate, implying $\kappa^N = m\alpha y$, $\kappa^S = m\sigma$, and κ^D is unchanged.

We denote worker utility in equilibrium as V^i for $i \in \{N, S, D\}$. These represent the expected utilities for each match at the beginning of two periods, in contrast to U^i for $i \in \{n, u\}$ used previously, which denote the utility after notification or layoff.

For N we have $p = m$, and

$$\begin{aligned}
V^N &= (2-\theta)\, w_0^N(m)+\theta\left[mU^n+(1-m)\,U^u\right] \\
&= (2-\theta)\, y+\theta\left[m(\sigma-\alpha y)+U^u\right]
\end{aligned}$$

This has two implications. First,

$$\frac{\partial V^N}{\partial m}=\theta(\sigma-\alpha y)$$

Second,

$$\begin{aligned}
V^D-V^N &= \theta\left[\{(1-z)\,y-U^u\}-m\{\sigma-\alpha y\}\right]<0 \\
V^D-V^S &= \theta\left[(1-z)\,y-U^u-m\sigma\right]<0
\end{aligned}$$

At the boundary, i.e., for the marginal cases,

$$\begin{aligned}
V^D-V^N &= \theta\left[w_0^D-U^u-m\sigma\right] \\
V^D-V^S &= \theta\left[w_0^D-U^u\right]
\end{aligned}$$

Given that profits are zero in equilibrium, social welfare is

$$V=\sum\int_{\Omega^i}V^i d\mu$$

To determine the properties of the optimal mandate in the most general terms, we write the first order condition with the respect to mandate duration using the multi-dimensional version of the Leibniz rule (Flanders, 1973):

$$\int_{\Omega^N}\frac{\partial V^N}{\partial m}=\int_{\partial\Omega^D\cap\Omega^N}\iota\left(V^N-V^D\right)+\int_{\partial\Omega^D\cap\Omega^S}\iota\left(V^S-V^D\right) \tag{A8}$$

where ∂ boundary operator and ι denotes, with slight abuse of notation, the interior product with the vector field of the velocity.

Consider a marginal extension of MN. Such a change has two effects. First, it extends notice for the sub-set of the population already receiving notice (infra-marginal effect). Second, the cost of providing notice increases, and marginal cases are moved between states, N, S, and D. Moves from notification to severance are, in general, not relevant for welfare as severance exactly compensates the marginal worker for not receiving the mandated amount of advance notice.[67] The same is not true for the marginal cases pushed from notice or severance to delay, where there is a discontinuous reduction in utility. In other words, delays are inefficient.

In its most general form, the first order condition that characterizes the optimal duration of MN is:

$$P^N \ \mathbb{E}\left(\frac{\partial V^N}{\partial m}|\Omega^N\right) = \frac{\partial P^D}{\partial m} \ \mathbb{E}\left(V^o - V^D|\partial D\right)$$

where ∂ denotes the boundary operator and V^o is the alternative utility, being V^N or V^S depending on where we are on the boundary. The left-hand side represents the net gain of extending MN among those who receive longer notice due to the extension. The right-hand side represents the cost of extending MN among those whose separation is delayed due to the extension. To connect the optimal duration of MN formula to the data, we rewrite it in terms of observable moments as:

$$\underbrace{P^N \ \mathbb{E}\left(\sigma - \alpha y|\Omega^N\right)}_{\substack{\text{Net Production gain of info sharing} \\ \text{Infra-marginal}}} = \underbrace{\frac{\partial P^D}{\partial m} \ \mathbb{E}\left(\widetilde{w} - w^D|\partial D\right)}_{\substack{\text{Net Production loss of delaying} \\ \text{Marginal}}}$$

where $\widetilde{w}$ is post-displacement earnings for marginal workers. The left-hand-side measures the increase in aggregate production due to MN. It is unambiguously positive since notice is given when $\sigma \geqslant \alpha y$. In other words, firms provide notice if and only if information sharing is production efficient. The ability of firms to pay severance is crucial: Although the payment of severance has no effects on aggregate production, the possibility of paying severance implies that firms avoid inefficient notice. The right-hand-side measures the aggregate production loss due to MN. It is unambiguously positive since separation is efficient in the second period. In general equilibrium, the efficiency loss is proportional to the difference between the re-employment wage and the wage in jobs where separation is delayed. Intuitively, $\widetilde{w} \geqslant w^D$, since equilibrium wages must be low in firms that would delay when hit by a negative productivity shock.

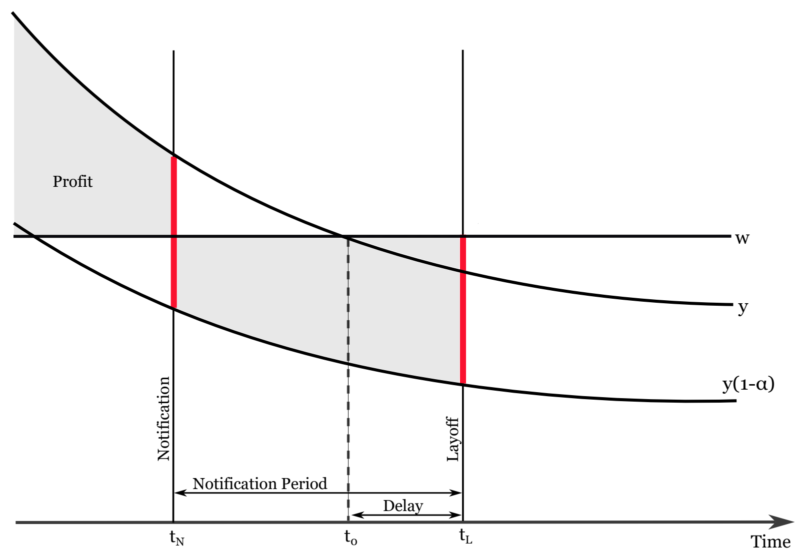
A.4 The delay caused by MN

To study delay, we consider a continuous-time model with an exogenous wage. Productivity, $y(t)$, is differentiable and decreasing over time: $\partial y/\partial t < 0$. Firms face a mandate of m periods and a severance-pay function of $\sigma(n)$, which captures workers' willingness to pay for notice. Our setting is illustrated in Figure A1. Among other things, this figure shows that in the absence of a mandate,

[67] In our probabilistic two-period model, the boundary between N and S does, in fact, not move, but this is an artifact of the model rather than a general result.

layoff occurs at t_0 where worker productivity is equal to wage, $y(t_0) = w$.

Figure A1: Timing of notification and delay



Notes: This figure shows the optimal timing of notification and delay chosen by the firm facing exogenous fixed wage and falling productivity.

First, we consider the problem of choosing the timing of notification and layoff – t_N and t_L respectively – for a firm that wants to give n periods of notice:

$$\max_{t_N, t_L} \int_0^{t_N} y(t) - w \, dt + \int_{t_N}^{t_L} (1-\alpha) y(t) - w \, dt$$

under the constraint

$$t_L = t_N + n$$

The optimal timing of notification is defined by equalizing net marginal profits at the time of notification and layoff

$$[y(t_N) - w] - [(1-\alpha) y(t_N) - w] + [(1-\alpha) y(t_L) - w] = 0$$

which is illustrated by the two red-lines in Figure A1. Substituting the constraint into the first order condition, we find that optimal notification time is given by

$$\alpha y(t_N(n)) + (1-\alpha) y(t_N(n) + n) = w \tag{A9}$$

The weighted average of marginal productivity at the time of notification and at the time of layoff must equal the wage, with the weighting factor being the productivity loss of notice. To see the connection to our framework in the main text, the counterpart of equation (A9) is the comparison between the cost of notice and the cost of delay, that is, αy and $w-(1-\alpha)(1-z)y$, or equivalently comparing the wage with the weighted average of productivities, $\alpha y+(1-\alpha)(1-z)y$.

From equation (A9) it follows that t_N is increasing in α. Therefore, delay is increasing in the productivity loss of notice. To see this, note that the left hand side of equation (A9) is increasing in α and decreasing in t_N.

Comparative Statics We can evaluate the change in delay with respect to the notice period, n, by taking the derivative of equation (A9) with respect to n:

$$\frac{\partial t_N(n)}{\partial n}=-\frac{(1-\alpha)\frac{\partial y(t_L(n))}{\partial t}}{\alpha\frac{\partial y(t_N(n))}{\partial t}+(1-\alpha)\frac{\partial y(t_L(n))}{\partial t}} \tag{A10}$$

In the case of a linear production decline, we get $\frac{\partial t_N(n)}{\partial n}=-(1-\alpha)$. This implies $\frac{\partial t_L(n)}{\partial n}=\alpha$. An increase in the notification period, advances notification by $1-\alpha$ period and postpones layoff by α period. In other words, delay is proportional to α,

$$t_L(n)-t_0=\alpha n$$

The relative slope of the productivity function at the notification point and the lay-off point also matters. In particular, if $\frac{\partial y(t_N(n))}{\partial t}$ is much smaller than $\frac{\partial y(t_L(n))}{\partial t}$, then a one-period increase in the notification period, advances notification by one period and does not affect the timing of layoff. Conversely, if the opposite is true, a one-period increase in the notification period, does not change the timing of notification and causes a one-period increase in delay.

Denote by $\sigma(n)$ the willingness to pay of the worker for n period of notice. We can micro-found $\sigma(n)=U^n(w,n)-U^u$

$$rU^n+\frac{\partial U^n}{\partial t}=\max_x w+\lambda(x)(x-U^n)$$

where x denotes the target wage and $U^n(w,t_L)=U^u$.

First-best notice equates the marginal gain and loss of notice. That is,

$$\sigma\left(n^{FB}\right)=w-(1-\alpha)\,y\left(t_L\left(n^{FB}\right)\right) \tag{A11}$$

So the first-best layoff time is decreasing in α.

Now, consider the problem for the firm choosing the notice period, when it has the option of paying severance. This second-best notice policy given a mandate of m is as follows. Either the firm obeys, $n^{SB}=m$, or the firm provides the first best notice, $n^{SB}=n^{FB}$, and tops it off with severance.

$$n^{SB}=\begin{cases} m & \text{if} \quad m<n^{FB} \\ \\ n^{FB} & \text{if} \quad m\geqslant n^{FB}\end{cases} \tag{A12}$$

Consider an increase in MN, $\Delta m>0$. Firms with $m\geqslant n^{FB}$ just pay more severance and continue to give first-best notice, $n=n^{FB}$. Others, increase their notice period by Δm. This increase involves more rapid information sharing – $\Delta t_N < 0$ – which is efficient, but also a delay in separation – $\Delta t_L > 0$ – which is inefficient.

Consider the extreme case where there is no production loss due to notice, i.e., $\alpha = 0$. In this scenario we have $\frac{\partial t_N(n)}{\partial n} = -1$, according to equation (A10), indicating that the timing of notification changes one-for-one with the duration of notice.[68] A marginal extension of MN then leads to a corresponding one-to-one increase in the actual notice when $m < n^{FB}$. Since the timing of notificaiton completely offsets the increase in notice duration, the timing of separation remains unchanged. In other words, there is no delay when $\alpha = 0$.

B Online Appendix: Empirical Part

B.1 RD validity tests

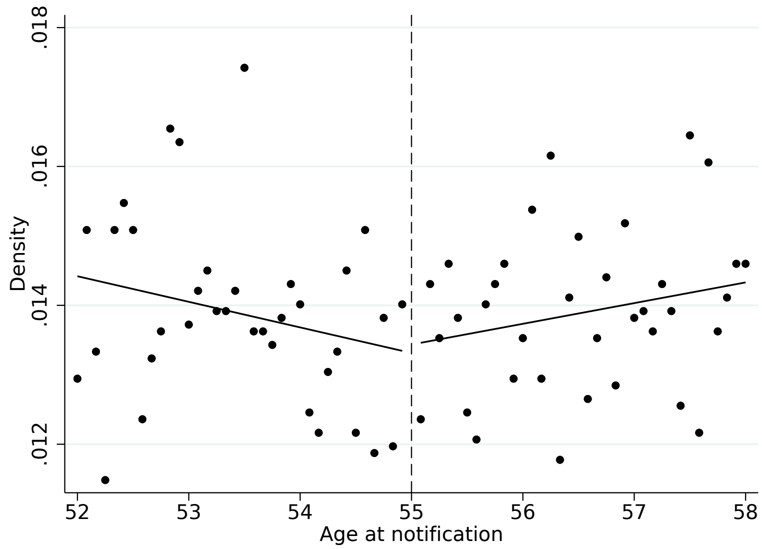
A possible concern is that firms try to selectively displace low-cost workers, along the lines of the insider-outsider theory (see Lindbeck and Snower, 1989). In our setting, this would manifest itself through more laid-off workers just to the left of the age-55 threshold. Figure A2 examines whether there is manipulation around the age-55 threshold by comparing the number of observations in the vicinity of the threshold. There is no evidence of suspect bunching on either side of the threshold.

Table A1 investigates whether baseline covariates are evenly distributed across the age-55 threshold. Columns (1)-(4) examine overall balancing. We regress an indicator for being above the age-55 threshold on all baseline characteristics and polynomial control functions in age. We are mainly interested in the F-statistics, reported at the bottom end of the table, which test the null hypotheses that all coefficients on individual (and firm) characteristics are jointly zero. As indicated by the p-values of the F-tests we cannot reject these hypotheses. Also, the individual coefficients are typically small.

Columns (6) and (7) report bivariate tests of equality of baseline covariates above and below the threshold. These tests reinforce the view that the coefficients are generally small: those just above the threshold earned 0.85% less than those just below the threshold according to the estimates in Column (6), for instance.

[68] If $\alpha = 0$, the duration of notice in the first best is at its maximum, that is, $y\left(t_L\left(n^{FB}\right)\right) = w - \sigma\left(n^{FB}\right)$using equation (A11). This condition implies that the notice period given the mandate is also at its maximum, see (A12).

Figure A2: Number of observations by age at notification



Notes: The figure shows the distribution of displaced individuals by age at notification (measured in months). The regression lines come from estimating a regression corresponding to equation (6) with the fraction of observations at each age bin as the outcome variable. The regression includes a linear age polynomial interacted with the threshold dummy. The estimated jump at the threshold is 0.0005 (standard error = 0.0006, p-value = 0.435).

Table A1: Balancing of pre-determined covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Earnings_{t-1}	-0.0041	-0.0034	-0.0018	-0.0049	-0.0004	-0.0085	0.0189
	(0.0052)	(0.0054)	(0.0056)	(0.0077)	(0.0036)	(0.0221)	(0.0339)
Female	0.0037	0.0044	0.0041	0.0001	0.0021	0.0165	0.0012
	(0.0052)	(0.0053)	(0.0058)	(0.0083)	(0.0036)	(0.0205)	(0.0314)
Immigrant	-0.0040	-0.0028	-0.0023	0.0040	-0.0002	0.0008	0.0021
	(0.0072)	(0.0075)	(0.0075)	(0.0099)	(0.0048)	(0.0119)	(0.0187)
Tenure	-0.0060**	-0.0058**	-0.0057*	-0.0074	-0.0026	-0.0662**	-0.0756
	(0.0029)	(0.0029)	(0.0029)	(0.0055)	(0.0018)	(0.0333)	(0.0514)
Highest attained Education							
Primary	-0.0163	-0.0167	-0.0142	-0.0030	-0.0001	0.0086	0.0107
	(0.0179)	(0.0180)	(0.0184)	(0.0241)	(0.0112)	(0.0128)	(0.0195)
High school	-0.0237	-0.0256	-0.0233	-0.0098	-0.0061	-0.0302	-0.0588
	(0.0165)	(0.0165)	(0.0169)	(0.0220)	(0.0103)	(0.0198)	(0.0320)
College	-0.0159	-0.0178	-0.0167	-0.0045	-0.0014	0.0167	0.0446
	(0.0163)	(0.0164)	(0.0165)	(0.0210)	(0.0101)	(0.0192)	(0.0305)
Firm Characteristics			✓		✓		
Polynomial order							
1st degree	✓	✓	✓	✓		✓	
2nd degree					✓		✓
Interacted w. threshold	✓	✓	✓	✓	✓	✓	✓
Month/Year FE		✓	✓		✓	✓	✓
Displacement FE				✓			
F-statistic	1.36	1.30	1.39	0.51	0.85	.	.
p-value	0.220	0.245	0.163	0.830	0.595	.	.
R^2	0.766	0.766	0.766	0.765	0.904	.	.
Number of observations	10,275	10,275	10,275	10,275	10,275	10,275	10,275

Notes: The table shows balancing of covariates at the age-55 threshold for notified white-collar workers aged 52-58 at the time of notification. Columns (1)-(5) show the results of regressing an indicator for being above the age–55 threshold on baseline covariates and polynomial control functions in age. The bottom part of the table reports the F-statistic and the associated p-value from testing the null hypothesis that all coefficients on (individual and firm) baseline covariates are jointly zero. Firm characteristics included in Columns (3) and (5) are workforce characteristics – average earnings, share of females, share of immigrants, average age, share of college-educated, and number of employed. All firm characteristics are balanced, except average age in Column (3), which is 0.0014 years higher for individuals above the age-55 threshold. Column (6) and (7) report the results of bivariate balancing tests where each covariate listed in the left-hand column is regressed on the treatment indicator and an interacted first and second order polynomial control function in age at notification, respectively. Standard errors are clustered on notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

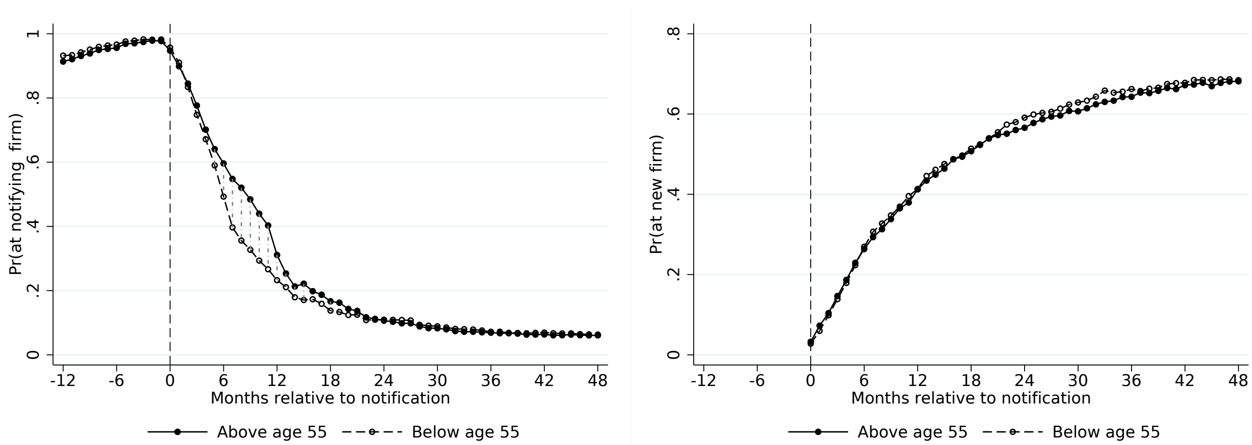
B.2 Optimal bandwidth

Figure A3 show the dynamic effects of MN on Employment using the default bandwidth of ±3-years. Figure A4 reproduces Figure A3 but with the optimal bandwidth selector of Calonico et al. (2014). The figure is built from 48-60 separate RD-regressions, and, consequently, there are 48-60 optimal bandwidths. In general, optimal bandwidths are in between 2.0 and 4.0 years, and our default bandwidth of 3 is thus well in-line with the optimal ones. There are instances when bandwidth-selector picks 1.8 or 4.5 years as the optimal ones, but these are rare occasions.

The most important message from A4, however, is that none of our results change when we use this approach rather than the one we opt for in the main text. Conceptually, since age is discrete in our data, we prefer the parametric approach of the main text. Moreover, Appendix C illustrates that the optimal bandwidth selector is sensitive to measurement error in the assignment variable. Our default approach also avoids the slightly cumbersome exercise of using potentially different data sets for each single point estimate.

Figure A3: Dynamic effects of mandated notice on employment

(a) Probability of remaining at the notifying firm (b) Probability of working at a new Firm

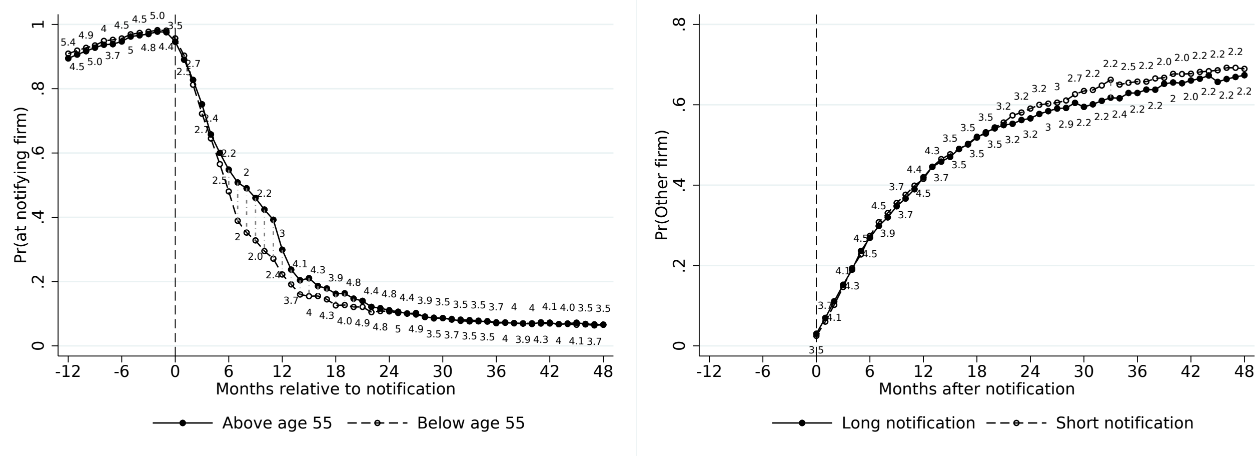


Notes: The figures show employment outcomes by month relative to notification. A new firm is defined as a new employer-employee spell that i) pays more than 10 kSEK per month; and ii) the worker has not derived any income from during the 12 months preceding notification. We set the indicator of working at the notifying firm to zero in case of a new job (so that the two employment markers are mutually exclusive). At any given point in time, we plot estimates of the constant (hollow circles) and the constant+β (black circles) from a regression corresponding to equation (6) where the outcome is one of the employment outcomes. Dashed vertical lines indicate that the estimate of β is significant at the 5% level. These regressions include a linear age polynomial interacted with the threshold indicator, individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs), and month-by-year FE:s. The analysis includes individuals aged 52-58 at the time of notification. Standard errors are clustered on notification event.

Figure A4: Employment by month relative to notification (optimal bandwidth)

(a) Probability of remaining at the notifying firm

(b) Probability of working at a new firm



Notes: The figure shows the probability of working at the notifying firm (panel a) and at a new firm (panel b) by month relative to notification. At any given time point, we plot estimates of the constant (hollow circles) and the constant+β (black circles) from a local linear regression corresponding to (6) with an optimal bandwidth according to Calonico et al. (2014), which is indicated by the number next to each point in the graph. Dashed lines indicate that the estimate of β is significant at the 5%-level. The regressions include baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs) and month-by-year FE:s. Standard errors are clustered on notification event.

Table A2: Sensitivity of RD-design to bandwidth and functional form

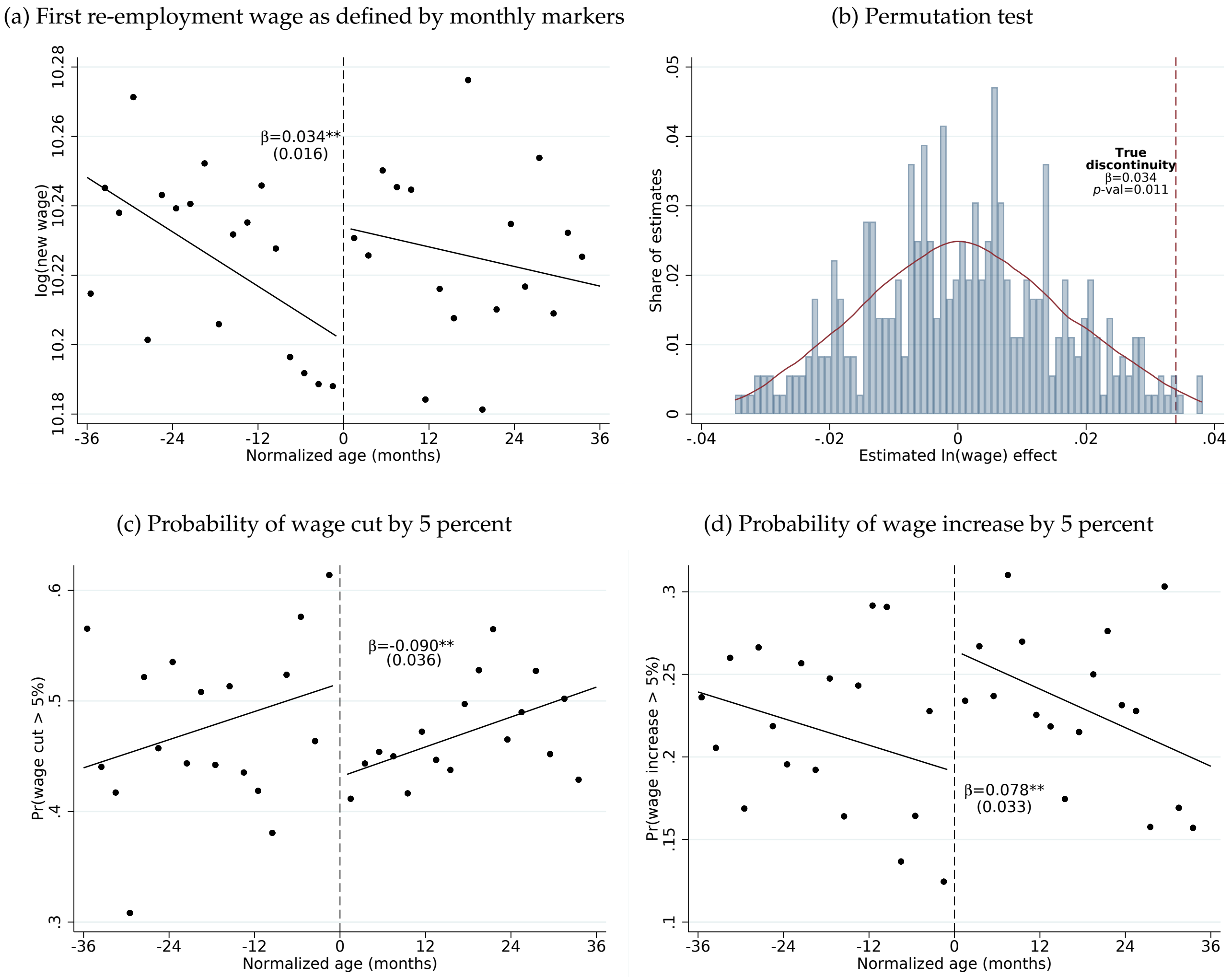
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Polynomial degree:	1	1	1	1	1	1	1	2	2	2	2	2	2
Donut RD	No	No	No	No	Yes	No	No	No	No	No	Yes	No	No
Bias-corrected	No	No	No	No	No	No	Yes	No	No	No	No	No	Yes
Notification time (days)	2.59***	1.25***	2.06***	2.76***	3.23***	1.75***	1.00**	0.42	0.99**	2.06***	2.59***	1.23***	0.82*
	(0.20)	(0.36)	(0.26)	(0.19)	(0.26)	(0.30)	(0.44)	(0.62)	(0.40)	(0.28)	(0.54)	(0.34)	(0.47)
Bandwidth	3.00	1.00	2.00	4.00	3.00	1.54	1.54	2.00	3.00	4.00	3.00	2.88	2.88
Polynomial degree	1	1	1	1	1	1	1	2	2	2	1	2	2
# observations	10,275	3,298	6,776	13,786	9,453	5,061	78,562	3,298	6,776	13,786	9,453	9,687	78,562
Annual earnings $t+1$ **(1000 SEK)**	38.487***	21.716	37.980***	43.324***	42.219***	40.694***	28.232**	23.378	27.148	33.109**	42.712*	33.020**	27.413*
	(10.552)	(16.048)	(12.116)	(9.708)	(13.202)	(10.970)	(14.143)	(26.415)	(17.734)	(13.431)	(24.314)	(12.860)	(15.711)
Bandwidth	3.00	1.00	2.00	4.00	3.00	2.52	2.52	2.00	3.00	4.00	3.00	4.20	4.20
Polynomial degree	1	1	1	1	1	1	1	2	2	2	1	2	2
# observations	10,275	3,298	6,776	13,786	9,453	8,543	78,562	3,298	6,776	13,786	9,453	14,406	78,562
Months non-employed within 2 years	-1.177***	-1.162**	-1.078***	-1.451***	-1.074***	-1.046***	-0.989***	-1.167	-1.609***	-0.915**	-0.702	-1.058***	-0.843**
	(0.288)	(0.502)	(0.335)	(0.272)	(0.358)	(0.310)	(0.376)	(0.859)	(0.555)	(0.373)	(0.648)	(0.340)	(0.390)
Bandwidth	3.00	1.00	2.00	4.00	3.00	2.39	2.39	2.00	3.00	4.00	3.00	4.82	4.82
Polynomial degree	1	1	1	1	1	1	1	2	2	2	1	2	2
# observations	10,275	3,298	6,776	13,786	9,453	7,963	78,562	3,298	6,776	13,786	9,453	16,564	78,562
Severance pay (1,000 SEK)	17.045**	3.217	10.434	18.730***	20.618**	16.425**	6.161	5.181	3.964	13.123	11.083	12.808	5.500
	(7.714)	(13.436)	(8.564)	(6.837)	(10.472)	(7.902)	(11.055)	(27.225)	(14.322)	(9.038)	(16.775)	(8.925)	(12.434)
Bandwidth	3.00	1.00	2.00	4.00	3.00	2.64	2.64	2.00	3.00	4.00	3.00	4.14	4.14
Polynomial degree	1	1	1	1	1	1	1	2	2	2	1	2	2
# observations	9,850	3,146	6,470	13,223	9,057	8,452	74,747	3,146	6,470	13,223	9,057	13,512	74,747
log(wage)	0.034**	0.083***	0.050**	0.030**	0.037*	0.034**	0.038**	-0.005	0.070**	0.047**	0.097**	0.057**	0.057**
	(0.016)	(0.030)	(0.020)	(0.013)	(0.020)	(0.016)	(0.018)	(0.049)	(0.031)	(0.021)	(0.038)	(0.024)	(0.027)
Bandwidth	3.00	1.00	2.00	4.00	3.00	3.07	3.07	2.00	3.00	4.00	3.00	3.19	3.19
Polynomial degree	1	1	1	1	1	1	1	2	2	2	1	2	2
# observations	2,752	914	1,856	3,625	2,528	2,752	21,655	914	1,856	3,625	2,528	2,912	21,655
Δ log(wage)	0.032**	0.073**	0.045**	0.018	0.027	0.045**	0.051**	0.008	0.068**	0.047**	0.060	0.060**	0.057**
	(0.016)	(0.029)	(0.019)	(0.013)	(0.020)	(0.019)	(0.021)	(0.051)	(0.031)	(0.021)	(0.038)	(0.025)	(0.028)
Bandwidth	3.00	1.00	2.00	4.00	3.00	2.21	2.21	2.00	3.00	4.00	3.00	2.90	2.90
Polynomial degree	1	1	1	1	1	1	1	2	2	2	1	2	2
# observations	2,276	753	1,528	2,970	2,093	1,660	16,554	753	1,528	2,970	2,093	2,150	16,554

Notes: The table probes the robustness of our main results. The first column replicates the baseline estimates for different outcomes. The remaining columns show estimates for the same outcomes, varying the bandwidth, the polynomial degree (see the first row), and the use of bias-corrected methods (Stata package rdrobust). Columns (2)-(4) present estimates akin to the baseline but instead apply bandwidths 1, 2, and 4 months. In (5) we exclude observations 3 months just above and below the threshold in a "donut RD". In (6), we rely on the parametric specification in equation (6) but apply the optimal bandwidth suggested by Calonico et al. (2014). Column (7) instead displays bias-corrected estimates using the optimal bandwidth selector of Calonico et al. (2014). Columns (8)-(13) repeat the exercise with a 2nd order polynomial instead. Standard errors are clustered by notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.3 MN wage effect

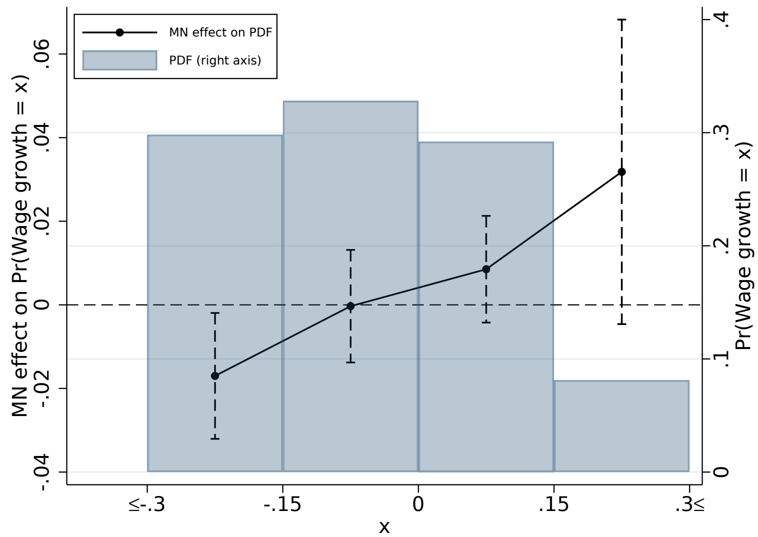
Additional evidence on MN effect on wages

Figure A5: Effect of MN on re-employment wage



Notes: Panel (a) shows the log of the wage from the first new job after notification by age at notification relative to the 55-threshold, corresponding to the estimate in Table 3 Column 1. In contrast to Figure 3a, the first new job is defined via the monthly markers as the first employer the worker is observed working for after notification for whom she has not worked for at least one year prior to notification. The wage refers to full-time equivalent monthly wage reported in the Wage Survey by this particular employer within two years after notification. Sample size is 2,752. Panel (b) shows results from a permutation test where we vary the threshold between the ages 30-60 (at the monthly level), keeping a fixed bandwidth of +/- 3 years. The figure plots the distribution of the 360 placebo estimates from fictitious discontinuities including the true estimate (shown in panel a), indicted by the red dashed vertical line. Panel c) and d) show the probability of the wage at the first new job being either 5 percent lower or higher, respectively, compared to the worker's wage at the notifying firm. All estimates are based on equation (6) controlling for age linearly interacted with the threshold indicator for individuals aged 52-58 (except panel (b) where we vary the threshold) at the time of notification. The estimated jump at the threshold and its standard error are displayed in the figures. The regressions include individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs) and month-by-year FE:s. Standard errors are clustered by notification event

Figure A6: Effects of MN on the distribution of wage changes



Notes: The figure plots the MN effect on the PDF of wage growth distribution on the left y-axis (marked solid line) and surrounding 95%-confidence intervals (dashed vertical lines). The PDF itself is on the right y-axis. The regression estimates come from estimating equation (6) with a linear age polynomial interacted with the threshold indicator. The regressions also include baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs) and month-by-year FE:s. The analysis only includes individuals aged 52-58 at the time of notification. Standard errors are clustered on notification event.

Table A3: Effect of MN on wage dynamics

	(1)	(2)	(3)	(4)
Years Relative to New Job After Notice	$t+1$	$t+2$	$t+3$	$t+4$
Above Age-55	-0.005	-0.000	-0.018*	-0.026**
	(0.005)	(0.008)	(0.010)	(0.012)
Control mean	0.012***	0.028***	0.062***	0.082***
	(0.003)	(0.005)	(0.007)	(0.009)
Number of clusters	1,819	1,573	1,443	1,246
Number of observations	3,092	2,558	2,302	1,923

Notes: This table shows regression estimates of equation (6) where the outcome is wage growth. We examine notified workers who find a new job at some point after notification. For those workers, we measure wage growth relative to the wage in the new job. In Column (1), for example, we show the difference between the log wage one year after re-employment and the log wage in the year of re-employment. We include a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification as controls. The regressions also include individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs) and month-by-year FE:s. Standard errors are clustered on notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Role of sorting in the MN wage effect

Higher firm quality partly explain our positive wage effects. Table A4 presents estimates of the effect of longer MN on characteristics of the new firm. We measure all outcomes in the year prior to notification, and thus before the worker joins the firm, so that the firm outcomes are not affected by the worker. With varying statistical significance, longer notification is associated with higher wages,, older workers, and larger firms. Estimates of the effects on firm productivity and profits per worker are positive but imprecise.

Table A4: Mandatory Notice (MN) effect on firm sorting

	(1)	(2)	(3)	(4)	(5)
	Panel (a): Firm characteristics using individual-level data				
	Log(New firm wage)	Log(New firm earnings)	Share males	Mean age	Log(firm size)
Above Age-55	0.014	0.000	-0.005	0.706**	0.017**
	(0.012)	(0.028)	(0.011)	(0.281)	(0.007)
Control Mean	10.191***	12.180***	0.425***	41.029***	3.702***
	(0.008)	(0.018)	(0.008)	(0.199)	(0.005)
Number of clusters	2,199	3,527	3,530	3,530	3,530
Number of observations	4,028	7,728	7,747	7,748	7,748
	Panel (b): Firm characteristics using firm-level data				
	Log(VA)	Log(sales per worker)	Log(VA per worker)	Profits per worker	
Above age-55	0.159	0.018	0.032	9.634	
	(0.141)	(0.067)	(0.053)	(83.313)	
Control mean	11.242***	6.683***	6.285***	84.568***	
	(0.101)	(0.045)	(0.036)	(66.234)	
Number of clusters	2,926	2,890	2,879	2,944	
Number of observations	5,637	5,549	5,515	5,683	

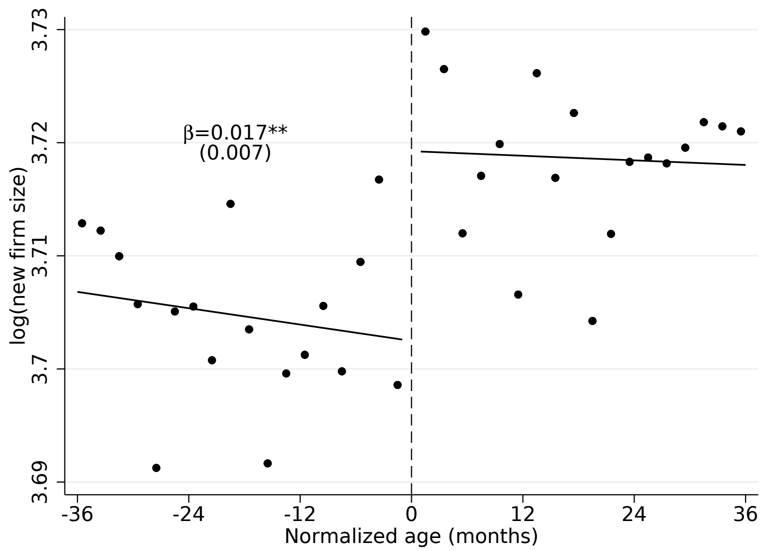
Notes: The table show estimated effects of longer mandated notice on the firm level characteristics of the new job. **Panel (a)** shows outcomes based on individual worker characteristics. New firm wage and New firm earnings refer to the average monthly wage and average annual earnings of the new firm, respectively. **Panel (b)** focuses on outcomes based on the balance sheets and income statements of firms. Regressions include individuals aged 52-58 at the time of notification. The regressions include a linear age polynomial interacted with the threshold indicator, baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs), and month-by-year FE:s. Standard errors are clustered by notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Effect of MN on new job characteristics

	(1)	(2)	(3)
	Changing		
	Industry (1-digit)	Industry (3-digit)	Occupation
Above age-55	-0.031	-0.003	-0.055
	(0.033)	(0.023)	(0.041)
Control mean	0.723***	0.882***	0.564***
	(0.025)	(0.018)	(0.030)
Number of clusters	1,732	1,732	1,402
Number of observations	2,849	2,849	2,185
	Job stability		
	Tenure	Separation within 1 year	Separation within 2 years
Above age 55	0.485	-0.021	0.041
	(2.117)	(0.021)	(0.031)
Control mean	51.538***	0.121***	0.253***
	(1.565)	(0.015)	(0.021)
Number of clusters	2,122	2,122	2,122
Number of observations	3,771	3,771	3,771

Notes: The table show the estimated effect of longer mandated notice on the characteristics of the new job. Regressions include individuals aged 52-58 at the time of notification and comes from estimating equation (6) with a linear age polynomial interacted with the threshold indicator. The regression also includes baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FE:s) as well as month-by-year FE:s. Standard errors are clustered by notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A7: The effect of MN on firm size



Notes: The figure plots the log of firm size (measured as the number of employees) of the first new job against age at notification relative to the 55-threshold in 2 months bins. The estimated jump at the threshold along with its standard error is depicted in the figure. The regression lines come from estimating equation (6) with a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification and corresponds to the estimate found in Column (5) of Table A4. The regression includes individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure at notification, educational attainment FE:s), and month-by-year FE:s. Standard errors are clustered on notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.4 Severance pay and cash-constrained firms

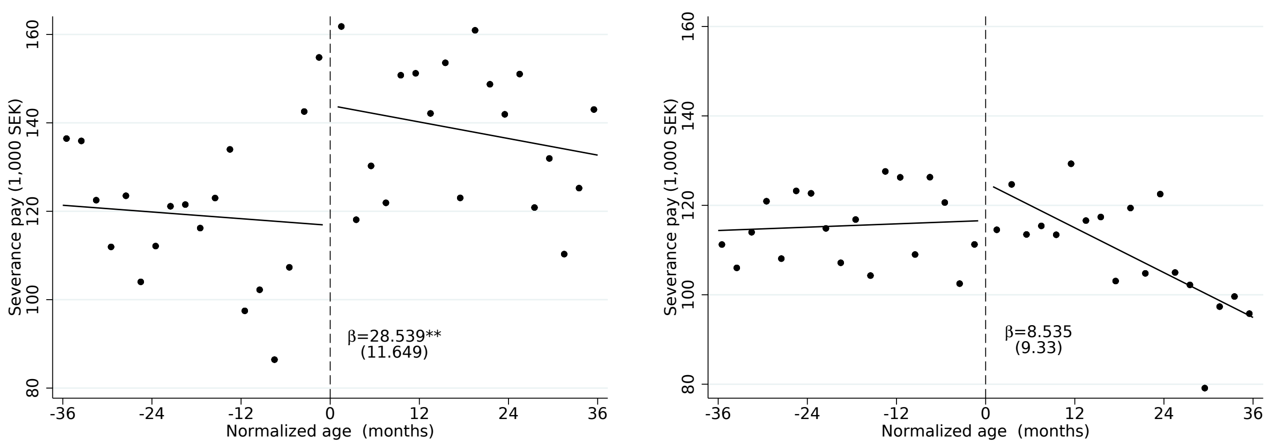
A key message of our theory in Section 1 is that the efficiency case for MN relies on firms being able to pay severance in the instances where giving advance notice would be inefficient. If some firms that lay off workers are credit constrained, it is possible that inefficient MN cannot be undone by such monetary side-payments.

To shed light on this issue, we examine whether the usage of severance payments is lower among cash-constrained firms. Empirically, we define a firm as cash-constrained if the share of liquid assets over total assets is below the mean. The information on the asset position of firms comes from balance sheets. Figure A8a shows severance pay for workers in unconstrained firms. Unconstrained firms pay about one additional month worth of severance to workers eligible for long notification. Figure A8b examines constrained firms. There is little indication of additional severance to workers eligible for longer notice. We interpret this as evidence suggesting that MN is associated with some efficiency loss for cash-constrained firms. Note, however, that our estimates of the gains and losses associated with MN already reflect that some firms are unable to pay severance. For instance, the inability to pay severance most likely inflates the estimate of α from Section 5.1 relative to a scenario where all firms could undo inefficient notice.

Figure A8: Effects of MN on severance by cash constraints

(a) Severance among unconstrained firms

(b) Severance among constrained firms



Notes: The figure shows severance pay and notification time by age at notification relative to the 55-threshold in 2-month-bins for cash-unconstrained firms and cash-constrained firms. Cash-unconstrained firms are defined as firms with the share of liquid assets over total assets (measured in the year before notification) above the mean (the share constrained firms is 0.65). Severance pay is measured as excess earnings in the year of displacement (detailed in Section 3.1). The estimated jump at the threshold along with its standard error is depicted in the figure. The regression lines come from estimating equation (6) with a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification. The regressions include baseline covariates earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FE:s) and month-by-year FE:s. Standard errors are clustered by notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.5 Additional results

Impact of MN on advance notice

Table A6: Effects of MN on advance notice duration, specification analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Above 55	2.415***	2.593***	2.623***	1.298***	1.561***	1.499***
	(0.215)	(0.203)	(0.198)	(0.388)	(0.347)	(0.379)
Control mean	6.832***	6.723***	6.916***	7.697***	7.498***	7.680***
	(0.213)	(0.171)	(0.171)	(0.349)	(0.277)	(0.309)
Polynomial order						
1st degree	✓	✓	✓			
2nd degree				✓	✓	✓
Interacted w. threshold	✓	✓	✓	✓	✓	✓
Baseline covariates		✓	✓		✓	✓
Month/Year FEs		✓			✓	
Displacement FEs			✓			✓
F-stat	125.97	162.43	176.25	11.18	20.22	15.66
R^2	0.085	0.178	0.231	0.087	0.180	0.235
Number of clusters	4,158	4,158	4,158	4,158	4,158	4,158
Number of observations	10,275	10,275	10,275	10,275	10,275	10,275

Notes: The table investigates the sensitivity of the first-stage with the outcome being notification time in days. The regression come from estimating equation (6) with a linear or quadratic age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification. Where indicated regressions also. baseline covariates consisting of earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FE:s. Standard errors are clustered on notification event * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Earnings decomposition

Two sources of approximation are (i) earnings and wage effects are estimated on two *calendar* year horizon; (ii) the wage effects are estimated on a subsample of the baseline population. Despite the approximations involved in the decomposition, the estimated and imputed severance pay effect line up fairly well, leading to the following decomposition:

$$\underbrace{\frac{\Delta y}{w_0}}_{\text{Earnings effect of MN}} = -\underbrace{\Delta NE}_{\text{non-employment dur. effect}} - \left(\frac{w_0 - w_1}{w_0}\right)\underbrace{\Delta l_1}_{\text{new job dur. effect}} + \underbrace{\frac{\Delta w_1}{w_0}}_{\text{wage effect}} l_1 + \underbrace{\frac{\Delta SP}{w_0}}_{\text{Severance pay effect}} \tag{A13}$$

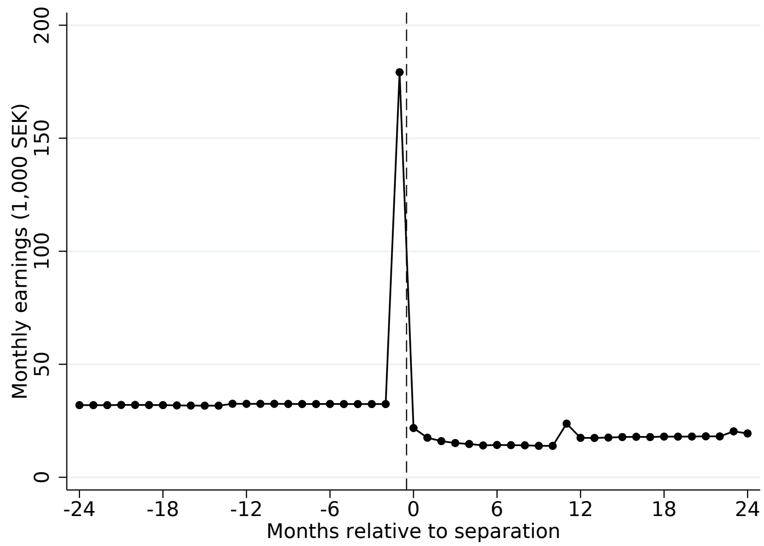
Table A7: Decomposition of 2-year cumulative earnings effect

	Component/monthly wage	Percent of earnings effect
Component		
Employment effects		
non-employment	1.177	59.2
new job	-0.008	-0.4
Wage effects		
wage at new job	0.272	13.6
imbalance in initial wage	0.008	0.4
Estimated severance pay effect	0.546	27.3
Imputed severance pay effect	0.148	
Sum of estimated components	1.987	100
Estimated earnings effect	1.599	

Notes: The table shows a decomposition of the 2-year cumulated earnings effect estimated at the age-55 threshold. Estimated earnings effect $= \Delta y / w_0^L$. Employment effects: non-employment $= -\Delta NE$; new job $= -\left[(w_0^L - w_1^L)\Delta l_1\right] / w_0^L$. Wage effects: wage at new job $= \left[l_1^S w_1^S \Delta \ln w_1\right] / w_0^L$; imbalance in initial wage $= \left[l_0^S w_0^S \Delta \ln w_0\right] / w_0^L$. Estimated severance pay effect $= \Delta SP / w_0^L$. The index L (S) denotes eligibility for long (short) notification. The employment effects come from Table 2, the wage estimate from Column (2) of Table 3, the severance pay from Figure 1b.

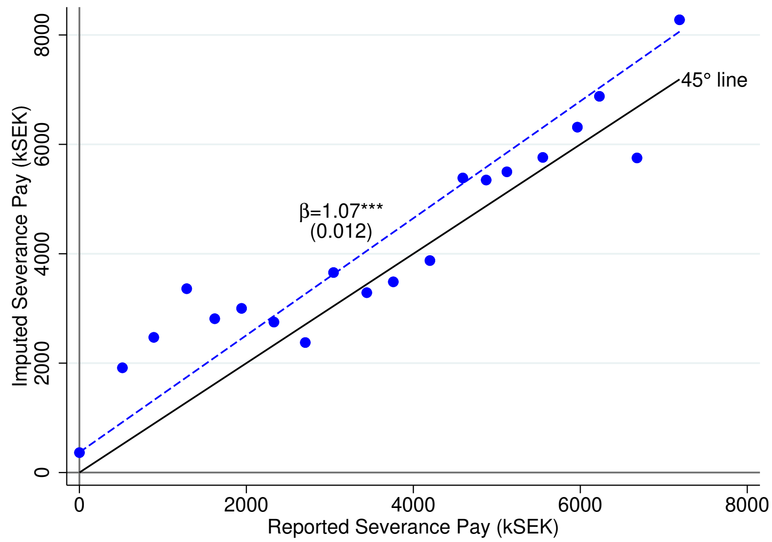
Measurement of severance pay

Figure A9: Earnings for workers for whom January is the last month with the notifying firm



Notes: The figure shows average monthly earnings (in 1,000 SEK) for workers whose last recorded month with the notifying firm is January in the year of separation. The sample includes individuals aged 52-58 at the time of notification. Earnings in t⩽0 come from the notifying firm whereas earnings in t>0 are summed over all employers and include zero earnings.

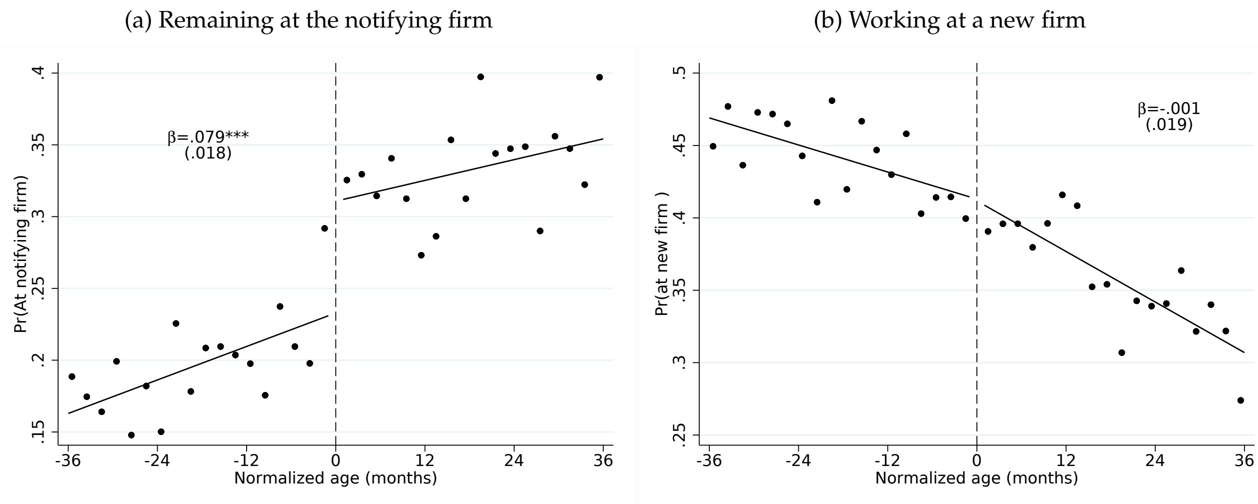
Figure A10: Firm-level correlation between imputed and actual severance payments



Notes: The figure probes our imputation of severance payments. For each worker who separates from her employer, we calculate the severance payment as the difference between total earnings from the separating employer in the year of separation and imputed annual earnings. The latter is defined as the CPI-adjusted monthly earnings from the separating employer in the year prior to separation times the number of months worked for that employer in the separation year. We censor severance payments from below at zero. We then sum the severance payments to the firm- and year-level. From Statistics Sweden we retrieve a supplemental dataset to the Balance Sheet and Income Statements. This supplement, available to a random sample (stratified by size) of around 14,000 firms annually, specifies firm-level costs in a more detailed manner than the income statements. Importantly, it includes total severance payments during the year. The plot is a binscatter of our imputed severance payments against the reported severance payments in the data. The slope coefficient is reported in the graph. Number of observations: 183,848.

Employment outcomes

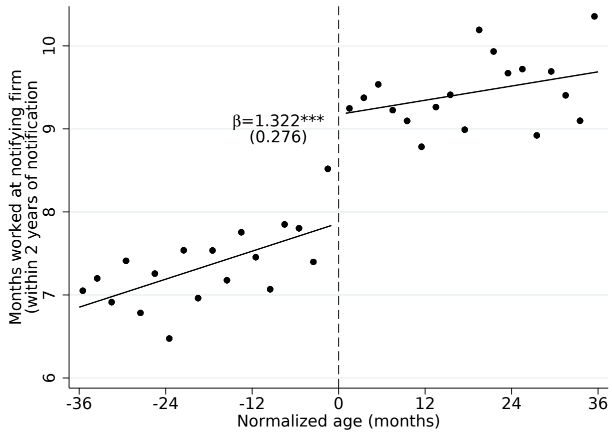
Figure A11: Employment outcomes 12 months after notification by age



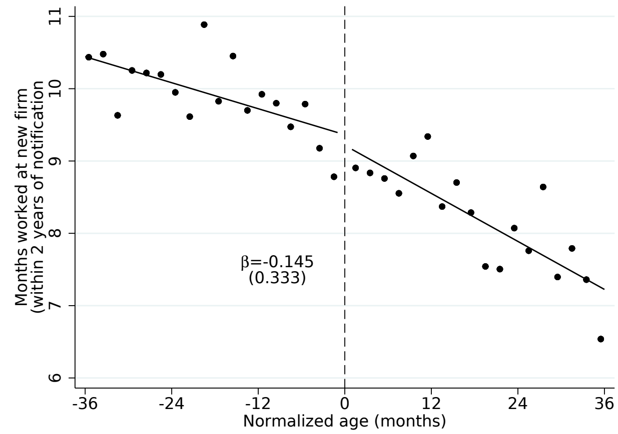
Notes: The figure shows employment outcomes 12 months after notification by age at notification (2-month-bins). The regression lines come from estimating equation (6) with a linear age polynomial interacted with the threshold indicator. The estimated jump at the threshold is depicted in the figure along with its standard error. The regressions include baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs) and month-by-year FE:s. The analysis only includes individuals aged 52-58 at the time of notification.

Figure A12: Employment status cumulated over 2 years after notification by age at notification

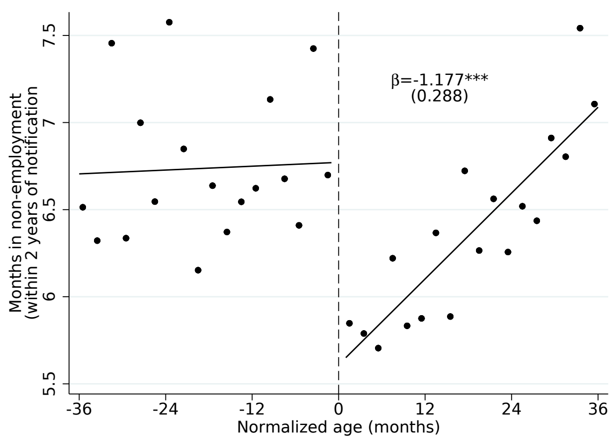
(a) Months at notifying firm



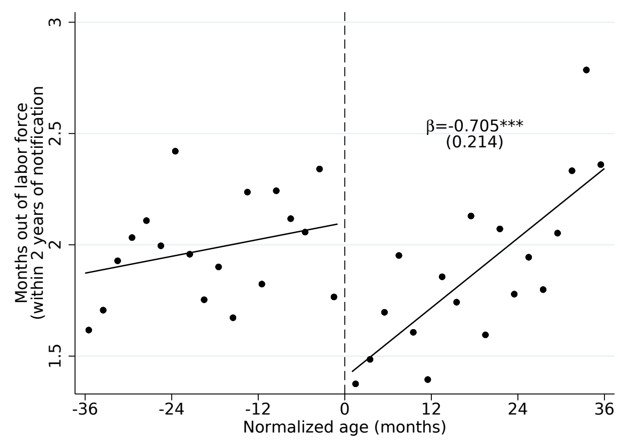
(b) Months at new firm



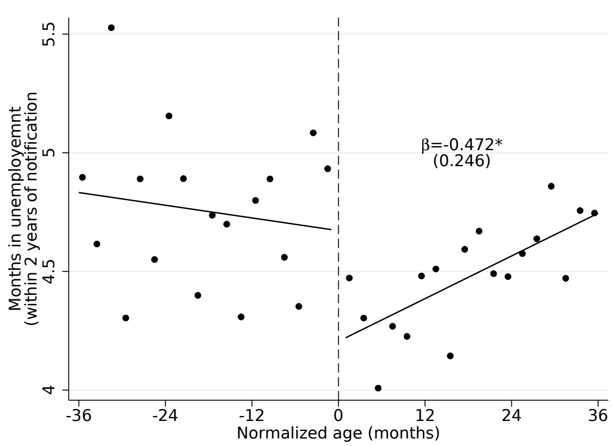
(c) Months in non-employment



(d) Months out of labor force



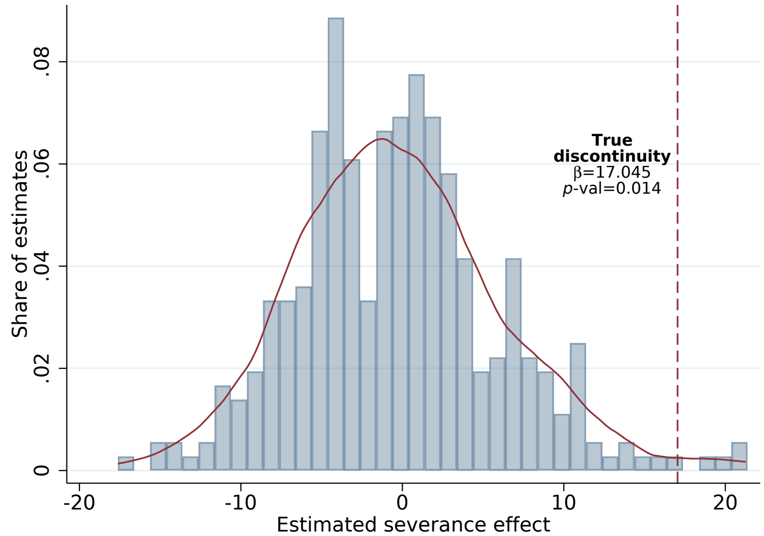
(e) Months in unemployment



Notes: The figure shows employment outcomes cumulated over 2 years post notification by age at notification relative to the 55-threshold in 2-month bins. These figures correspond to the estimates displayed in Table 2. The regression lines come from estimating equation (6) with a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification. The regressions also include individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure at notification, educational attainment FEs), and month-by-year FE:s. Estimated jumps at the threshold along with standard errors are depicted in the figures. Standard errors are clustered by notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Permutation test of the MN severance pay effect

Figure A13: Permutation test of the MN severance pay effect



Notes: The figure shows results from a permutation test where we vary the threshold between the ages 30-60 (at the monthly level), keeping a fixed bandwidth of +/- 3 years. The figure plots the distribution of the 360 placebo estimates from fictitious discontinuities including the true severance estimate, indicted by the red dashed vertical line.

Table A8: First Stage Estimates of Advance Notice and Severance Pay

	Search intensity		Months until new job		Non-employment		$\Delta \ln(w)$	
	Notification time (months)	Severance (1,000 SEK)	Notification time (months)	Severance (1,000 SEK)	Notification time (months)	Severance (1,000 SEK)	Notification time (months)	Severance (1,000 SEK)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Above age-55	2.218***	20.469***	2.253***	20.074***	2.628***	16.306**	2.116***	13.938
	(0.227)	(7.653)	(0.223)	(7.571)	(0.201)	(7.288)	(0.351)	(13.336)
Share coworkers above 55	0.321	39.968***	0.556	49.261***	0.804	42.250***	-0.334	25.331*
	(0.734)	(11.696)	(0.740)	(11.794)	(0.687)	(11.215)	(0.837)	(14.506)
Joint F-statistic	47.71424	8.623043	51.31399	11.53885	86.98985	8.586808	18.45528	1.928524
Number of clusters	3,973	3,973	4,023	4,023	4,237	4,237	2,530	2,530
Number of observations	34,359	34,359	35,486	35,486	53,723	53,723	12,320	12,320

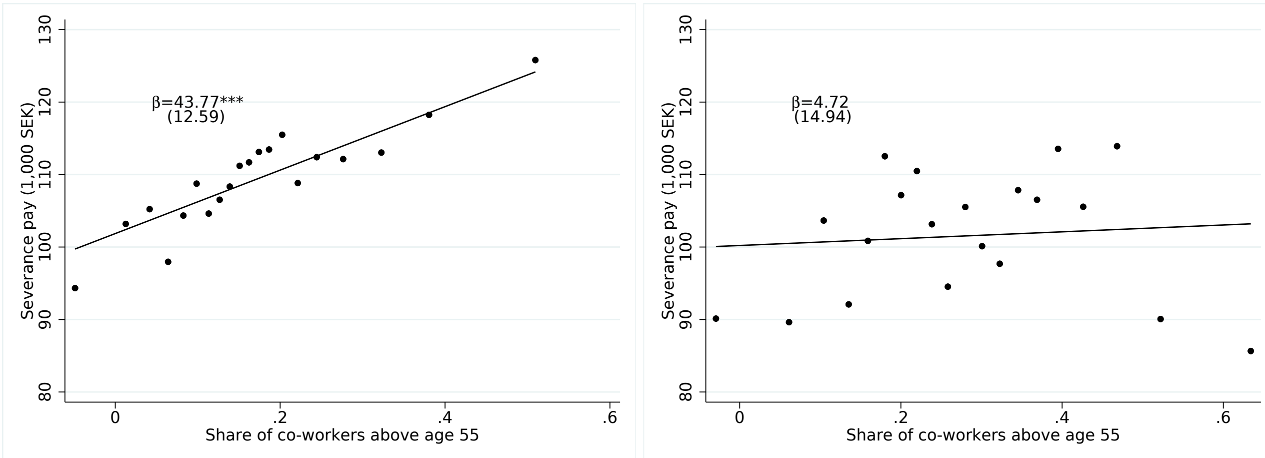
Notes: The table shows first stage estimates corresponding to Columns (3)-(6) in Table 4. The regression specifications are such that the effect of the above-55 indicator is identified within the sample of white-collar workers aged 52-58, while share coworkers above 55 is identified across all white-collar workers. All regressions include (i) a linear age-polynomial interacted with age-brackets FEs (the age brackets are 6 years wide, consistent with the 52-58 bracket), individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs), month-by-year FE:s. Individual covariates are interacted with a dummy for being close to the threshold (age 52-58). (ii) Firm covariates: average age of workers, average age squared, average earnings of workers, share female workers, share college educated and firm size. (iii) Layoff characteristics: size of layoff and flexible controls for average tenure within layoff. (iv) 2-digit industry FEs. Standard errors are clustered by notification event. The sample comprises all white-collar workers in notification events where a white-collar worker aged 52-58 was notified. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Supporting material, 2-IV analysis

Figure A14: Spillover of the share coworkers aged 55 and above on severance pay

(a) Workers below age 55

(b) Workers above age 55



Notes: Panel (a) shows the relationship between severance pay and the instrument, i.e., the share of coworkers aged 55 and above, for workers who themselves are below age 55. Panel (b) shows the corresponding relationship for those above age 55. The plots show residualized relationships where we control for a linear age polynomial interacted with age bracket FEs (in 6 year bins), individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs), month-by-year FEs. Individual covariates are interacted with a dummy for being close to the threshold (age 52-58). We also control for firm covariates consisting of average age of workers, average age squared, average earnings of workers, share female workers, share college educated, firm size and layoff characteristics that include size of layoff and flexible controls for average tenure within layoff. Regression estimates are depicted in the figure along with its standard error clustered by notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Balancing tests with respect to share coworkers above age-55

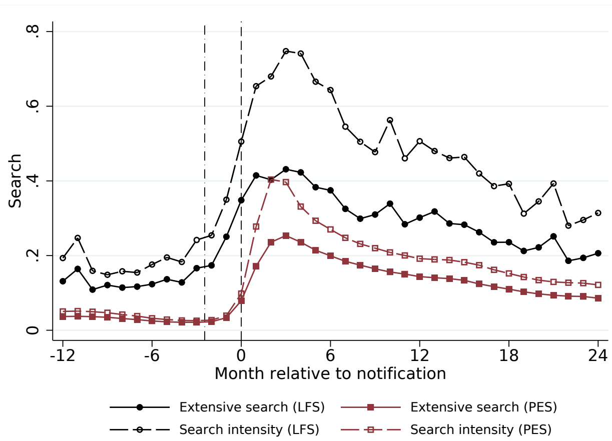
	(1)	(2)	(3)	(4)	(5)	(6)
Above Age-55	-5.076	-4.620	-2.999	-2.986	-3.294	-3.179
	(7.603)	(7.543)	(7.319)	(7.319)	(7.339)	(7.277)
Share WC Coworkers Above Age-55		-0.999	7.911	2.182	6.746	11.038
		(10.089)	(9.792)	(10.415)	(10.029)	(10.430)
Control mean	378.149***	372.852***	208.283**	201.791**	1228.711***	341.795***
	(56.523)	(64.023)	(94.777)	(94.827)	(190.806)	(99.763)
Polynomial order						
1st degree	✓	✓	✓	✓	✓	✓
Interacted w. threshold	✓	✓	✓	✓	✓	✓
Year/month FE	✓	✓	✓	✓	✓	✓
Size of layoff		✓	✓	✓	✓	✓
Baseline covariates	✓	✓	✓	✓	✓	✓
Firm covariates			✓	✓	✓	✓
Age shares controls				✓		
1-letter industry					✓	
2-digit industry						✓
F-statistic	0.446	0.192	0.398	0.103	0.311	0.636
Number of observations	56,625	56,625	56,625	56,625	56,455	56,592

Notes: The table examine whether the earnings a year prior to notification are balanced with respect to the (individual) age-55 indicator and the share coworkers who are above age-55. The sample includes all white-collar workers in notification events where a white-collar worker aged 52-58 was notified. Sample size varies marginally across columns since we do not observe firm covariates for all observations. The regression specifications are such that the effect of the Above 55 indicator is identified within the sample of white-collar workers aged 52-58, while share coworkers above 55 is identified across all white-collar workers. All regressions include a linear age-polynomial interacted with age-bracket FEs (in 6 year bins), individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs), month-by-year FEs. Individual covariates are interacted with a dummy of being close to the threshold (age 52-58). Where indicated we control for firm covariates consisting of average age of workers, average age squared, average earnings of workers, share female workers, share college educated and firm size. Layoff characteristics include size of layoff and flexible controls for average tenure within layoff.. Standard errors are clustered by notification events. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

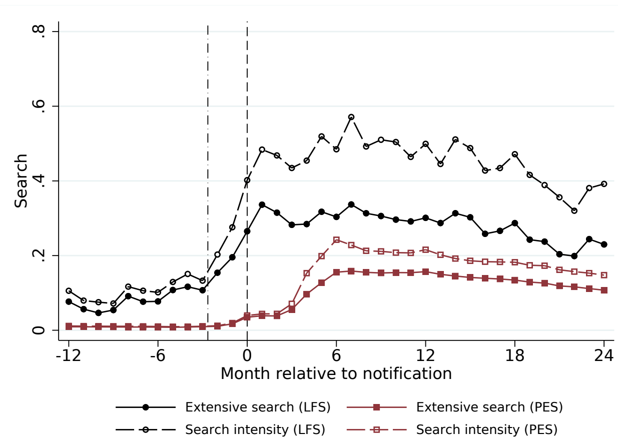
Additional search graphs

Figure A15: Search by month relative to notification

(a) Short notification time

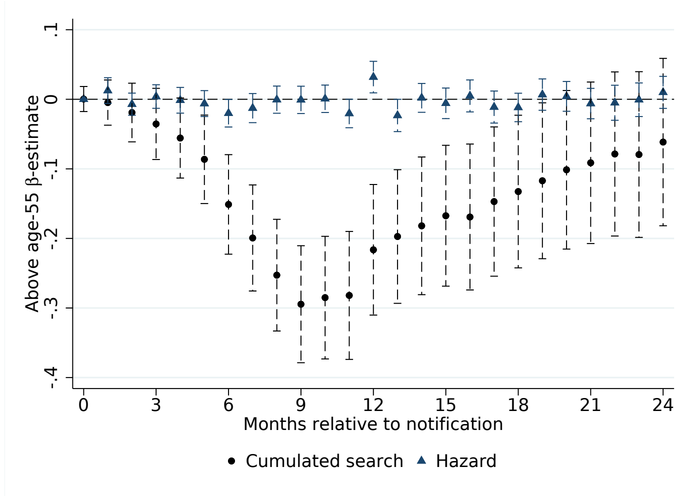


(b) Long notification time



Notes: The figure shows the evolution of job search around notification during 2005-2016 for four measures of search. We do this separately for workers with short and long notification time defined as below or above median (90 days) in panel (a) and (b), respectively. The solid and hollow black series correspond to self-reported search in the Labor Force Survey (LFS). The former shows the probability of search (extensive margin) whereas the latter show the intensity of search, defined as the inverse hyperbolic sine function (arcsinh) of the number of channels used to look for a job. The channels are: visiting the Public Employment Service (PES), using a PES job coach, searching jobs databases, searching via recruitment firms, searching by directly approaching firms, applying to posted ads, reading ads, asking friends for job tips. The red solid and hollow squared series use administrative data from PES. The former shows the probability of being registered at the PES (extensive search) and the latter search intensity defined as the inverse hyperbolic sine function (arcsinh) of the number of interactions with a PES caseworker within a month. The dash-dotted line between t-3 and t-2 indicates when on average the firm reports the notice to the PES, relative to a workers individual notice at t=0.

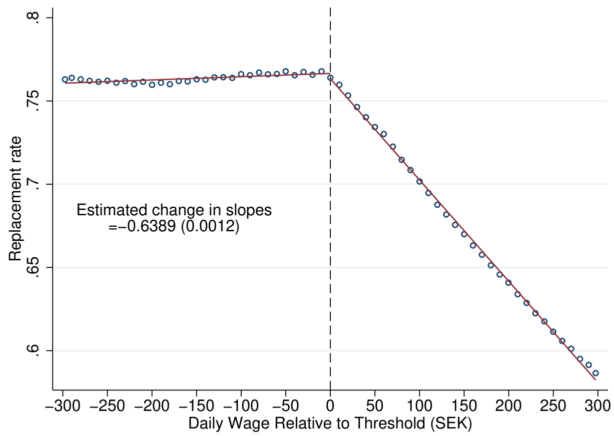
Figure A16: The effect of MN on cumulated search and hazard to new job



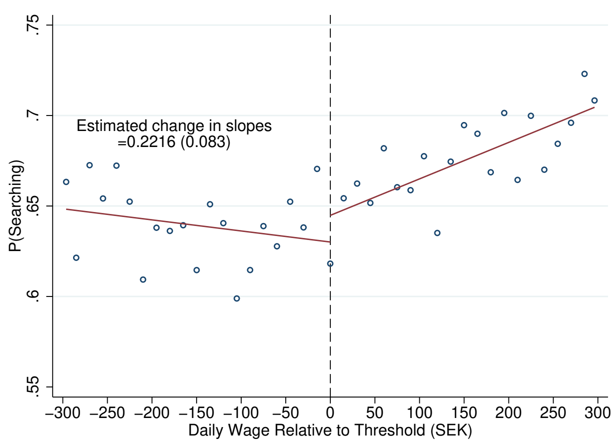
Notes: This figure plots the dynamic effects of being above the age-55 threshold on cumulative search until a new job (circles) and the hazard to a new job (triangles), both relative to month of notification. The sample comprises all white-collar workers in notification events where a white-collar worker aged 52-58 was notified. Search is defined as inverse hyperbolic sine function (arcsinh) of the number of interactions with the PES, cumulated over time until the workers finds a new job. All regressions include (i) a linear age-polynomial interacted with age-bracket (in 6 years width) indicators, individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure, educational attainment FEs), month-by-year FE:s. Individual covariates are interacted with a dummy for being close to the threshold (age 52-58). (ii) Firm covariates: average age of workers, average age squared, average earnings of workers, share female workers, share college educated and firm size. (iii) Layoff characteristics include size of layoff and flexible controls for average tenure within layoff. (iv) 2-digit industry FEs. Dashed lines surrounding the estimates indicate 95% confidence intervals with standard errors clustered by notification events.

Figure A17: Effects of the kink in the UI-schedule on search and the job finding rate for the unemployed

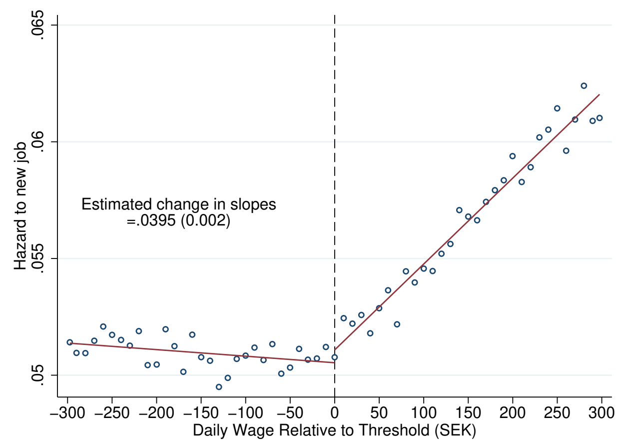
(a) Replacement rate



(b) Search



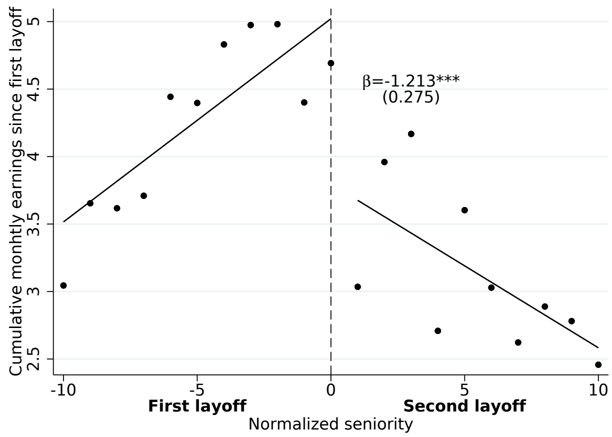
(c) Job finding rate



Notes: The figure shows the workings of our RKD-design to estimate the effect of job search on the hazard among unemployed. Panel (a) makes the research design visible by plotting the replacement rate as a function of the daily wage prior to becoming unemployed. Panel (b) plots the likelihood of searching for a job and panel (c) the likelihood of finding a new job in the subsequent month. The search measure is retrieved from the Labor Force Survey (LFS). Finding a job is defined as no longer being registered with the PES and having positive monthly wage earnings. The plots are shown for unemployed individuals eligible for UI during the years 2005-2015. The search graph is estimated using 32,138 observations while the hazard is estimated based on 9,988,274 observations. Standard errors are clustered at the individual level.

Earnings loss of delay

Figure A18: Cumulative Earnings Loss of Delay



Notes: These results are based on 862 establishments with two consecutive layoffs within 4-6 months (1,724 layoff events, involving 12,646 individuals). The y-axis shows cumulative earnings from the new job during the first year following the initial layoff report (of the first event). Cumulative earnings are normalized by monthly earnings in the pre-notification job. The x-axis orders workers by their seniority within layoff event. The two dots closest to the threshold thus compare the most senior worker in the first layoff (normalized seniority = 0) to the least senior worker in the second layoff (normalized seniority = 1). The regression lines come from estimating an equation containing a linear seniority polynomial interacted with the threshold indicator. The regressions also include individual-level baseline covariates: age, age squared, tenure and gender, all interacted with an indicator for white-collar status. The estimated jumps at the threshold along with standard errors are depicted in each panel. Standard errors are clustered by notification event* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C Measurement Error in the Assignment Variable

C.1 Illustration of measurement error

Summary

Measurement error in the assignment variable is a potential problem for RD settings as it causes some individuals to be placed on the wrong side of the threshold. In our case, the measurement error stems from misreporting of notification dates, which translates into a measurement error in age at notification – our assignment variable. If the measurement error is sufficiently large, a true discontinuity looks like a non-linearity; see Davezies and la Barbanchon (2017) for example.

Measurement error in the assignment variable severely complicates non-parametric RD-analyses. Intuitively, as the measurement error grows, a greater fraction of individuals are misplaced relative to the threshold and the discontinuity at the cutoff increasingly looks like a non-linearity. Optimal bandwidths then fall. Non-parametric RD thus places larger weight on the portion of the data that is most affected by the measurement error. A parametric RD approach, on the other hand, is less susceptible to the severity of the problem, since the bandwidth is fixed a priori.

Figure A19 illustrates this intuition. It is based on a simulation that mimics the data configuration that we observe. The effect of interest is the impact on notification times when an individual surpasses the age threshold. Individuals are uniformly distributed over age at notification. The true effect is 90 days. Along the horizontal axis, we vary the share of the observations which is potentially misplaced. The left-hand panel shows the outcome of the non-parametric RD approach devised by Calonico et al. (2014). Their approach features a local linear regression using data on an optimally chosen bandwidth around the age threshold. When there is no measurement error it recovers the true treatment effect, i.e., 90 days. The estimated impact then falls linearly with the amount of measurement error in the data; when 50% of the data is affected by the measurement error, the estimated impact is reduced to 60 days for example.

The right-hand panel shows that our baseline parametric approach is essentially immune to the measurement error problem. This approach features a linear interacted control function on data within $\pm$ 3 years of the age threshold. By estimating the linear control function, it puts equal weight on all data points belonging to the analysis window. Consequently, the fact that individuals closest to the threshold are misplaced has little impact for the estimate of interest.

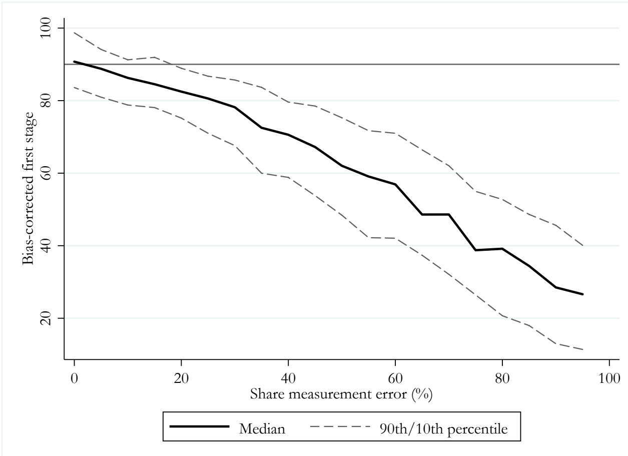
The details: measurement error in our setting

Our data contain information on all firms intending to lay off at least five workers simultaneously. A firm must submit a list containing the identities of laid-off workers and the displacement dates to the Public Employment Service (PES). The list also contains information on individual notification dates (which in 84% of the cases is the same as the arrival date of the list to the PES). Our running variable is age at individual notification date.

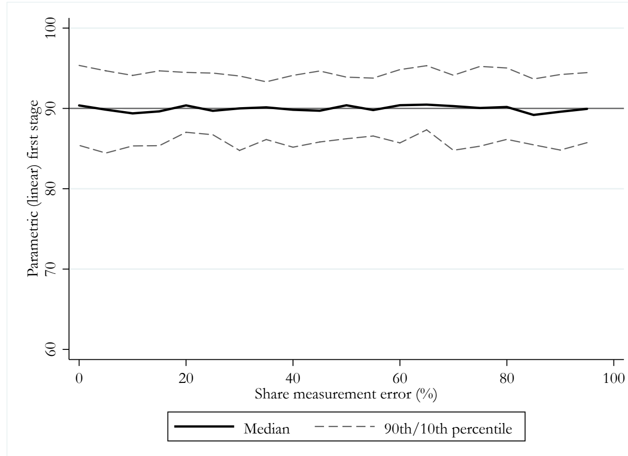
A concern is that individual notification date may contain some measurement error. Such errors imply that age at notification is measured with error. Measurement error in the assignment variable is potentially destructive for RD-analysis, since individuals may be placed on the wrong side of the

Figure A19: RD Estimates using different approaches

(a) Non-parametric (local linear) RD



(b) Parametric (linear) RD



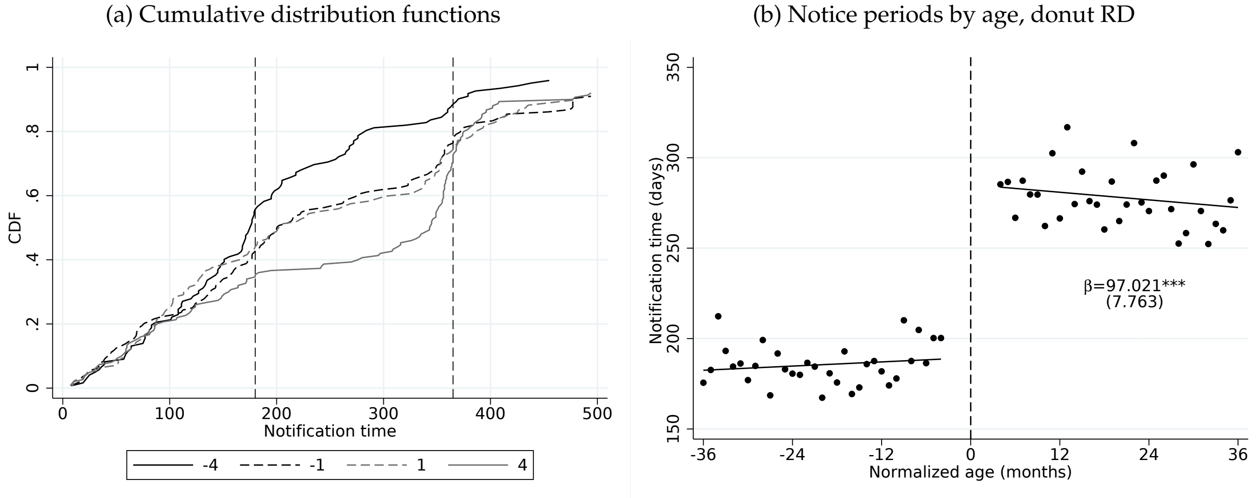
Notes: Panel (a) shows the results from the non-parametric (local linear) RD approach following Calonico et al. (2014). At each level of measurement error, the local linear regressions using a triangular Kernel are fit to data determined by an optimally chosen bandwidth. Each point in the graph has a different bandwidth as illustrated by panel (b) of Figure A22. Panel (b) mimics our baseline approach where we use data on ± 3 years around the threshold. In the analysis, we include a linear control function which is allowed to have different slopes above/below the age threshold. Both panels are based on simulated data where individuals are uniformly distributed on age a notification. Conditional on the total amount of measurement error, reported on the x-axis, the age of individuals is reported ± 2 months erroneously with 50% probability; ± 3 months erroneously with 30% probability; and ±4 months erroneously with 20% probability. The simulation is based on 100,000 observations, the true age distribution is uniform and the true discontinuity is 90 days.

threshold. As illustrated by Davezies and la Barbanchon (2017), for example, a true discontinuity looks like a non-linearity when the measurement error is sufficiently large.[69]

Figure A20a illustrates the presence of measurement error and its likely form. The left-hand panel shows the cumulative distributions functions (CDF:s) of the notice period in the vicinity of the age-55 threshold. The lines labeled -4 and +4, for example, display the CDF:s for individuals who are notified 4 months before and after the age-55 threshold, respectively. These two cumulative distributions have mass points roughly where one would expect them to be, i.e., at 180 and 360 days, consistent with workers above age 55 receiving an additional 180 days of notice if they have more than 10 years of tenure. Workers with less than 10 years of tenure, receive less than 180 days, no matter their age, which explains why 35% of workers above age 55 have less than 180 days of notice; moreover, workers may bargain for additional notice, which explains why some 40% of workers below age 55 have more than 180 days of notice.

[69]It is possible that measurement error in notification dates translates to measurement error in the notification period (depending in part on whether the measurement error translates into displacement dates or not). This is not a concern as it will only reduce the precision of the first-stage estimates.

Figure A20: The nature of the measurement error



Notes: Left-hand panel: Cumulative distributions of notification periods in the data for individuals whose recorded age at notice is 4 months below (-4), 1 month below (-1), 1 month above (1), and 4 months above (4) the age 55 threshold, respectively. Dashed vertical lines at 180 and 360 days. Right-hand panel: Notice time by age (1-month bins). Data within ± 3 months of the discontinuity are excluded.

Matters are different for the data just above (+1 month) and below (-1 month) the threshold. These two distributions are indistinguishable from one another, suggesting a good deal of measurement error in the notification date; it would take a measurement error of at least 2-months to move an individual from above to below the threshold, or vice versa. The CDF:s for those reported to be 2 and 3 months above (below) the threshold lie in between CDF:s at 1 and 4 (-1 and -4), suggesting some measurement error at these horizons as well.

Figure A20b pursues the same theme by showing notice times by normalized age, excluding data +/-3 months relative to the threshold. There is a trend in notice periods below the threshold, which largely has to do with age being positively correlated with tenure. But importantly, there is no trend in mean notice periods above the threshold. This is as it should be, because (i) tenure does not matter above the threshold, and (ii) there is no reason to expect spillover from workers below to workers above the threshold. The fact that there is some curvature in Figure 1a above the threshold is entirely due to measurement error.[70] The "donut RD" shown in Figure A20b suggests that notification jumps by 90 days – from 191 days to 284 days – at the threshold.

The presence of measurement errors poses problems for all non-parametric RD approaches. Since they are designed to pick up non-linearities in the conditional mean function at the threshold, the local linear regression will focus on a smaller and smaller portion of the data when the measurement error increases.

Simulation To illustrate this problem, we have simulated data containing measurement error. We generate a data set containing 100,000 observations, where individuals are uniformly distributed on (true) age at notification. Individuals just below the threshold are assumed to have 180 days of

[70]For workers below the threshold, on the other hand, spillover could cause a non-linearity.

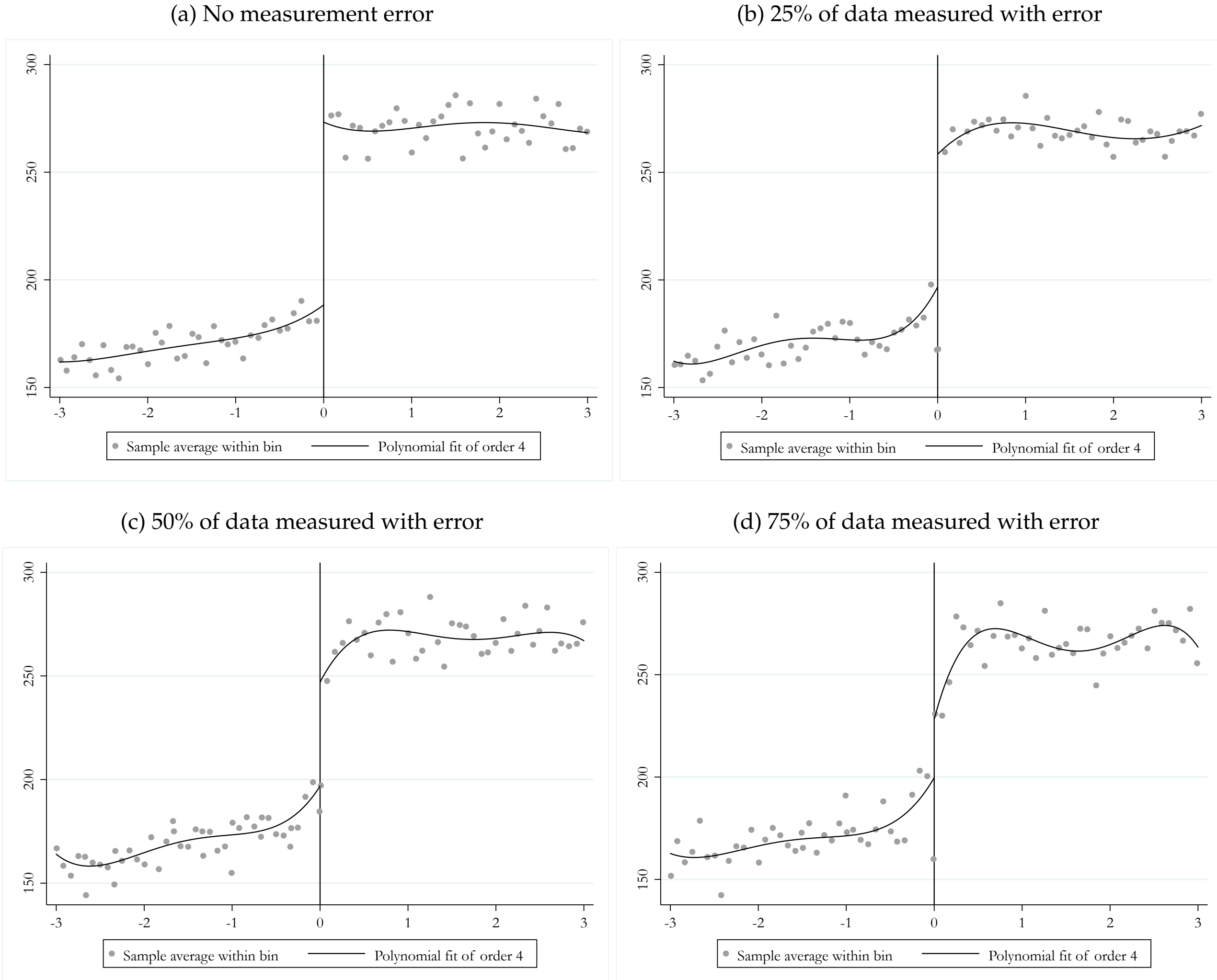
advance notice. Among individuals below the age threshold, average days of notice are assumed to increase linearly with age as in Figure A20b. Individuals just above the threshold have an additional 180 days of notice with 50% probability. This yields an increase in advance notice of 90 days as in Figure A20b.

We follow Battistin et al. (2009) and assume that notification dates are measured with error for a subset of workers. Conditional on the overall amount of measurement error, the error is ± 2 months with 50% probability, ± 3 months with 30% probability, ± 4 months with 20% probability.[71]

Figure A21 illustrates the consequences of different amounts of measurement error. In panel (a) there is no measurement error. We fit a 4th order polynomial control function separately on each side of the threshold. The estimated jump at the threshold is 90 days. In panel (b), the measurement error affects 25% of the observations; the estimated jump at the threshold equals 70 days. In panels (c) and (d), we increase the proportion of the data containing a measurement error to 50 and 75%, respectively. The figure shows that the estimated conditional mean functions becomes more non-linear around the threshold when the amount of measurement error increases. Consequently, the estimated "treatment effects" decrease in magnitude with the amount of measurement error. In panel (a) the estimate is 90 days (as it should be); in panel (c), for example, the estimate drops to 50 days.

[71] Since we observe birth month rather than birth dates we exclude individuals exactly at the threshold. To be moved across the threshold, the measurement error must thus be at least two months.

Figure A21: Simulated data, by amount of measurement error



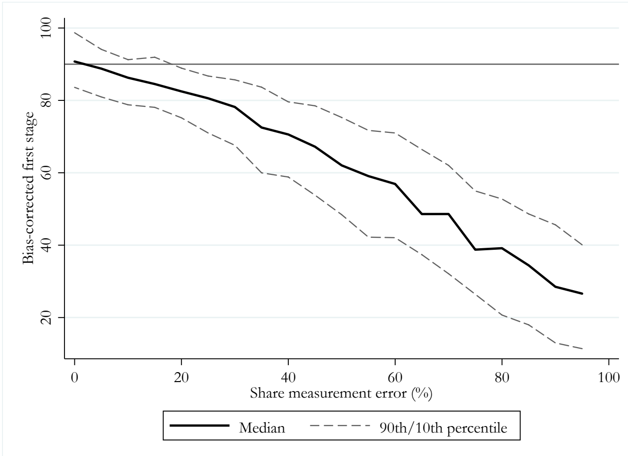
Notes: The scatter plots show notice time by age in 1-month bins. The lines show estimates from a 4th order control function fitted separately to data on each side of the threshold. In panel (a) there is no measurement error; in panel (b) 25% of the data is affected by the measurement error; in panel (c) 50% of data contain a measurement error; in panel (d) 75% of data contain measurement error. Conditional on the overall amount of measurement error (0, 25%, 50% or 75%), the error is ± 2 months with 50% probability, ± 3 months with 30% probability, and ± 4 months with 20% probability.

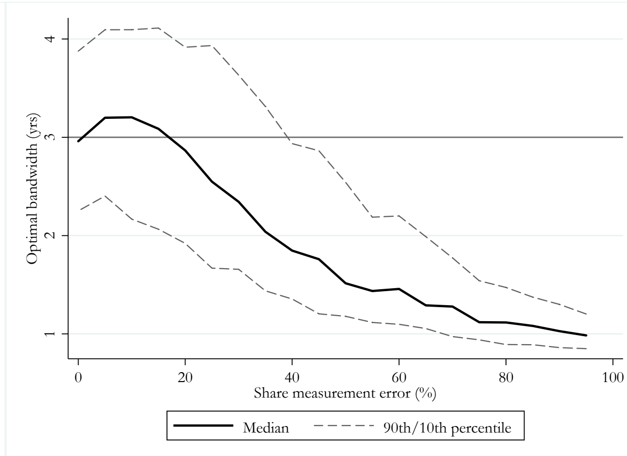
Figure A22 illustrates what different amounts of measurement error does to non-parametric regression discontinuity estimates. We use the Calonico et al. (2014) approach. We thus fit a local linear regression on a portion of the data determined by selecting the optimal bandwidth and weighing the data by the default triangular kernel.[72] The left-hand panel reproduces Figure A19a, while the right-hand panel illustrates the reduction in the bandwidth that comes along with an increase in the share of the data that is measured with error.

In the context of our simulation, a parametric approach which uses all data $\pm$ 3 years of the threshold is more or less immune to the measurement error. Since it places equal weight on all

[72]When we use a uniform Kernel, the problem caused by the measurement error is not as severe as in Figure A22. Yet, the downward bias is still substantial. The first-stage estimate is 70 when 50% of the data is plagued by measurement error, for example.

Figure A22: Simulated data with measurement error: Non-parametric RD estimates





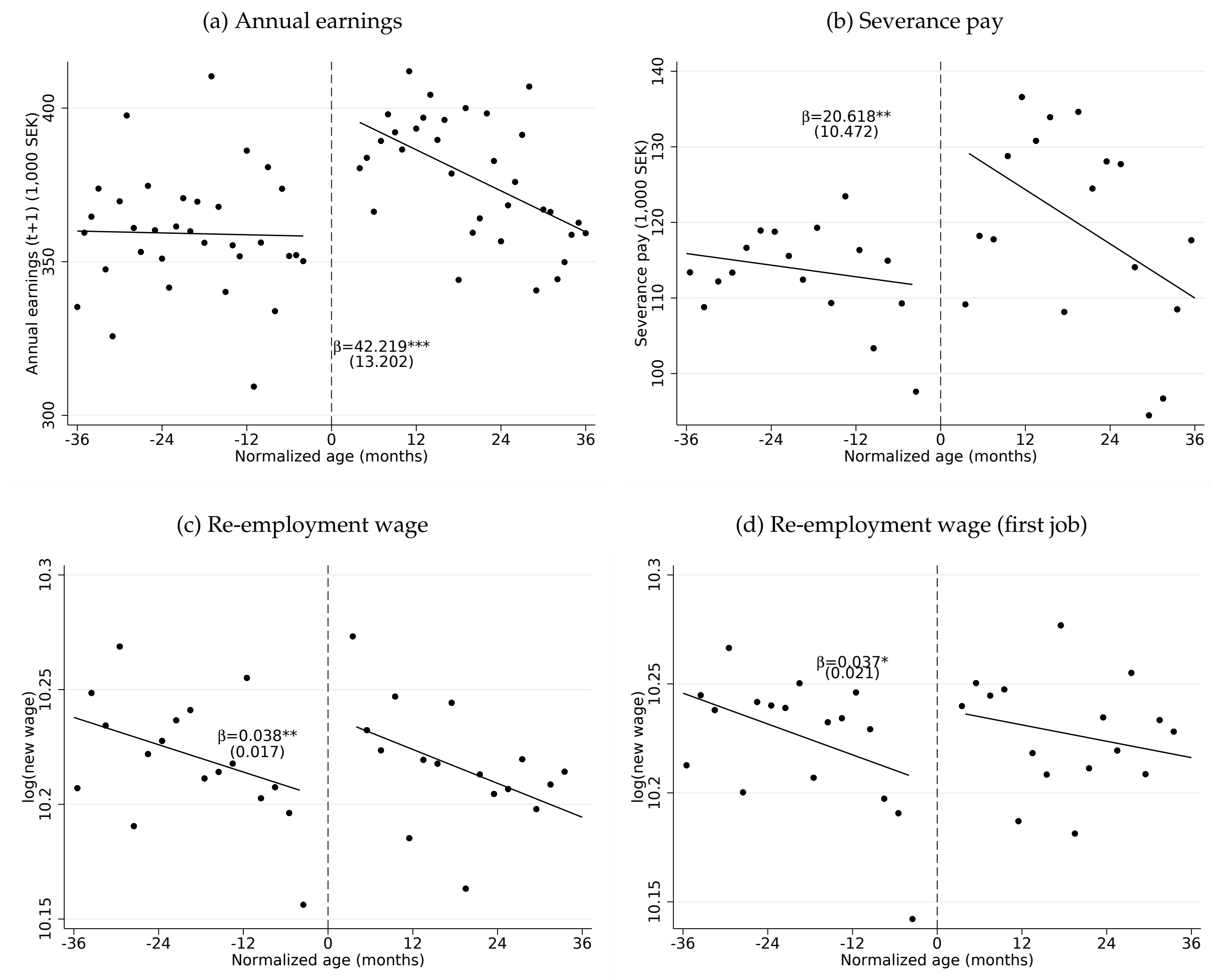
Notes: Panel (a) show the results from the non-parametric (local linear) RD approach due to Calonico et al. (2014). This approach features a local linear regression fit to a portion of the data determined by an optimally chosen bandwidth using a triangular Kernel. Each point in the graph has a different bandwidth as illustrated by panel (b). Both panels are based on simulated data where individuals are uniformly distributed on age at notification. Conditional on the total amount of measurement error, the individual is reported: $\pm$ 2 months erroneously with 50% probability; $\pm$ 3 months erroneously with 30% probability; and $\pm$ 4 months erroneously with 20% probability. The simulation is based on 100,000 observations, and the true treatment effect is assumed to be 90 days. In each panel, the solid line depicts the median across 100 simulations, while the dashed lines correspond to the 90th/10th percentile.

observations used in estimation, it is less sensitive to the measurement error in notification date; see A19b.

C.2 Additional donut RD-graphs

Figure A23 and A24 show RD-graphs for various outcomes where we exclude data +/-3 months relative to the age-55 threshold.

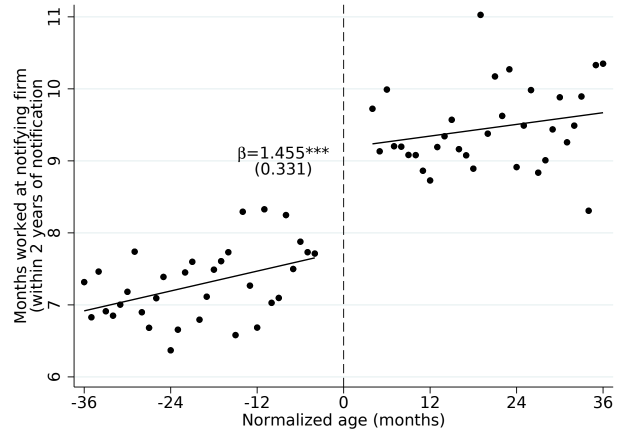
Figure A23: Donut RD-graphs for earnings, severance and wages



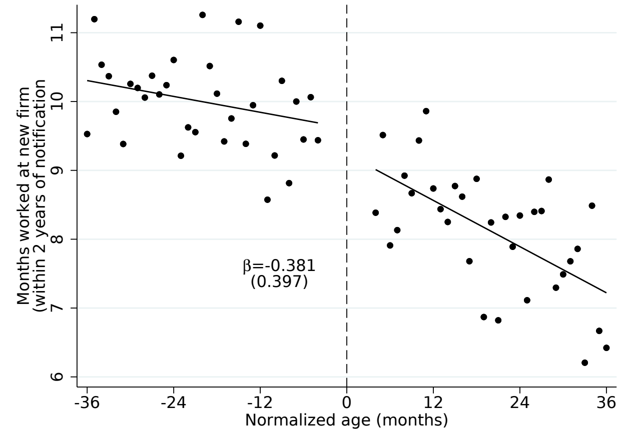
Notes: The figure plots (a) annual earnings, (b) severance, (c) and (d) re-employment wages, all by age at notification relative to the 55-threshold in 1-month bins where we have excluded observations +/- 3 months away from the threshold. Severance pay is measured as excess earnings in the year of displacement (see Section 3.1). Re-employment wage refers to full-time equivalent monthly wage reported in the Wage Survey by an employer other than the notifying firm for whom the worker have have not worked for at least one year prior to notification. In panel c) we let the Wage Survey determine the first new job whereas in panel d) the first new job is determined by the monthly employment markers. The regression lines come from estimating equation (6) with a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification. The regressions also include individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure at notification, educational attainment FEs), and month-by-year FE:s. The estimated jump at the threshold along with its standard error is depicted in the figures. Standard errors are clustered by notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A24: Donut RD-graphs for employment outcomes

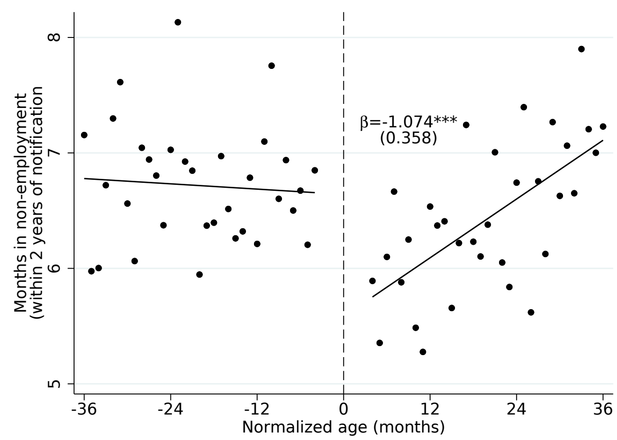
(a) Months at notifying firm



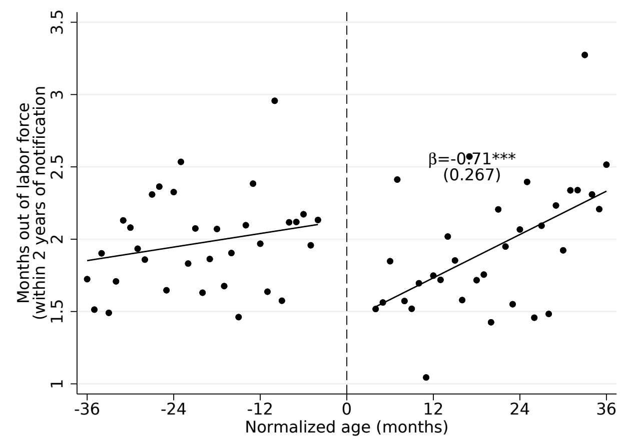
(b) Months at new firm



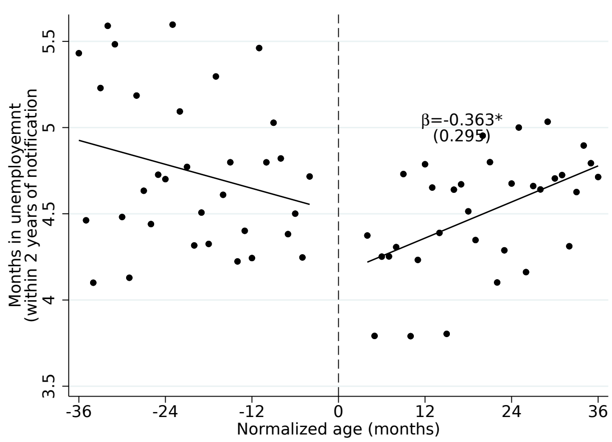
(c) Months in non-employment



(d) Months out of labor force



(e) Months in unemployment



Notes: The figure shows employment outcomes cumulated over 2 years post notification by age at notification relative to the 55-threshold in 1-month bins, where we have excluded observations +/- 3 months away from the threshold. The regression lines come from estimating equation (6) with a linear age polynomial interacted with the threshold indicator for individuals aged 52-58 at the time of notification. The regressions also include individual-level baseline covariates (earnings in the year prior to notification, gender, immigrant status, tenure at notification, educational attainment FEs), and month-by-year FE:s. The estimated jump at the threshold along with its standard error is depicted in the figures. Standard errors are clustered by notification event. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D Firm-level Analysis

D.1 Construction of the notification event panel

This appendix describes the construction of the data for the firm analysis. We start with the notification data. These data cover all notified workers during 2005-2018 and their employer-IDs. In constructing the analysis data set, we face two challenges. First, there is likely measurement error in the timing of the notification event. Second, we want to relate the productivity change to a particular layoff event. To achieve the second objective, we focus on firms that had no layoff events in the two years preceding a particular event.

To deal with measurement errors, we clean the data as follows. We first compute the firm-level share of workers with erroneously reported notification times. A displaced individual's notification time is erroneously reported if it is negative or zero (7% of all notified workers) or larger than 18 months (0.9% of notified workers). We drop firms from the notification data if the share of displaced workers with erroneous notice times exceeds 10%. The reason for dropping the firm, rather than just the particular event, is that we want to be sure that included firms does not have prior layoff during the two years prior to layoff. Dropping firms with erroneous layoff events, eliminates 25% of the notification events.

Next, we match the employer-employee data with the cleaned notification data. In doing so, we define an indicator for whether a notified worker is recorded at the displacing firm in the year and month of the notification event. We compute the firm-level share of such workers and retain firms with a share above 95%. The reason for this is that we want to make sure that the notified workers were employed at the firm at the time of the event. This restriction drops 40 percent of the cleaned notification data.

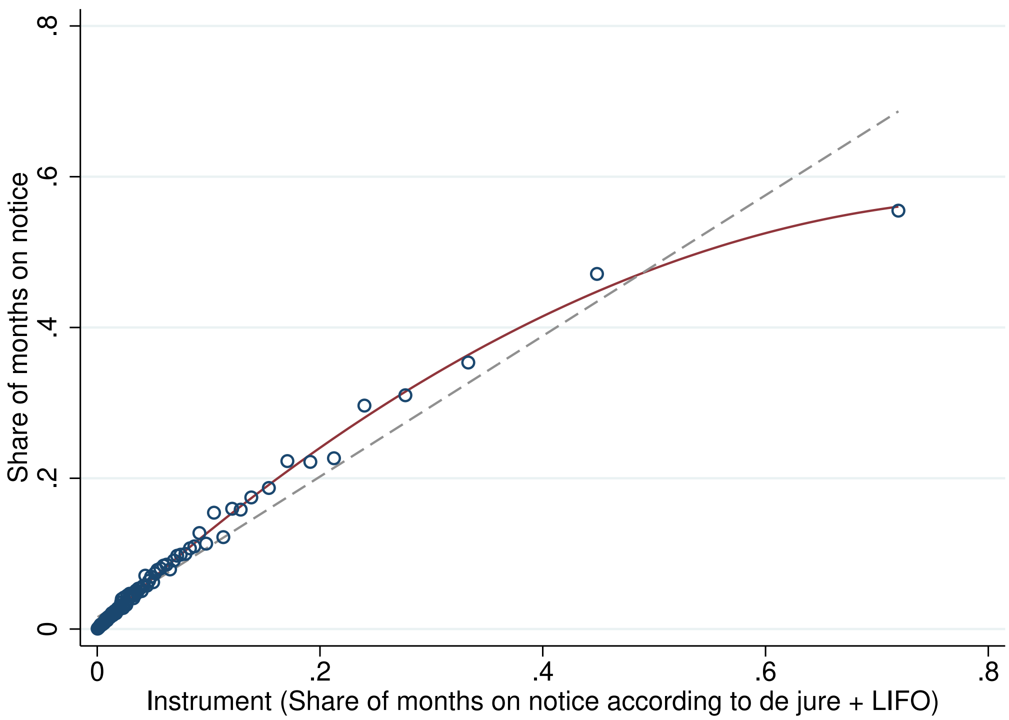
The implementation of the LIFO-rules depends on plant and year, plus whether the worker belongs to a blue-collar or a white-collar wage agreement. The combination of these three factors (plant/year/type-of-agreement) defines a so-called order-circuit. Within each circuit, we take the number of displaced workers as given, and rank workers by their tenure. We then compute the *de-jure* notification time for individuals with a tenure rank lower than or equal to the number of displaced workers within that circuit. If the de-jure notification time is longer than the remaining calendar months of the year, we assign the remaining notification time to the next calendar year, irrespective of whether and when the worker leaves.

We then create a panel for firms that notify workers by adding firm-level outcomes. We use both the SCB database FEK and the database Serrano (maintained by the Swedish House of Finance). Sales or revenue is our key variable. We focus on incorporated firms. Since balance sheet data are noisy, we make sure that these two sources of information are consistent with one another. We define an observation as inconsistent if the difference in sales between the two sources exceeds 10%. We remove firms, where more than 10% of the observations over time is labeled as inconsistent. This drops 32 percent of the remaining notification data. The outcome variable of interest, the differences in log revenue over two time-periods minus the corresponding difference in the log of employees, is winsorized at the 5th and 95th percentile, within displacement year.

D.2 Precision of the first-stage

Figure A25 illustrates the first stage relationship between χ – the share of months worked by individuals on notice – and the instrument – the share of months predicted by the *de-jure* rules. The circles illustrate the relationship between these two variables over the percentiles of the instrument. The dashed line represents the linear relationship, while the solid line shows the fitted values when we fit a second-order polynomial to the data. As illustrated by Figure A25, the second-order polynomial fits the data much better. Indeed the first-stage F-value increase from 79.4, in the linear specification, to 222 in the quadratic specification. This also matters for the 2SLS estimates of α, which becomes unreasonably large with a linear first stage. Adding additional higher-order terms neither improves the first-stage nor changes the 2SLS estimate of α.

Figure A25: The first-stage relationship



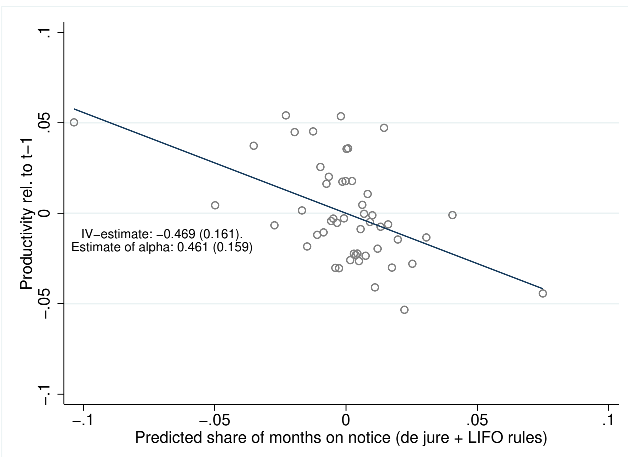
Notes: The figure plots the firm-level relationship between χ – the actual share of working time provided by workers on notice – and the instrument. The latter is the share of working time provided by workers under notice in case the firm had followed the last-in-first-out seniority rules in the selection of who to lay off and the law as well as the CBAs for how long advance notice these workers would have received. Each circle represents a percentile of the distribution of the instrument. The location in x- and y-space is determined by the mean $\chi$and the mean instrument within each percentile. The dashed, straight line corresponds to the line of best fit while the solid, curved line represents the quadratic best fit.

D.3 Graphical illustration of the IV estimates

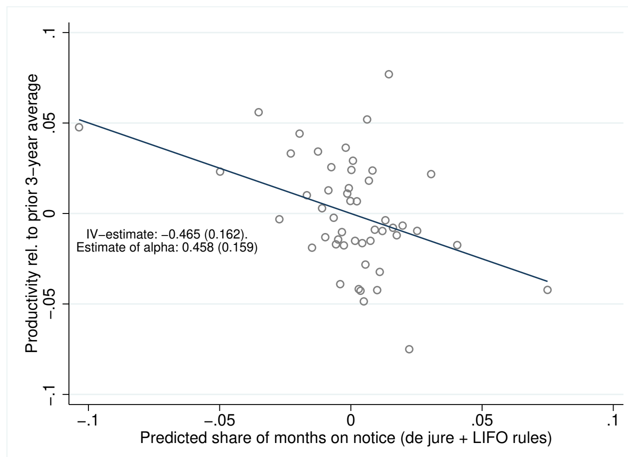
Figure A26 illustrates the IV estimates graphically.

Figure A26: IV estimates

(a) Change in productivity



(b) Productivity relative to prior 3-year average



Notes: This figure plots the relationship between the outcome and the fitted values from the first-stage regression. In panel (a) the outcome is change in productivity, defined as $\log(y_{it}) - \log(y_{it-1})$ whereas panel (b) focuses on $\log(y_{it}) - (\log(y_{it-1}) + \log(y_{it-2}) + \log(y_{it-3}))/3$. The fitted values are estimated as explained in Section 5.1 according to equation 9. We divide firms in 50 equal-sized bins according to the fitted values and show the average outcome against the average fitted value within each bin, including control variables as explained in the text. The line represents the coefficient on the predicted share of months on notice. We report the slope of the line along with standard errors in parenthesis that are clustered at the firm-level. We also report the corresponding estimate of α as well as its standard error.