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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 avoid unemployment and find better jobs by encouraging workers to search for a new job while still employed, thereby increasing 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 current production losses, longer advance notice improves production efficiency. If not, Coasian bargaining predicts that firms offer a larger severance instead of longer notice. 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 notice and larger severance, resulting in less exposure to non-employment spells and higher-paying jobs. These favorable labor market outcomes are solely due to longer notice; in contrast, larger severance delays job finding and has no impact on wages. We also show that advance notice replaces job search while unemployed with more effective search while employed. On the production side, we document a productivity drop among notified workers and estimate a production loss due to delayed separations. Using our estimates of production gains and losses to evaluate the overall production efficiency, we conclude that the gains of MN seem to outweigh the losses.

JEL Codes: J31; J33; J63; J68.

Keywords: Advance notice, Job search, Layoff, Job quality

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I. Introduction

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 most attention in this context is unemployment insurance (UI), which supports the unemployed while they search 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 thus remain unanswered. Does MN assist displaced workers in finding jobs? If so, how does the policy achieve that? Which behaviors of workers and firms 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 mandated information sharing by compensating the worker instead of providing Advance Notice (AN). 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' decisions to comply with the rule and provide AN or 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. Fourth, we offer evidence on the production loss of the policy. 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 option to inform workers in advance. From an efficiency perspective, this option incorporates a trade-off.

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

Notified workers search for a new job and supply less effort in the current job. 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. In the absence of a mandate, firms never notify workers, 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 with severance.[2] 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. 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, delayed separation is 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 of our empirical analysis 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 the 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.[3] Longer MN yields smoother transitions across jobs for displaced workers: they are less exposed to 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: less non-employment exposure, 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

[2]Use 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.

[3]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.

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. Our two-instrumental variables (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.

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 research (Nekoei and Weber, 2017). Based on these findings, we hypothesize that job search is more effective while employed than while unemployed.

The third part of the empirical analysis tests this hypothesis. Our 2-IV design reveals that one additional month of AN – compared to the counterfactual of one month of UI – reduces search activity and raises re-employment wages but does not delay job finding. These findings imply that employed job search is more effective than search when unemployed: search while employed allows the worker to target high-quality jobs without a significant decline in job finding. We reach these conclusions also when comparing how effective job search is by the same individual when employed versus unemployed, and when exploiting two sources of exogenous variation in search incentives (one for employed and one for unemployed). All three research designs align closely with one another: the ratio of the return to job search in employment to unemployment ranges from 1.12 to 1.25, and the confidence intervals exclude 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 mandated advance notice duration (MN eligibility) of their laid off workers. We instrument advance notice duration with the duration that would have occurred if the firm had adhered to the MN rules and the last-in-first-out (LIFO) rule, which prescribes that workers should be notified in an inverse order of seniority.[4] We estimate that productivity falls by

[4]Identifying the costs of MN requires stronger assumptions than for the benefits, since we cannot employ an age-

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 and find that separations are indeed inefficiently delayed. To quantify the cost of delay, we estimate foregone earnings using variation from the LIFO rule and 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 production gains and losses of MN. The production gains – equivalent to 1.2 months of earnings – comprise 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. The net impact of the MN extension on average production is positive and amounts to about 30% of monthly income. A back-of-the-envelope calculation suggests that a reform that applies the MN extension to all separations would increase aggregate labor income by 0.25%.[5]

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). Our theory also allows firms to circumvent MN using side-payments akin 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 emphasize the importance of side-payments for enhancing efficiency and to document empirically 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

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.

[5]This assumes that the estimated production gain – 0.29 of monthly income – can be generalized to all separations. Under this assumption we obtain $0.29 \times 0.87\% = 0.25\%$, where 0.87% is the monthly separation rate for Sweden estimated by Hobijn and Sahin (2009).

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 across the employed and the unemployed.

The paper unfolds as follows. Section II provides the conceptual framework that guides our empirical analysis. Section III describes the institutional setting and the data. Section IV provides our empirical analysis of the benefits of MN. We also separate the causal effects of advance notice and severance in this section. Section V offers estimates of the relative effectiveness of searching from employment compared with unemployment. Section VI contains our empirical analysis of the costs of MN. Section VII connects the empirical results to the theory and calculates the net impact on production of MN. Section VIII concludes.

II. Conceptual framework

We examine 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 that results 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 that illustrates the essence of the idea. It also highlights 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 these falls, 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 $y_2' < 0$.[6] 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.

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

II.A. 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 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 by the notified worker, combined with the lower incentive to exert effort (shirking), reduces first-period productivity in the notifying firm by a factor of α. There is heterogeneity in α across firms. Before the start of the second period, the laid-off worker searches for a new job with the job-finding rate of $\lambda^u = \phi^u(w)$.[7]

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

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

[7] 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).

η captures the relative effectiveness of employed and unemployed search. There is heterogeneity in η across individuals.[8]

As our focus is on production efficiency, we take workers to be risk neutral. Another assumption is that private contracts do not include AN.[9] This assumption is analogous to 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 and examine whether MN improves outcomes in the second-best.[10] We show empirically that MN increases the notice period, which supports the assumption that private contracts are not optimal.

II.B. MN effect on AN and severance

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. As such, separation is efficient. But no firm provides notice, and this decision may be inefficient. 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.

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

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

[9]Our findings remain unchanged when the contract space includes severance. In addition, since all agents in the economy are risk-neutral, we ignore UI.

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

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.[11] 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.

II.C. 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.[12] 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 effect The 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 component is due to the relative effectiveness of employed job search, the other is due to notified workers having an extended

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

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

search period:

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

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.

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

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

The employment effect is ambiguous in sign, even if $\eta > 0$, because advance notice encourages workers to target higher quality jobs, thus reducing the job-finding rate.

Earnings effect 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

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

where $w^D = \mathbb{E}(w|\Omega^D)$ is the average wage in firms that delay notice in response to the mandate. In the Appendix, we show how the effect of MN on earnings can be decomposed into four intuitive components that can be estimated separately, that is

$$\Delta Y / w^N \simeq \underbrace{-\Delta NE}_{\text{Non-employment dur. effect}} \quad \underbrace{-\left(\frac{w^N - w^u}{w^N}\right)\Delta L}_{\text{New job dur. effect}} + \underbrace{\lambda^n \frac{\Delta w}{w^N} P^N}_{\text{Wage effect}} + \underbrace{\frac{\sigma}{w^N} P^S}_{\text{Severance effect}} \tag{4}$$

where NE denotes non-employment duration and L the duration on the new job. The new job duration effect arises because the new job yields a lower wage due to the displacement effect.

Effects on aggregate production and social welfare In general equilibrium, aggregate market production equals labor income, while Utilitarian social welfare is the sum of aggregate (market) production and home production.[13] To examine the MN effect on welfare, we extend our model in the Appendix 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 on production and welfare and, thus, the optimal duration of the mandate and production efficiency.

A marginal MN extension has two effects on welfare. 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:

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

where $\widetilde{w}$ denotes expected post-displacement production (including home production) for marginal workers and $\partial\Omega^D$ the boundary of delay. The first expression on the right-hand side is unambiguously positive since notice is given when $\sigma \geq \alpha y$. In other words, firms provide notice if and only

[13] Recall that we assume risk-neutral agents, and abstract from disutility of search and work.

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 the remainder of the paper, we add 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 IV.A). Section IV.A also provides estimates of the wage and employment impacts of MN (equations 1 and 2). Section V offers direct evidence on the relative effectiveness of searching from employment compared to unemployment. In Section IV.A, 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 VII) uses equation (5) as a basis for evaluating the production effects of MN; we compare an estimate of the expected production gain from giving advance notice with an estimate of the productivity drop associated with having workers on notice, as well as the production loss of delay. In our empirical exercise, we proxy $\widetilde{w}$ with expected earnings; we thus ignore home production and focus on the effects of MN on production efficiency.

III. Institutional setting and data

III.A. Institutional setting

The Swedish Employment Protection Law (Lag 1982:80 om anställningsskydd, 8§) 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 negotiated in collective bargaining agreements. For instance, many white-collar agreements within the private sector stipulate that workers above age 55 with 10 years of tenure get an additional 6 months of notice.[14] Neither the law nor these agreements include mandatory severance. That said, notified workers may agree to compensation packages involving advance notice and severance if they are perceived as more generous than the default rules. The typical configuration is that the union negotiates with the employer on behalf

[14] We have hand-collected information from all major collective agreements in the Swedish labor market for the relevant time period (2005-2016). Other age rules do exist but they are much less prevalent.

of its workers to arrive at packages that may be tailored specifically to each employee. 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.

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. 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. 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). The firm first reports the number of workers that it intends to lay off along with the reason for downsizing. Upon this initial layoff report, the firm starts to negotiate with the labor unions over who to lay off, respecting the LIFO rule that requires workers with shorter tenure to be laid off first conditional on occupation type (Cederlof, 2024). The endpoint of the negotiations is the compilation of a list of workers who would be notified with their individual-specific layoff 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.

III.B. 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.[15] We assume that the notification date is also the date when the worker learns about her future displacement. Our evidence on worker search behavior (see Section V, Figure V) is consistent with this assumption.

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 unem-

[15]For 84% of workers, the individual notification date corresponds to the arrival of the list at the PES.

ployment spells and information on contacts with the PES caseworkers; (v) the Income Statement and Balance Sheet Register contains information on firms' revenue, value added, and profits; and (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 I presents descriptive statistics for notified workers overall (Column 1); notified workers in the baseline estimation sample (Column 2), and for two comparison groups (all workers in Column 3 and all workers reweighting the industry shares to match notified workers in Column 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 across the two columns since the data contain notifications involving at least five workers. Second, educational attainment is lower among the notified reflecting the known fact that lower educated workers have a higher layoff risk. The individuals in our estimation sample have higher wages, earnings, and educational attainment than the average notified individual (c.f. Columns 1 and 2).

IV. Benefits of mandatory notice

IV.A. Estimating the effect of MN using a RD design

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

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

where $\mathbf{1}(\cdot)$ denotes the indicator function, A_i 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, Cattaneo and Titiunik (2014) (see Appendix II.B). Since age is discretely measured in months we rely on a parametric control function.[16] Our default specification has a linear control function interacted with the threshold. In addition, $\mathbf{X_i}$ 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 II.A shows both that the distribution of notified individuals is smooth and that pre-determined covariates are balanced around the threshold.

IV.A.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 Ia 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 indicates that private contracts do not incorporate efficient notice, as in the model of Section II. According to the model, the observed increase in AN implies that MN has enhanced production efficiency.

Controlling for baseline covariates and displacement event fixed effects changes the point estimates only marginally (Appendix Table 6), suggesting that baseline characteristics are similar on

[16] 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.

either side of the threshold both within and across notifications (Appendix Table 1). 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.[17] Appendix C also shows that the parametric 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 II suggests that MN will be substituted by severance pay when losses to firms from 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.

We validate the imputed severance by comparing it to survey data where firms report total severance payments made to their employees. Imputed and reported severance are strongly and positively correlated (see Appendix Figure 10). When regressing imputed severance on reported severance we obtain a coefficient of 1.07 (standard error 0.012). Imputed severance slightly overestimates reported severance, 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 discrepancy is not an issue since the additional payments are balanced around the threshold.

Figure Ib shows that severance increases by 17 kSEK at the age-55 threshold, correspond-

[17] Appendix C further analyzes the consequences of measurement error. It shows, inter alia, that measurement error in notification dates makes a discontinuity look like a non-linearity (as observed in Figure Ia). 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 includes 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 most affected by measurement error.

ing to 53% of a monthly wage.[18] Agents thus make private agreements to partially undo public regulation, providing empirical support to the theoretical conjecture of Lazear (1990).

IV.A.2. MN effect on employment status

Figure II examines how MN affects the transition process for workers, in general, and whether longer MN shields workers from non-employment, in particular. Panel (a) illustrates 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 still work at the notifying firm within two years after notification.

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 MN increases the likelihood of remaining at the notifying firm in the first year after notice. The effects are strongest after six months, which corresponds to the baseline AN period to the left of the age-55 cutoff (Figure Ia). Conversely, 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 also that MN has no impact on the probability of working at a new firm at any horizon after notice (see Appendix Figure 3b).

Table II 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 Ia or Appendix Table 6, Column 2), suggesting that they leave the firm 1.3 months prior to their layoff date. As suggested by panel (b) of Figure II, 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 result stands in sharp contrast with the UI literature, where more generous UI delays job finding (Krueger and Meyer, 2002). Table II 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

[18] All amounts have been deflated to 2010 values. In February 2024, the conversion rates are 10.29 SEK/Dollar and 11.17 SEK/Euro. Appendix Figure 13 shows the result of a permutation test where we vary the age threshold. The one-sided p-value associated with the actual threshold at age 55 is 0.014.

force (Columns 4 and 5).[19]

IV.A.3. MN effect on job quality: wages and beyond

The theoretical framework outlined in Section II 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, Chetty and Weber, 2007). The two-year time frame avoids selection because the employment effects of MN have subsided after two years (see Appendix Figure 3).

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

For our subsequent analysis it is vital to time job transitions precisely. In the analysis below, we estimate wage effects separately 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 job finding rate and search responses, which requires 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 falls by 30%. Despite the reduction 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 III). 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%. Therefore, the effect of being eligible for longer MN is 3.2 percentage points.[20]

[19] Appendix Figure 12 provides graphical illustrations of the RD-estimates in Table II.

[20] The Appendix reports additional evidence regarding the MN wage effect. For example, Appendix Figure 5 (b) shows the result of a permutation test where we vary the age threshold. The one-sided p-value associated with the actual threshold at age 55 is 0.011. Appendix Figures 5 (c)-(d) show that workers eligible for longer notice avoid large

The wage effect of MN can stem from three channels: i) workers targeting higher wages when they search as employed due to longer notice; ii) a higher likelihood of finding a job while employed; or iii) workers eligible for longer MN being more selective when unemployed, since they receive larger severance. We fully investigate these factors in Sections IV.B and V. At this stage, we offer a simple decomposition to quantify the magnitude of each channel.

The MN extension results in a 4.5% increase in wages for those who make an employer-to-employer (EE) transition within six months after receiving notice (Column 4 of Table III). We focus on a six-month period because longer MN does not affect the likelihood of making an EE transition within this period (see Figure IIb). The share of the main wage effect attributable to a higher selectiveness while searching as employed is obtained by multiplying the Column-(4) estimate (4.5%) by the EE transition probability for workers eligible for longer MN ($0.566+0.075=0.641$ according to Column 5):

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

The decomposition thus suggests that the wage effect when making an EE transition contributes to 91% (2.9/3.2) of the overall wage effect.[21] 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 4 by studying the effect of MN on the characteristics of the new firm, 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 find that eligibility for longer MN increases subsequent firm size by 1.7% (Appendix Figure 7), which indicates that part of the wage effect may be due to securing employment in better firms. In the same vein, workers find new employment in firms paying 1.4% higher wages; although economically important, this estimate is not statistically significant.

We also investigate the effect of MN on other characteristics of the new job (see Appendix Table 5). 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,

wage losses and have a higher chance of experiencing a large wage increase. Appendix Figure 6 pursues the same theme by showing the distributional impact, following Nekoei and Weber (2017).

[21] 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.

and increases tenure in the new job. Overall, the non-wage characteristics of the new job are positively 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 IIIb 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 III – 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.[22]

IV.A.4. MN effect on earnings: total impact and decomposition

Figure IVa 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 IVb 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, Lalonde and Sullivan, 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 rise is consistent with the existence of severance, as predicted by theory.

What are the proximate drivers of this overall earnings effect? We decompose earnings accord-

[22]Consistent with this finding, Appendix Table 3 shows lower wage growth for those above age-55 three years post notification.

ing to equation (4) and focus on the first two calendar years after notification since all employment adjustments have subsided by then (see Figure IIb and Appendix Figure 3). The earnings effect of longer MN corresponds to 1.60 months of additional pay. 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%. Appendix Table 7 presents additional detail regarding the decomposition.

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

IV.B. 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 for displaced workers.

Separating the effect of longer AN from larger severance requires two instruments. In addition to the discontinuity used in Section IV.A, 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 14 (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 **??** (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 9 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 IV shows the Instrumental Variables 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 prolonging AN by one month reduces non-employment exposure by 0.6 months over a two-year horizon and increases wages in the first new job by 1.7%. Moreover, an increase in severance of 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 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 wage effect of UI. 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 duration.[23] 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, Chetty and Weber, 2007, Lalive, 2007, Schmieder, von Wachter and Bender, 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.

That severance has no wage impact aligns with our finding that the estimated wage effect is mainly associated with EE transitions (Section IV.A). The absence of a wage effect of severance also aligns with the findings in Card, Chetty and Weber (2007). However, our estimate of the effect of severance on non-employment is substantially larger than those in Card, Chetty and Weber (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, Chetty and Weber (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.

Our wage and non-employment effects of advance notice are new to the literature. They are substantively large: a one-month increase in the advance notice period by one month leads to a reduction in non-employment exposure by 0.6 months and an increase in the re-employment wage

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

by 1.7%, after accounting for the impact of severance pay. Again, our theory sheds additional light on the magnitude of these effects. The causal impacts of AN are identified in the sub-population whose value of conducting on-the-job search is particularly high. 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 job search while employed is more effective than job search while unemployed (see Section II). We now turn to this issue.

V. Search effectiveness by employment status

This section compares the effectiveness of job search in employment versus unemployment. 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 empirical 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 Va 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.[24] All other series in Figure Va show that notification yields additional search activity. However, the impact on the PES

[24]The persistent increase in search intensity is most likely tied to the negative earnings impact of displacement (e.g., Jacobson, Lalonde and Sullivan, 1993). The literature shows that displacement causes a reduction in future employment prospects (e.g., Cederlof, 2024) as well as wages (e.g., Lachowska, Mas and Woodbury, 2020; see also Column 2 of Table III).

measure is only visible after individual notification and not after the initial layoff report, and the magnitude of the response is muted relative to the more comprehensive LFS measure. In part, this might be due to the fact that registering with the PES is a precondition for collecting UI.

Figure Vb compares search intensity for workers with long and short AN periods, below and above the median of 3 months. The average length of AN periods in these groups is 1.6 and 7.0 months, respectively. Search intensity in both groups evolves 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 do workers with long notice search reelatively 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 IV.B to identify the effect of AN separately from severance. Table IV shows that the number of contacts with the PES caseworker is reduced by 8.7% due to longer AN.[25] The estimated reduction in the duration until a new job (-0.205 on a base of 8.28 months) suggests a slight (and non-significant) increase in the job finding rate by 2.5%.[26] The striking fact is that the job finding rate 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 searching from 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 the effectiveness of search intensity. It 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 estimate is downward biased as it fails to account for the increase in target wages

[25] A caveat of the 2-IV design is that it uses the less granular PES measure of search as we lack statistical power using the LFS measure.

[26] Appendix Figure **??** shows direct estimates of the job finding rate. There is little variation in the effect of MN on the job finding over all time horizons post notice. It also shows how MN affects search over time.

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 V). While straightforward, this OLS approach suffers from the problem that search effort is endogenously chosen. Fortunately, the LFS is a rotating panel where each individual is surveyed eight consecutive quarters, so we can thus estimate the relationship between the job-finding rate and search in a sample that is restricted to individuals who experience both 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.[27] The return to search from employment is uniformly higher than the return to search from unemployment (Table V). Our preferred specification, presented in Column (4), holds composition constant and suggests that the return to searching from employment is 25% higher than searching from unemployment.

A concern with the estimates in Table V is that they could be 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. 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 I.

Table VI presents the results.[28] Since the LFS is available for a small sub-sample, we increase the statistical power by implementing a two-sample IV approach (see Inoue and Solon, 2010), where we estimate the reduced-form 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

[27]The addition of these fixed effects does not significantly alter the estimates. The key point is that the sample composition is held constant.

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

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. The estimates in Column (2) imply that the elasticity of search with respect to the UI replacement rate is 0.4.[29] The impact on the hazard is consistent with, e.g., Kolsrud et al. (2018). According to panel (a), the effects of these shifters on the wage in the new job are not significant, supporting the exclusion restriction. 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.

Figure VI 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.

VI. Costs of Mandatory Notice

While the focus of Section IV is on the benefits of MN, this section considers the costs of the mandate. Section VI.A exploits variation in notification times across firms to provide an estimate of the average productivity loss of notice – α in the framework of Section II. In Section VI.B we estimate the production loss associated with the delay of separation due to MN.[30] The analysis of the costs of MN requires stronger assumptions than our previous RD analysis, since we generally

[29]We obtain the elasticity as follows. We divide the change in slopes for search (0.222 from Column (2) of Table VI) with the level at the kink (0.65 as shown in Appendix Figure 17b). 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).

[30]Appendix Section **??**, provides evidence that cash-constrained firms do not pay severance, indicating that some firms provide inefficient advance notice.

cannot rely on the age threshold for identification.

VI.A. MN effect on productivity

The productivity of notified workers may decline as they reduce their work effort and increase their search for alternative employment. The goal of this section is to estimate the magnitude of the productivity loss due to notification, i.e., α as defined in Section II.B. We consider firms that give notice to some employees and we leverage variation across firms in how long notified workers stay at the firm.[31] 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 II, a worker's productivity falls by a factor α once notified. The production at firm i at time t is therefore $Y_{it} = A_{it}\left(1-\alpha\chi_{it}\right)L_{it}$, 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 $\Delta \ln y_i = \Delta \ln A_i + \ln\left(1-\alpha\chi_{it}\right)$, 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.

As a starting point, we use OLS to estimate α. We address 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. We compare 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.

We then use an instrumental variables approach 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 III.A). Second, we use the LIFO rules that determine how redundancies should be made (see Section III.A for details). The instrument – denoted $\widetilde{m}_i$ – takes the number of notified workers in each occupation as given, and calculates

[31] 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 IV.A.

the MN period for workers who would have been notified according to the LIFO rules. This approach 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

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

$$\Delta \ln y_i = \beta \chi_i + d_t + d_{j(i)} + f_y(s_i) + g_y(\widetilde{m}_i) + \eta X_i + \varepsilon_i \tag{10}$$

where i denotes a notification event and $\widetilde{m}_i$ the instrument, δ_t (d_t) and $\delta_{j(i)}$ ($d_{j(i)}$) 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.[32] 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.[33]

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 $\widetilde{m}_i$ because the instrument depends on the workforce composition, which in turn may depend on changes in firm productivity. For instance, firms with sluggish productivity may have lower hiring rates, leading to a workforce skewed towards high-tenure and older workers (and thus higher $\widetilde{m}_i$). Additionally, the workforce composition may directly impact productivity, beyond the effects of average age and tenure, which are accounted for in the regression.[34]

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

[33] As shown by Table VII, 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 25.

[34] Another reason for controlling for $\widetilde{m}_i$ is that firms may postpone layoffs in response to MN. When the firm keeps the worker longer than is optimal – because of its inability to predict the productivity decline or because MN creates incentives to delay layoffs (see Section VI.B) – a larger portion of the work spell has lower productivity. This, however,

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). The fact that the pre-notification evolution of the marginal revenue product is unrelated to subsequent average notice duration across firms supports this assumption (see the rows pertaining to outcomes $t-1$ and $t-2$ in Table VII).

The first rows of Table VII contain 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.[35]

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 VII is 0.37, which suggests that a worker's productivity falls by around one third during the notification period.

VI.B. The production loss due to delay in separations

Section II 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. We now address 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 em-

is not due to the behavior of workers.

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

phasized in Section II, 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 VI.B. 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, with a linear productivity decline, delay is proportional to α.[36] This result is derived in a setting 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 workers before those with short MN. Empirically, we find a negligible adjustment of the timing of notification in response to extended MN (see Table **??**), suggesting that we can approximate the duration of delay using the increase in the duration at the notifying firm (1.32 according to Table II).

Next, we examine the cost of delay. Intuitively, this cost is approximated by the worker's expected earnings 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. Three months is a natural lower limit since workers notified within this period are considered part of the same layoff event (see Section III). We then sort workers by seniority within layoff event.[37] 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. This measure overestimates the production loss of delay to the extent that the productivity in the notifying firm ($(1-z)(1-\alpha)y$) exceeds the value of home production (b).

[36]In general, an increase in α makes the timing of layoff more responsive to MN than the timing of notification. We also show that there is no delay in the extreme case where $\alpha=0$.

[37]This analysis includes all workers—both blue-collar and white-collar, with no age restrictions—in contrast to our RD approach.

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 (Appendix Figure 18). 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 ($0.29 \times 1.32 = 0.38$) months of earnings.

VII. Production efficiency

This section investigates the impact of MN on production efficiency. We calculate the net effect of MN on total production by inserting estimates from Sections IV and VI into equation (5). The estimates from equation (8) yield total production gains of crossing the age-55 threshold of $1.60 \times (1 - 0.274) = 1.16$ of the average monthly wage per notified worker (the standard error is 0.26); w deduct the severance effect of MN – 27.4% of the total earnings effect – since severance is a transfer from the firm to the worker.

The production losses due to MN have two components. We first combine the estimated production loss factor, α, from Section VI.A (we use $\alpha = 0.37$, i.e., the average of the estimates in Table VII) with the additional number of months a notified worker stays in the notifying firm (1.32 according to Table II) 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 (standard error 0.33). The second component of the production losses come from delay. According to Section VI.B, the production loss due to delay is 0.38 of a monthly wage. Combining the various pieces together in 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, with a standard error of 0.35.

(11)

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

To better understand the economic significance of the magnitude of the production gain, we conduct two back-of-the envelope calculations. First, we consider a reform that applies the MN extension to all separations, while assuming that the treatment effect can be generalized to the overall population. Such a reform would increase aggregate labor income by $0.29 \times 0.87\% = 0.25\%$.

Here, 0.87% corresponds to the monthly separation rate for Sweden, as estimated by Hobijn and Sahin (2009). Second, we compare the production gain with the duration of the MN extension (six months). This suggests that 5 percent of the MN extension is converted into increased production per eligible laid-off worker. Given that not all workers above 55 are eligible for the MN extension, this is a lower-bound estimate.

There are two concerns about the external validity of the estimated net impact on production. 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 II (-0.472), we find a fiscal externality corresponding to 24% of the average monthly wage.[38] 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.

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

These calculations are indicative but should be taken with a 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.

[38] A 50% replacement rate aligns with reality, given the low earnings cap in the public UI system during the period and the high-income group considered.

VIII. 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. We analyze 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. Inducing firms to provide advance notice (AN) encourages workers 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 firmsparticularly when they are already facing difficulties. 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 fronts. 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 can 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 to which targeting a better job is easier for an employed worker. We hope that further research will shed light on this key issue.

At several stages of our analysis we have pointed to the similarities and differences between MN and 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 I
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 II
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 12 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 III
EFFECT OF MN ON WAGES AND EE-TRANSITIONS

	Re-employment wages				Pr(EE)
	$\ln(w)$	$\ln(w)$	$\Delta \ln(w)$	$\Delta \ln(w)$ $EE = 1, t \leq 6$	
	(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 IV
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 IV.B. 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 variable in Column (4) is the duration of the period between notification and the next job, in Column (5) is the duration of the period between end of notifying job and the next job (zero if EE), and in Column (6) is the difference in the log wage between the first new job and the notifying job (c.f. Column (2) in Table III). 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 IV.A.3); the additional drop in Column (6) is due to sampling in the Wage Survey (Section III.B). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE V
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 ×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 VI
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	

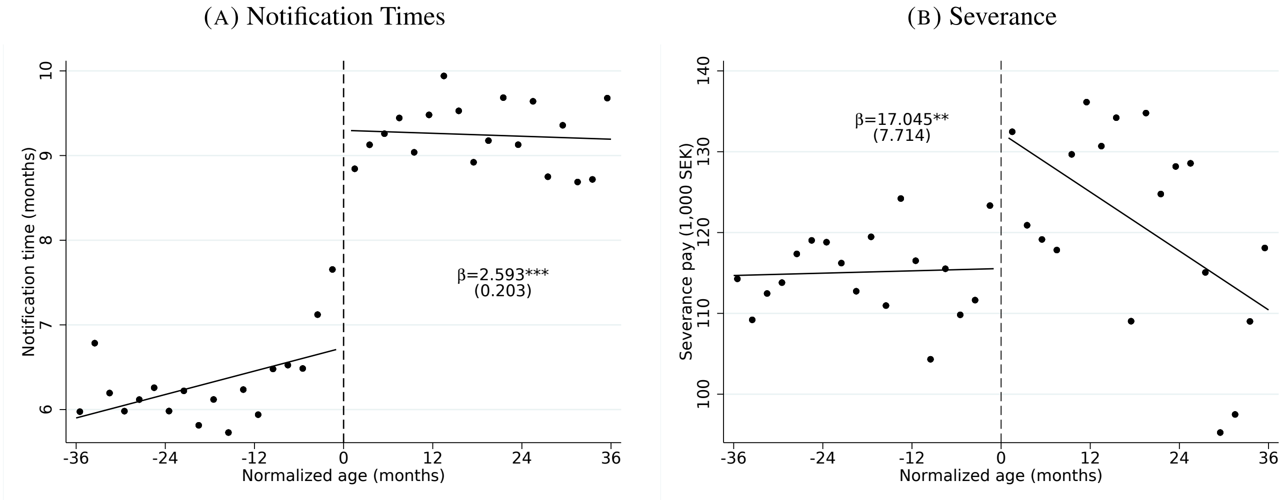
Notes: Each coefficient derives from separate regressions where the outcomes are reported in each row. Panel (a) shows the reduced-form effects exploiting exogenous variation in search for employed and unemployed, respectively. The independent variable in Column (1) is de jure notice time (in months) 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 VII
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 VI.A. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

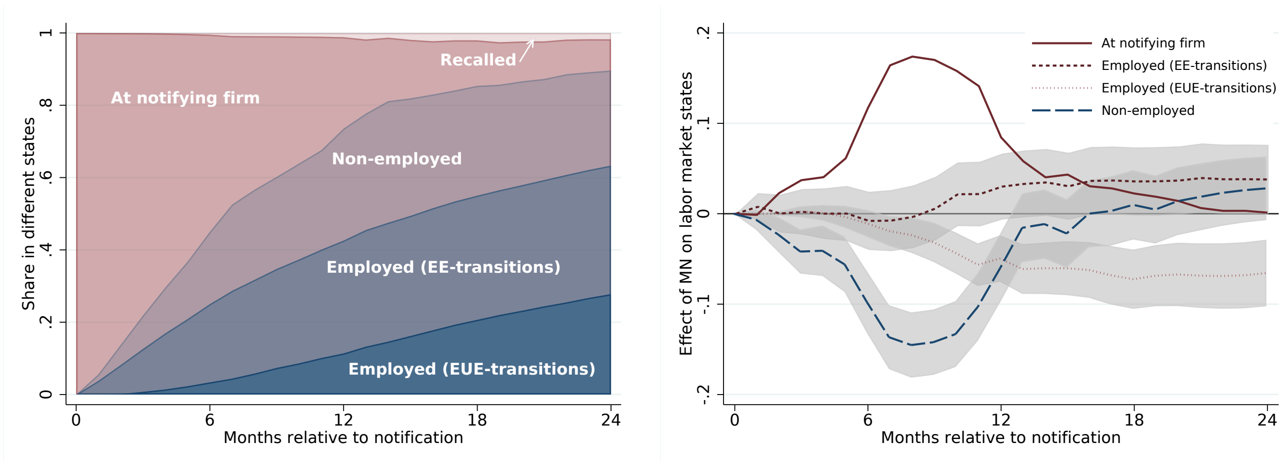
FIGURE I
The Effect of Mandated Notice (MN) on Actual Notification Times and Severance Pay



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 IV.A). 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 II
Dynamic Employment Transitions

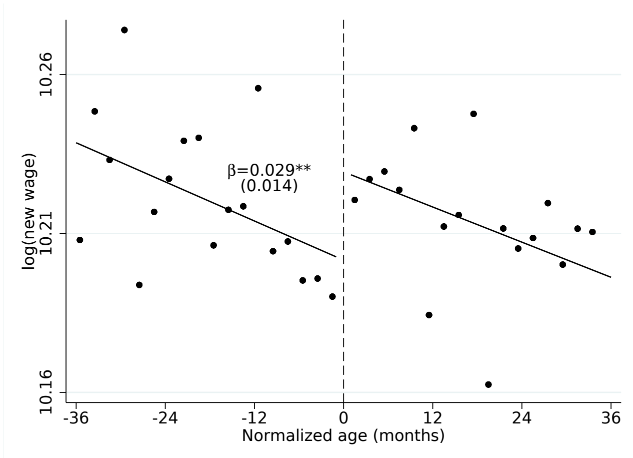
(A) Employment Transitions Over Time (B) Effect of MN on Employment Transitions



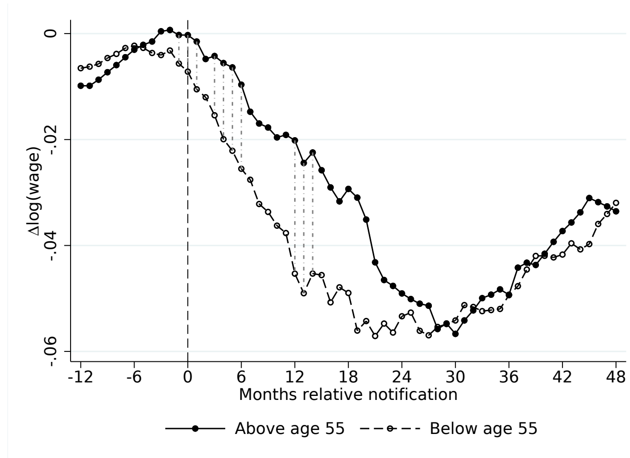
Notes: The figure presents employment outcomes over time relative to notification for individuals aged 52-58 at the time of notification. Panel (a) shows the employment dynamics after notification where we treat new employment as an absorbing state. Notified workers who are employed are split into those 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 III
Effect of MN on Wages

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



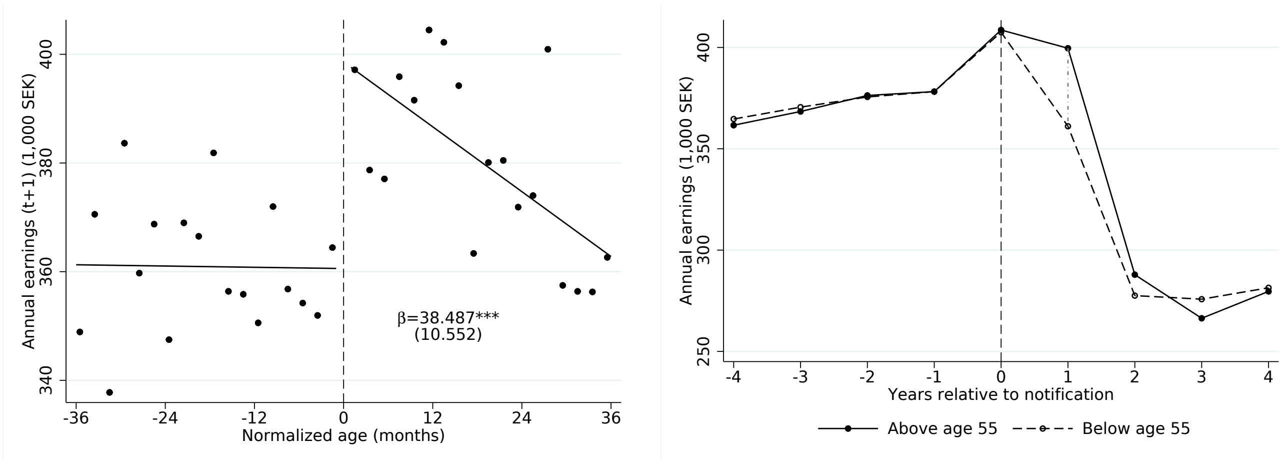
(B) Dynamic Effect of MN on Wages



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 IV
Effect of MN on Annual Earnings

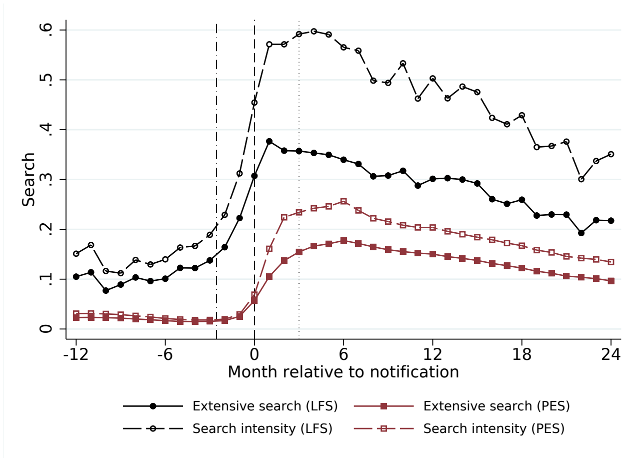
(A) Effect of MN in the Year after Notification (B) Dynamic Effects of MN



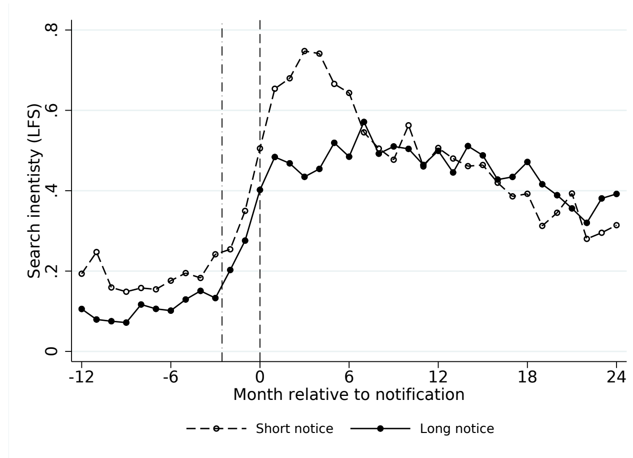
Notes: Panel (a) plots annual earnings in 1,000 SEK in the calendar year after notification against age at notification relative to the 55-threshold. The estimated jump at the threshold and its standard error are displayed in the figure. In panel (b) we plot the RD-estimates of earnings effects over 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 indicates 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 V
Search by Month Relative to Notification

(A) Search in the LFS and PES

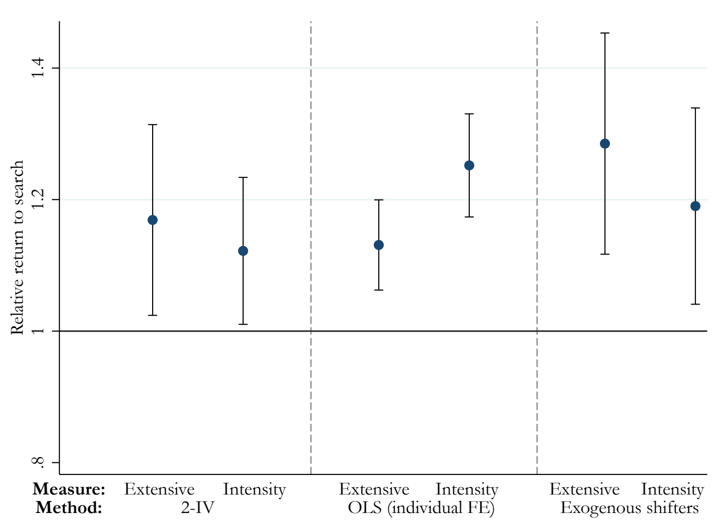


(B) Search intensity by notification time



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 shows 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 worker's 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 VI
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 IV 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 V. The final two dots use the exogenous shifters as in Table VI Column (3). Each dot is surrounded by 95% confidence intervals.