Employee Debt Overhang and Firm Performance

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Abstract

Enforcement of household debt supports credit supply, but may also weaken borrowers' work incentives and affect the performance of the firms that employ them. We study employee debt overhang under wage garnishment in Sweden, where earnings above a protected subsistence threshold are remitted to creditors. Exploiting quasi-random variation in enforcement timing, we find that workers bunch earnings at the protected threshold: monthly earnings fall by 5%, unemployment and sickness absence rise, and promotions decline by 27%. Earnings reductions are larger among workers with high debt relative to income, consistent with weaker incentives over long repayment horizons. Coworkers partly offset the withdrawal of labor by working longer hours, but experience increased sickness absence and hospital contacts. Exposed firms nevertheless experience persistent declines in value added and sales, as well as higher exit rates, with effects most visible among small firms and firms with weak balance sheets before exposure. Earnings rise following a 2022 reform that increased protected income thresholds while leaving garnishment status and employer notification unchanged, supporting an incentive channel. These findings identify workplace costs of debt enforcement that extend beyond the debtor; such consequences of debt enforcement should be considered alongside its benefits for credit markets.

JEL classifications: D14, J01, J13, J62

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1 Introduction

Debt enforcement institutions support credit markets by making repayment promises credible and sustaining lenders' willingness to supply credit (Djankov et al., 2007; Fedaseyeu, 2020). Yet enforcing repayment can also change borrowers' incentives. When repayment is tied to earnings, enforcement alters the return to work, with potential consequences for labor supply and production. These consequences are less well understood, particularly when they extend beyond the debtor to coworkers and employers. In this paper, we study how wage garnishment, a widely used debt-enforcement institution, affects workers' labor supply and career progression, their coworkers, and the performance of the firms that employ them.

Our setting is wage garnishment in Sweden, where earnings above a legally protected subsistence threshold are withheld by the employer and remitted to creditors. Garnishment is common across developed economies: for example, about 1% of U.S. private-sector workers are garnished in a typical month (DeFusco et al., 2024), and 2% of the Swedish workforce in a given year. In both countries, roughly 5% of firms employ at least one garnished worker.[1] Enforcement through wages thus connects household indebtedness directly to a substantial number of employment relationships.

Wage garnishment creates a form of employee debt overhang: workers bear the cost of additional effort, while earnings above the protected threshold are transferred to existing creditors. The intuition parallels corporate debt overhang, where outstanding debt discourages investment because creditors capture much of its return (Myers, 1977). Here, the investments at stake include additional hours, discretionary effort, and the pursuit of promotions and better-paid jobs. Under the Swedish system, additional net earnings above the protected threshold are remitted to creditors through the employer. In this sense, garnishment resembles a 100% marginal tax on those earnings.[2] However, there are countervailing forces. The loss of disposable income may increase desired labor supply through an income effect, while additional repayments reduce outstanding debt and can bring forward the end of garnishment. Sweden's system operates under full recourse for most debtors: debts remain collectible absent formal debt relief, which is rare. A worker with modest debt may

[1] DeFusco et al. (2024) find that garnishment absorbs about 11% of gross earnings, using payroll data covering roughly 20% of U.S. private-sector employees. In Sweden, around 100,000 individuals are garnished per year; this estimate is based on our calculations.

[2] The tax analogy is imperfect: although additional net earnings above the protected threshold do not increase current disposable income, they reduce outstanding debt and can bring forward the end of garnishment.

therefore maintain or increase labor supply to repay quickly, whereas a heavily indebted worker faces a repayment horizon so distant that the discounted benefit of additional effort is small. The net response depends on the balance of these forces. Appendix B provides a simple static illustration of the immediate incentive distortion created by the protected earnings threshold.

These individual responses can affect coworkers and firms because employees often produce jointly. Models of team production emphasize that workers may have weak incentives to exert effort when they receive little of the return their effort generates for the team (Holmstrom, 1982). Garnishment can further weaken these incentives by reducing the worker's current reward from additional effort. When tasks are complementary, one worker's reduced effort or attendance can also impair coworkers' productivity, magnifying the effect on firm output (Kremer, 1993). At the same time, coworkers can cushion the disruption caused by a colleague's reduced effort or attendance. Where workers can substitute for one another, they can take over responsibilities or increase their hours, replacing part of the affected worker's contribution. Such adjustment may limit the loss of firm output while imposing additional effort and health costs on colleagues. The consequences for firms therefore depend both on how workers' contributions interact and on the scope and cost of reallocating work. Whether coworkers absorb the shortfall, and whether that adjustment is sufficient to maintain firm performance, are questions that we address in this paper.

We test how garnishment affects workers, coworkers, and firms using Swedish administrative data. We combine the universe of wage garnishment decisions from the Swedish Enforcement Authority (Kronofogden) over 2004–2022 with matched employer–employee registers containing wages, employment spells, sickness absence, and firm financials.

Measuring the causal effect of garnishment requires more than a simple comparison of garnished and non-garnished workers, because people who fall behind on their debts differ from others in ways that also affect their earnings, health, and employment, independent of garnishment itself. We therefore restrict attention to individuals who are eventually garnished and exploit variation in when garnishment begins. After the Enforcement Authority decides to collect a claim, the start of garnishment depends largely on factors outside the debtor's control, such as caseworker processing times, the Authority's workload, and how quickly individual creditors pursue their claims. The delay between the debt-collection decision and the first garnishment therefore varies widely, including among debtors with only a single decision, for whom that decision is necessarily the claim being en-

forced. We implement a staggered difference-in-differences design (Callaway and Sant'Anna, 2021) that compares newly garnished individuals with individuals who are garnished later, but not yet at the same point in calendar time, and apply the corresponding design to coworkers and firms. The main threat to this design is a common shock that both triggers garnishment and worsens outcomes, such as financial distress at the employer or in the household. Three pieces of evidence argue against this. The timing of garnishment is unrelated to the financial health of the debtor's employer. Coworkers in the same firms respond in the opposite direction at exactly the onset date. And our results are essentially unchanged when we exclude workers who divorce or experience a health shock.

The effects we estimate with this design capture the overall impact of garnishment, which likely combines two channels. Workers may supply less labor because additional earnings are seized, and employers may respond to learning about the worker's financial distress: the employer is notified of the garnishment and administers the withholding. An employer may thus revise its assessment of the worker's reliability or suitability for advancement and respond by assigning fewer hours and responsibilities or withholding promotions. Both distorted worker incentives and this potential stigma channel can produce lower earnings and stalled careers. To distinguish the two channels, we examine a June 2022 reform that raised the statutory subsistence allowance for families with children older than seven years. The size of the increase depended on children's predetermined ages, generating variation in protected thresholds while garnishment status and employer notification remained unchanged. The reform allows us to test whether earnings respond to repayment rules without a new disclosure of financial distress to the employer.

We document three sets of results. First, wage garnishment triggers a sharp and persistent decline in workers' labor-market outcomes. Using monthly wage data, we find that garnished workers bunch earnings at the protected threshold, and monthly gross earnings fall by roughly SEK 1,250, about 5% of pre-treatment pay. The earnings decline begins at the onset of enforcement, following stable relative earnings trends. Transitions into unemployment also rise. Among workers who remain employed, contract hours fall by 1.2%, sickness absence rises by 13%, and the probability of promotion drops by 27% relative to baseline. Hospital contacts and psychotropic prescriptions, however, decline by 7.7% and 12.3%, respectively.[3] These patterns make deterioration in measured

[3] Hospital contacts include in- and outpatient care. Psychotropic medications comprise anxiolytics (ATC N05B),

health an unlikely sole explanation for the additional absence and are consistent with weaker work attachment. A predictable repayment process may also resolve some of the uncertainty associated with unresolved delinquency, explaining the improvements in mental health after the start of garnishment. Garnishment also triggers avoidance on other margins: capital income rises, more workers who leave wage employment report business income, and spouses reduce their own earnings, since the protected amount is computed jointly for the household. These responses are too small to account for the earnings decline, but they add to the costs of enforcement. Consistent with the idea that the effective repayment horizon matters for worker incentives, workers with low debt-to-income ratios, who can more realistically repay their debt, show smaller earnings declines than heavily indebted workers.

Second, we find that the consequences of garnishment extend to coworkers. In firms with up to ten employees, coworkers who are not themselves garnished increase their hours and experience higher promotion rates. They also exhibit rising sickness absence and hospital contacts, together with an increase in psychotropic prescriptions. The opposing movements in hours and promotions between garnished workers and their colleagues are consistent with a reallocation of work and responsibilities. Coworkers appear to absorb part of the withdrawn labor supply, while their health outcomes suggest that this adjustment is costly.

Finally, we observe that firms employing a newly garnished worker experience persistent declines in value added and net sales, and their probability of exit rises. Among firms that survive, value added and net sales are about 11% lower three years after exposure. Value added per employee falls almost as much as value added itself, so the losses reflect lower productivity rather than a smaller workforce. The effects are most visible in small, labor-intensive firms, where one garnished worker constitutes a large share of productive capacity.[4] They also depend on the firm's capacity to absorb the shock: firms with weak balance sheets before exposure, measured by leverage, profitability, or interest coverage, experience declines roughly twice as large as financially strong firms, and the declines are larger when the garnished worker holds a higher-ranked rather than a blue-collar position. The firm losses occur despite coworkers' increased hours, indicating that work-

hypnotics and sedatives (N05C), and antidepressants (N06A); the outcome indicates whether the worker fills at least one prescription in any of these classes during the year.

[4] The modal exposed firm has fewer than five employees, and among exposed firms with one to three employees, garnished workers average 58% of the workforce.

place adjustment does not fully offset the disruption. The combined evidence identifies a channel through which enforcement against individual borrowers affects the performance and survival of their employers.

The 2022 reform provides further evidence on the role of work incentives. In a sample of workers already under garnishment, the reform raises protected allowances without changing employers' information about the workers. Those whose allowances increased earn roughly SEK 400 more per month relative to the comparison group after the reform; the increase is about SEK 570 among workers initially above the protected threshold. These estimates correspond to approximately 1.5% and 2.2% of the treated group's pre-reform earnings. The earnings increase therefore identifies an incentive effect of the higher allowance, holding the existing stigma associated with garnishment fixed. Employer stigma, of course, may nevertheless contribute to the response when garnishment first begins.

Our findings contribute to the literature at the intersection of labor and finance; for recent reviews, see Matsa (2018) and Nishesh et al. (2024). Existing work shows that credit-market information can affect employment outcomes (Bos et al., 2018; Dobbie et al., 2020), that financial strain can impair cognitive function and worker productivity (Mani et al., 2013; Kaur et al., 2025), and that housing-wealth losses can reduce inventor productivity (Bernstein et al., 2021). Financial distress can also spill over from affected individuals to others, including from teachers to students (Maturana and Nickerson, 2020) and across inventor teams, where collaborative relationships both transmit wealth shocks and partially insure team members against them (Baghai et al., 2026). We show how financial distress affecting an individual employee propagates through the workplace. Wage garnishment reduces the worker's labor supply and career progression; coworkers respond by increasing hours and taking on greater workloads, with associated increases in sickness absence; and exposed firms experience declines in performance and survival. These findings highlight the workplace as a channel through which household financial distress can affect parties outside the household and ultimately firm outcomes. They also show that coworkers provide an important but incomplete adjustment margin: colleagues absorb part of the withdrawn labor supply, but this response is costly and does not fully offset the disruption to production.

The paper also contributes to research on debt enforcement and household debt overhang. Closest to our setting, DeFusco et al. (2024) use U.S. payroll data to document the prevalence

and incidence of wage garnishment, showing that it absorbs a meaningful share of earnings and predicts job separation. We provide causal evidence on how wage garnishment affects workers' labor supply and career progression and show that its consequences extend to coworkers and employers. Our setting also complements theoretical work on household debt overhang and labor supply. Donaldson et al. (2019) show how limited-liability debt can make indebted households require higher compensation to work, reducing firms' incentives to create jobs and raising unemployment in equilibrium. Wage garnishment instead operates in a full-recourse setting in which workers retain an incentive to repay because additional repayments bring forward the end of garnishment, while earnings above the protected threshold generate little current disposable income. The resulting distortion depends on the worker's effective repayment horizon, consistent with our finding that earnings responses are strongest among workers with high debt relative to income. The effects on career progression also relate to work on household debt overhang and human capital investment (Manso et al., 2025), showing that enforcement can affect advancement within existing employment relationships.

More broadly, our findings identify a workplace channel linking household balance sheets to real activity. Existing work has linked household debt to aggregate demand, investment, and monetary policy transmission (Mian and Sufi, 2014; Melzer, 2017; Alpanda and Zubairy, 2019). Wage garnishment provides a complementary channel operating through workers, coworkers, and firms. Our evidence therefore identifies labor-market and organizational costs of debt enforcement that are relevant to its design alongside its benefits for creditor recovery and credit supply. It does not, of course, by itself establish the welfare balance between these effects.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and Section 3 the data. Section 4 presents the empirical strategy, Section 5 the effects on workers, coworkers, and firms, and Section 6 reports the evidence from a natural experiment separating incentives from stigma. Section 7 concludes.

2 Institutional Setting

Wage garnishment (Swedish *"löneutmätning"*) is the main legal mechanism through which overdue unsecured debts are enforced in Sweden. When a debtor has unpaid claims registered with the

Swedish Enforcement Authority (SEA, Swedish *"Kronofogden"*), the agency can issue a garnishment order instructing the employer to withhold part of the worker's net earnings and remit the withheld amount directly to the Authority. The SEA then distributes the funds across creditors according to statutory priority rules. The scale of the system is substantial: the SEA processes more than one million claims in a typical year. Figure 1 describes the composition of these claims. By number, the caseload is dominated by small public claims, such as unpaid taxes, fines, and fees. By value, private claims dominate, and the largest single category is debt held by collection agencies and debt buyers, which is largely repackaged consumer and bank credit. Enforcement thus spans a heterogeneous set of creditors, from government agencies collecting small routine claims to commercial debt buyers pursuing larger consumer balances.

Garnishment continues until the outstanding balance is repaid, the debtor experiences a substantial change in financial circumstances, leaves wage employment, or receives formal debt relief. Debt relief is rare. Sweden's restructuring procedure (*"skuldsanering"*) requires applicants to demonstrate that they are permanently unable to repay within a foreseeable horizon and that their situation is unlikely to improve; approved applicants enter a five-year repayment plan with a fixed disposable-income allowance. Approvals number roughly 10,000 per year, against approximately 400,000 individuals with debts registered at the SEA. For most garnished workers, the system therefore operates under full recourse: the debt follows the debtor until it is repaid.

The amount garnished is determined by a statutory subsistence allowance, the *"normalbelopp"*, which sets the minimum income a garnished debtor may keep. The allowance is set annually by the SEA, adjusted in line with the Consumer Price Index, and designed to cover basic living expenses such as food, clothing, and household goods. It varies with household composition, with separate allowances for single adults, cohabiting couples, and children of different ages.[5] In addition, the SEA exempts documented housing costs and certain necessary expenditures, such as childcare and commuting. The sum of these components is the protected income level, the *"förbehållsbelopp"*, below which earnings cannot be seized. The mandated garnishment amount (the figure the SEA writes into the garnishment decision specifying what the employer must withhold each month) for individual i in month t is defined as $G_{it} = \max\{0,\ \text{NetIncome}_{it} - \text{Normalbelopp}_{it} - \text{HousingCost}_{it}\}$. Only G_{it} is remitted to the SEA by employers, and the mandated garnishment amount is pre-determined:

[5] Table A.1 reports the normalbelopp for 2007–2026 across household types.

debtors cannot influence it directly and adjustments occur only when the SEA revises a decision based on updated information and are never applied retroactively. The onset of garnishment therefore produces an immediate and involuntary reduction in disposable income.

After the SEA issues a debt-collection decision, a caseworker determines the method of recovery. Liquid assets such as bank balances are seized first; when these are insufficient and the debtor has wage income, garnishment is pursued before the forced sale of physical assets. Wage garnishment thus typically begins only after the debtor's liquid assets are exhausted. The time between the debt-collection decision and the start of garnishment varies considerably across debtors (Figure 2). Part of the variation is mechanical: garnishment cannot begin while the debtor's income remains below the protected threshold, so debtors with low or irregular earnings may enter garnishment only once their income rises above it. However, much of this variation reflects factors unrelated to the debtor: caseworker processing times, the SEA's workload, and the behavior of individual creditors, who differ in how quickly they file and pursue claims. Our empirical design (see Section 4) exploits these timing idiosyncrasies.

Two features of this institution are important for our analysis. First, garnishment is administered through the employer: the firm is notified of the order and is responsible for withholding the garnishable amount, so the worker's financial distress is necessarily revealed to the employer. Second, the protected amount creates a sharp threshold above which every additional krona of earnings is seized, so garnishment imposes an effective marginal tax rate of 100% on earnings above the threshold. The first feature creates scope for a labor demand response; the second distorts labor supply. Both channels connect household financial distress directly to the workplace and will be discussed in more detail below.

3 Data

Our analysis combines debt enforcement records with matched employer–employee and firm-level registers, allowing us to study garnished individuals, their coworkers, and their employers before and after the onset of wage garnishment. The core of the dataset is the universe of wage-garnishment decisions from the SEA between 2004 and 2022, including the timing of garnishment initiation, the mandated monthly repayment amount, realized remittances, and the debtor's household composi-

tion and housing costs. We use these records to identify when an individual first becomes subject to wage garnishment and to measure the intensity and duration of enforcement.

We link the enforcement records to monthly earnings and employment information from the BAS, AGI, and LSS registers for 2019–2022, which provide employer-reported wages, employment spells, contract hours, and sickness absence at monthly frequency. The monthly worker-level analyses therefore cover garnishment events that begin during the years 2019 to 2022 and comprise 107,000 garnished individuals. Table 1 summarizes garnished individuals at the time of first garnishment. The average garnished worker is male, 43 years old, earns SEK 172,682 per year (about 50% below the national average), and faces a garnished amount of roughly SEK 4,200 per month. Garnishment spells are long: the mean duration of a first spell is 15 months, with substantial variation across individuals. Among individuals whose first garnishment has ended, 23% have repaid the debt, 8% apply for debt relief, and 79% are eventually garnished again (these events are not mutually exclusive). Wage garnishment thus exposes a relatively disadvantaged segment of the workforce to sizable and persistent income losses.

We link individual workers to health registers from the National Board of Health and Welfare. These include (i) all diagnoses and surgical treatments of in- and outpatients from the National Patient Register, and (ii) all prescribed drugs dispensed at pharmacies from the Swedish Prescribed Drug Register, which records the drug name, the active ingredient's Anatomical Therapeutic Chemical (ATC) classification, quantity, strength, and the dates of prescription and purchase. From these registers we construct two annual outcomes: an indicator for any in- or outpatient hospital contact, and an indicator for filling at least one prescription for psychotropic medication, defined as the pooled ATC categories of anxiolytics (N05B), hypnotics and sedatives (N05C), and antidepressants (N06A).

To study how individual distress transmits to firms at a longer horizon, we consider annual data, which is available for a longer sample period than our data of monthly frequency. We match garnished workers to employers using RAMS, Sweden's linked employer–employee register, available for 1999–2021. RAMS identifies each worker's main employer in a given year,[6] from which we construct firm-level exposure measures such as the share and number of garnished employees. We merge this information with balance-sheet and income-statement data from FEK for 1999–2020,

[6] The main employer is defined as the firm providing the largest source of income during the year.

which covers the full population of incorporated firms and provides annual net sales, value added, profits, employment, industry, and legal form. To separate firms from individuals, we restrict the main firm analysis to limited-liability companies, whose finances are legally separate from their owners; these firms account for 82% of firms with garnished workers (Figure 3).[7] This sample comprises 213,380 garnished workers. Furthermore, we currently identify coworkers (spillover) effects by focusing on firms that have up to ten employees; in such firms, workers are more likely to interact and work in a single establishment.[8]

Table 3 compares firms that ever employ a garnished worker with firms that never do; over the sample period, 41,620 firms employ at least one garnished worker. Such "exposed" firms are larger on average, employing around 45 workers and generating SEK 135 million in net sales, against about 2 workers and SEK 6 million for other firms. However, the size distribution of exposed firms is concentrated among small firms (Figure 3), and the average number of garnished employees per exposed firm-year is only 0.23. In many exposed firms, a single garnished worker therefore represents a nontrivial share of productive capacity.

Exposure is also concentrated by sector. Exposed firms are disproportionately in construction, manufacturing, wholesale, transport, and hospitality (Panel (a) of Figure 3); these industries are characterized by manual tasks, continuous labor input, and limited operational slack. Among small firms employing between one and five garnished workers, these industries account for nearly two-thirds of establishments (Table 5). These industries are also those in which worker performance tends to have particularly direct implications for firm output, suggesting that any reduction in effort, attendance, or hours among garnished workers may more readily translate into observable firm-level disruptions.

4 Empirical Strategy

We exploit a staggered adoption design and focus on individuals who experience a first wage garnishment during the analysis window. By conditioning on eventual garnishment, we compare individuals who differ only in the timing of their first enforcement action, holding constant unobserved

[7] In the sample of all treated firms, 82% are LLCs. For comparison, in the sample of firms who never experience a garnished worker, 60% are sole proprietorships and only 34% are LLCs.

[8] In future revisions, we will identify all coworkers at the establishment level, using workplace identifiers (CFAR) in RAMS.

factors that determine whether a person ever becomes subject to garnishment. The key identifying assumption is that, conditional on eventual garnishment, the precise timing of onset is as good as random. As discussed in Section 2, the delay between a debt-collection decision and the start of garnishment varies widely (Figure 2), including among debtors with only a single debt-collection decision, for whom that decision is necessarily the claim being enforced. This variation is driven largely by factors unrelated to the debtor: caseworker processing, the SEA's workload, and the filing behavior of heterogeneous creditors.

To estimate effects in this staggered setting, we implement the estimator of Callaway and Sant'Anna (2021), which compares each treated cohort only to individuals not yet treated at the same calendar date, avoiding the problematic comparisons that arise in two-way fixed effects estimation when treatment effects are dynamic. For each cohort g (individuals first garnished in period T_g) and period $t \geq T_g$, the estimator computes a group–time average treatment effect $ATT_{g,t}$ by comparing the change in outcomes of cohort g since the period before treatment with the corresponding change among not-yet-treated individuals. Because the estimator compares changes rather than levels, permanent differences across individuals are differenced out, and common shocks in calendar time are absorbed by the not-yet-treated comparison group. We estimate each $ATT_{g,t}$ with the doubly robust estimator, conditioning on age to account for life-cycle patterns in earnings; age enters both the propensity score and the outcome model, and the estimator remains consistent if either is correctly specified. Re-indexing to event time $s = t - T_g$ and aggregating across cohorts yields the event-study coefficients $\widehat{ATT}_s$ reported in our figures, with weights reflecting cohort sizes;[9] aggregating across all post-treatment cohorts and periods yields the average effects reported in our tables. Standard errors are clustered at the individual level (worker outcomes) or the firm level (firm outcomes).

We estimate the model at two frequencies: monthly, with a symmetric window of eight months around the first garnishment, for the high-frequency worker outcomes; and annually, with a three-year window, for annual worker outcomes and all firm outcomes.

[9] By default, pre-treatment coefficients in this estimator are computed relative to the preceding period rather than to a common base period, which `?` notes can make long-run deviations from parallel trends harder to detect. Re-estimating the event studies with a universal base period (the `long2` option in Stata's `csdid`) leaves both the pre-treatment and post-treatment coefficients essentially unchanged.

4.1 Threats to Identification

Two pieces of evidence support the assumption that onset timing is as good as random. First, using Weibull duration models, we examine whether the time from debt-collection decision to garnishment is related to observable worker and firm characteristics. The estimates in Table 7 show small associations with individual characteristics, and the employer's return on assets, solidity, and leverage are statistically unrelated to the onset of garnishment. If anything, faster entry into garnishment is weakly associated with employment at larger, higher-sales firms, that is, positive rather than negative selection into earlier garnishment. These relationships persist when controlling for industry and industry-by-year fixed effects. Second, the event-study estimates reported below show no differential pre-trends in the monthly data or among always-employed workers: treated individuals evolve in parallel with not-yet-treated individuals until enforcement begins.

A remaining concern is a common shock that both triggers garnishment and worsens outcomes, such as financial distress at the employer or in the household. Two further results argue against this. First, non-garnished coworkers in the same firms *increase* their hours and promotion rates at exactly the onset date (Section 5.2.2), whereas a firm-wide shock would move both groups in the same direction. This analysis covers firms with up to ten employees, which is also where the firm-level effects concentrate; for larger firms, the hazard estimates and the absence of pre-trends in firm outcomes (Figure 13) remain the relevant evidence. Second, our results are not driven by workers who experience a concurrent household or health shock: the estimates are little changed when we exclude workers who divorce during the event window (-0.149) or who experience a health shock (-0.135), compared with -0.148 in the full always-employed sample (Table 9).

One component of timing variation deserves separate attention. Garnishment cannot begin while a debtor's income is below the protected threshold, so for some debtors a long delay between the debt-collection decision and garnishment reflects earnings that only later rose above the threshold. Treatment onset is then partly triggered by the debtor's own earnings path. This is the mirror image of the classic "Ashenfelter dip," whereby earnings decline before enrollment in training programs and subsequent gains partly reflect mean reversion (Ashenfelter, 1978): here, selection into treatment requires earnings to *rise* above the threshold, so post-garnishment declines could partly reflect reversion from a selection-induced peak. Indeed, average earnings rise in the

months before garnishment in the raw data (Figure 5), and part of that rise is mechanical selection into treatment. Three observations indicate that mean reversion does not drive our results. First, the event-study estimates show no relative run-up before treatment in the monthly data or among always-employed workers, whereas mean reversion would produce one. The exception is the annual sample of workers employed in the year before garnishment, where the pre-period rise in Panel (c) of Figure 6 indicates that the estimate combines the extensive margin with some reversion; we therefore treat it as an upper bound and rely on the always-employed and monthly estimates for magnitudes. Second, the estimated effects persist and grow over several years, while reversion from a transitory peak would be temporary. Third, the effects appear on margins that mean reversion in earnings cannot explain: sickness absence, coworker outcomes, and firm exit.

5 Results

We present results in three steps: effects on garnished workers, spillovers to their coworkers, and consequences for firms. Throughout, the estimates capture the total effect of garnishment, combining the incentive distortion ("labor supply") and the employer's potential response to being notified of the worker's financial problems by the SEA ("labor demand"); Section 6 separates the two.

5.1 Initial Evidence on Garnishment Responses

We first document general patterns in the administrative data. These are descriptive, but they preview both the mechanism and the identification challenges addressed in Section 4.1.

Figure 4 compares mandated and realized repayments ten months after the first garnishment begins. Panel A shows that realized repayments fall significantly short of the mandated garnishment amount (see Section 2), with a large mass of observations at or near zero. Panel B plots the distribution of the gap between wage income and the protected threshold and shows sharp bunching at the statutory minimum: many garnished workers earn almost exactly the amount they are allowed to keep. Because the mandated amount is fixed by the SEA and cannot be adjusted by the debtor, the shortfall in realized repayments reflects changes in garnishable income. This observation is consistent with workers, on average, reducing earnings toward the protected threshold once garnishment begins, which is the behavior predicted by a 100% marginal tax on earnings above

it.

Figure 5 plots the corresponding income dynamics. Panel A reports average monthly earnings from ten months before to ten months after garnishment onset. Earnings rise before enforcement; this partly reflects the mechanical selection discussed in Section 4.1, because garnishment can begin only once income exceeds the protected threshold. After the onset of garnishment, earnings drop sharply and continue to decline for several months before stabilizing at a lower level. Panel B shows that the garnished amount spikes at enforcement and falls by roughly SEK 1,000 within two months, mirroring the earnings decline. Together, the two figures show an involuntary loss of disposable income, followed by a behavioral response: workers react by cutting garnishable earnings, which in turn erodes realized debt repayments. We now quantify these responses in the difference-in-differences framework.

5.2 Effects of Wage Garnishment on Employees

We begin with monthly outcomes, which trace possible short-run adjustments to garnishment at high frequency. We then turn to annual outcomes, which permit us to cover a richer set of outcome variables, including contract hours and sickness absence, that shed light on the drivers of the earnings decline documented in the previous section.

Table 8 reports average post-treatment effects on monthly earnings; Figure 6 presents the corresponding event-time dynamics. Monthly earnings fall by roughly SEK 1,245 on average, about 5% of pre-treatment monthly pay.[10] The probability of having no wage income rises by 1.6 percentage points on average in the monthly data (Table 11).[11] Figure 9 shows that exits build up gradually rather than jumping at onset: in the monthly data, the share without wage income is flat before garnishment and rises steadily to about 5 percentage points eight months after onset (Panel (a)). In the annual data, the share rises to about 45 percentage points three years after onset, with a post-period average of 28 percentage points (Panel (b)). The negative pre-period coefficients in Panel (b) are mechanical rather than evidence of differential trends. The

[10] The pre-period mean of monthly earnings is SEK 25,797 (Table 8). The corresponding estimate in inverse hyperbolic sine (IHS) units is -0.091. Throughout the paper, several outcome variables are transformed using the inverse hyperbolic sine function, $\text{asinh}(Y) = \ln(Y + \sqrt{Y^2 + 1})$, which behaves like a logarithm for large Y but is defined at zero. Following Chen and Roth (2024), IHS unit changes do not map one-for-one into percent changes (the mapping depends on the baseline level of Y), so we report effects in IHS units where appropriate.

[11] In unreported tests, we also find that the probability of receiving unemployment benefits increases by 2.4 percentage points.

sample is restricted to workers employed in the year before garnishment, and garnishment can begin only for workers who have wage income, so treated workers have wage income by construction in the years leading up to onset, whereas not-yet-treated workers in the same calendar years need not. The monthly data, where the window is short enough for this restriction to bind for both groups, show no such pattern. The dynamics in Figure 6 sharpen the interpretation: earnings are stable in the months before the first garnishment, drop abruptly at $t = 0$, and decline gradually thereafter before stabilizing. The drop in earnings among workers employed in the month before garnishment (Panel (a)) is partially driven by transitions out of employment, but earnings also drop significantly among workers who remain employed throughout the window (Panel (b)). The kink coincides exactly with the administrative start of garnishment enforcement, and pre-trends are flat in both samples. Because garnishment typically begins only after the debtor's liquid assets are exhausted (see Section 2), these workers have little scope to absorb the income loss through savings; labor supply is the principal remaining margin of adjustment.

Panels (a)–(c) of Figure 8 split the monthly earnings response of always-employed workers by gender, education, and age (Panel (d), which splits by sector, is discussed in Section C). The responses are similar across groups: each experiences a persistent decline beginning when garnishment starts, with only modest differences in magnitude. The absence of demographic heterogeneity is consistent with a mechanism operating through a margin common to all garnished workers, namely the 100% marginal tax on earnings above the protected threshold, rather than through differences in labor supply elasticities across groups.

Heterogeneity instead emerges along dimensions consistent with the repayment-horizon logic discussed in Section 1. Figure 7 splits the monthly earnings response of always-employed workers by the worker's debt-to-income ratio (Panel (a)) and by income rank (Panel (b)). The earnings decline is markedly stronger for workers with high debt relative to income, and it is concentrated among low-income workers: earnings in the bottom income quartile fall by more than 0.5 IHS units within eight months, whereas earnings in the top quartile barely move. Both patterns are consistent with the same repayment-horizon logic: under full recourse, one benefit of continuing to earn above the threshold is that additional repayments bring forward the end of garnishment. For workers with low debt relative to income, and for high earners, whose surplus above the threshold retires debt quickly, that prospect is near, and the income effect sustains labor supply. For heavily

indebted and low-income workers, the repayment horizon is distant, discouragement dominates, labor supply decreases, and earnings fall toward the protected threshold.[12]

Annual income data confirm and shed more light on these patterns (Panels (c) and (d) of Figure 6, and Table 9). Among workers employed in the year before garnishment, annual earnings fall by roughly 3.5 IHS units. This estimate is several times larger than the corresponding estimate for always-employed workers at -0.148 (Panel (d)), because it also captures the extensive margin: workers who lose employment after garnishment (Figure 9; Table 11) contribute post-treatment observations with earnings at or near zero. As discussed in Section 4.1, this broader estimate also shows a pre-period rise and should be read as an upper bound. The comparison across samples nevertheless separates the two margins of adjustment: the always-employed estimate isolates earnings reductions within ongoing jobs, while the broader estimate adds the effect of job loss itself. Among garnished workers who remain employed throughout the window, contract hours decline by 1.2% (Panel (a) of Figure 10), and the sick-leave rate rises by 0.5 percentage points (Panel (b)), about 13% of the pre-period baseline of 3.8% (Table 9). Hospital contacts and filled prescriptions of psychotropic medication, by contrast, *decline* by 7.7% and 12% respectively, relative to the pre-treatment means (Panels (d) and (e) of Figure 10). Both health measures thus move in the opposite direction from sick leave; this is consistent with the additional absence reflecting withdrawal from work rather than worsening health. Workers thus reduce earnings through fewer contracted hours and increased absence.

Workers may also respond to garnishment by shifting income or activity toward margins that are less directly exposed to wage withholding by the employer. Appendix C examines several such responses. First, workers may redirect income toward forms that are not withheld at source, for example, by earning income through a business or receiving capital income rather than wages. Consistent with this possibility, capital income rises after garnishment, and workers who leave wage employment become somewhat more likely to report business income. We also observe responses within the household. Spouses reduce their own earnings, consistent with the fact that the protected amount for cohabiting debtors depends partly on household income. Finally, workers

[12] An alternative reading of the income gradient in Panel (b) is that low-wage jobs simply permit easier adjustment: hours are divisible in hourly service work in a way they are not in salaried positions. Job flexibility likely contributes, but it cannot account for the full pattern: earnings decline significantly even among professionals and managers (Table 14), occupations where hours are least divisible.

may seek to maintain employment while shifting some compensation toward undeclared payments. Such adjustment may be easier in sectors in which cash payments and undeclared work are more prevalent, such as restaurants or transportation. To examine this possibility, we compare earnings responses across sectors with different exposure to undeclared work, yet, we find similar declines in high- and low-risk sectors.

These patterns suggest that garnishment induces some adjustment beyond reported wage income. These responses, however, are quantitatively too small to account for the decline in wage earnings. In particular, the increase in reported business income is small relative to the rise in the probability of having no wage income, and earnings fall substantially even among workers who remain continuously employed.[13] We therefore interpret these avoidance responses as an additional consequence of enforcement rather than as an alternative explanation for the labor-supply effects documented above. Detailed results are reported in Appendix C.

Taken together, the evidence indicates that the earnings decline reflects a meaningful reduction in labor supplied within ongoing employment relationships. We next examine whether garnishment also affects workers' longer-run career progression.

5.2.1 Career Progression

On the supply side, garnishment reduces the return to the discretionary effort required to earn a promotion: a higher salary above the protected threshold yields no additional disposable income. On the demand side, the enforcement process is highly visible to the employer (the SEA contacts the firm directly and the payroll office administers the withholding); garnishment therefore reveals the worker's financial distress and may change the employer's assessment of their reliability or suitability for advancement.

Panel (c) of Figure 10 plots event-study estimates for promotions among always-employed workers. The probability of promotion falls immediately after garnishment begins and remains depressed for at least two years. The average post-treatment effect of -0.4 percentage points is a decline of roughly 27% relative to the pre-period baseline promotion rate of 1.5% (Table 9). At the same time, hospital contacts and psychotropic prescriptions decline (Panels (d) and (e) of Figure 10),

[13] Moreover, income shifting cannot explain the subsequent coworker and firm responses: higher coworker hours and sickness absence, together with lower firm output, point to a reduction in the garnished worker's contribution to production.

making health shocks an unlikely explanation for the stalled career progression. Instead, the effect plausibly operates through reduced worker effort, revised employer perceptions, or both. The event-study estimates cannot separate these channels, as both predict fewer promotions, which is precisely the identification problem the reform in Section 6 addresses.

The picture that emerges is of workers whose careers stall at the onset of enforcement: earnings fall, hours and attendance decline, and internal advancement slows. The consequences of garnishment thus extend beyond immediate liquidity problems to workers' longer-run career trajectories.

5.2.2 Workplace Spillovers

If garnished employees reduce hours, take more sick leave, and contribute less effort, their colleagues may have to bear additional responsibilities or experience heightened stress in the workplace. We examine this directly. Figure 11 and Table 12 report outcomes for coworkers, which we compare with the corresponding outcomes for garnished workers in Figure 10. Coworkers are "non-garnished" workers employed at the same firm in the year of a colleague's first garnishment. Note that in this analysis involving coworker spillovers, we focus on firms with up to ten workers. The reason is that such firms are more likely to be single-establishment firms, where we can measure spillovers if such effects actually exist.

We study three outcomes: contract hours, sick leave, and promotions. The contrast is striking. While garnished workers reduce contract hours (Panel (a) of Figure 10), their coworkers *increase* hours by 9.2 percentage points, roughly 12.5% relative to baseline (Panel (a) of Figure 11). Coworkers' sick leave also rises by 64% relative to baseline (Panel (b)), an early indication that the added workload is costly. Further, their promotion rates increase by 60% relative to baseline (Panel (c)), mirroring the decline in promotions among garnished workers: advancement opportunities that garnished workers forgo appear to go to their colleagues instead. Coworkers, in short, absorb part of the labor supply that garnished workers withdraw.

This workload reallocation is not costless for coworkers of garnished workers. Panel (d) of Figure 11 shows that coworkers' hospital contacts increase by 11% relative to baseline, and filled prescriptions of psychotropic medications (anxiolytics, hypnotics and sedatives, and antidepressants) rise in the years following a colleague's first garnishment even though the increase is only statistically significant in $t = 2$. The two results corroborate each other: heavier workloads and

greater organizational strain are consistent with genuine health costs. As mentioned before, garnished workers themselves display the opposite pattern: their own psychotropic prescriptions and hospital contacts *decline* after enforcement begins (Panels (d) and (e) of Figure 10), consistent with the resolution of uncertainty once a predictable repayment process replaces unresolved delinquent debt. The burden of garnishment thus shifts within the workplace: health improves, on these measures, for the garnished worker, while the workload, and its physical and psychological cost, affects the colleagues.[14]

The consequences of garnishment thus extend beyond the garnished worker, but the direction of the net effect for the firm is ambiguous. On the one hand, coworkers partially insure the firm against the shock: by working longer hours and stepping into vacated roles, they replace some of the labor that garnished workers withdraw.[15] On the other hand, this buffering is costly (coworkers' rising sick leave and deteriorating health may themselves reduce productivity) and the replacement may be incomplete or of lower quality. We examine the net effects at the firm level next.

5.3 Effects on Firm Performance

We estimate the same staggered event-study design at the firm-year level, comparing firms that employ a newly garnished worker in a given year to firms not yet exposed in the same calendar year. Table 13 reports average post-treatment effects; Figures 13 and 14 plot the dynamics.

We find that firm performance deteriorates broadly and persistently after a worker gets garnished. Value added begins to fall in the first year after exposure and declines by roughly 0.6 IHS units within three years (Panel A of Figure 13); net sales fall by 0.12 IHS units (Panel C). Scaling by the number of employees attenuates but does not eliminate the declines (Panels B and D): value added per employee falls by 0.51 IHS units against 0.58 IHS units for total value added, and net sales per employee by 0.27 against 0.28 for unscaled net sales (Table 13). Roughly 90% of the value-added decline thus remains after scaling by headcount. Employment adjusts less than output, so the effects reflect lower productivity rather than a mechanical contraction in the scale of the firm,

[14] This asymmetry also speaks to the concern that a contemporaneous firm-level shock, rather than the garnishment itself, drives our results. A common shock would move garnished workers and their colleagues in the same direction; instead, hours, promotions, and health all move in opposite directions for the two groups within the same firm. The pattern is difficult to reconcile with a shock hitting the workplace as a whole, but follows naturally from the reallocation of one worker's withdrawn labor to the others.

[15] This insurance role of coworkers parallels the risk sharing among inventor teams documented in Baghai et al. (2026), where colleagues absorb part of the productivity loss from a team member's adverse wealth shock.

which is what one expects when the garnished worker remains on the payroll and colleagues cover for them.

The IHS estimates are large in part because the transformation is sensitive to firms whose output falls toward zero: a firm that shrinks from several million SEK in value added to almost nothing contributes a drop of more than ten IHS units to the average, so a small number of near-exits can move the average considerably. To gauge the magnitude for the typical exposed firm, Figure 15 reports the same event studies for value added and net sales in SEK levels, winsorized at the 1st and 99th percentiles, for firms observed throughout the event window. Value added falls by about SEK 1.3 million in the first year after exposure, 2.0 million in the second, and 2.9 million in the third, against a pre-period mean of SEK 27 million, that is, by roughly 5%, 7%, and 11%. Net sales fall by SEK 2.8, 6.3, and 9.3 million against a mean of SEK 88 million, again about 3%, 7%, and 11%. Pre-period estimates are close to zero. For a firm that survives the window, the garnishment of one worker thus reduces output by about a tenth within three years, a sizable but plausible loss given that garnished workers average 22% of the workforce in exposed small firms (Table 4). These averages also mask substantial heterogeneity across firms, which we examine in Section 5.3.1. Firm exit rises by 0.6 percentage points, which can be compared to the average pre-event mean at 0.0004. Across all outcomes, pre-trends are flat: these firms' trajectories do not deteriorate before a worker experiences garnishment.

The effects are concentrated among small firms. Figure 16 splits the sample by pre-exposure firm size: among firms with 1–10 employees, value added falls by roughly 0.67 IHS units within three years, compared to -0.45 units for firms with 11-100 employees. Small firms have less formalized roles, less redundancy in task assignment, and fewer financial buffers, so the same worker-level distortion plausibly translates into larger organizational disruption. The size gradient also serves as a consistency and plausibility check. If our estimates reflected confounding shocks (e.g., local economic conditions that depress both firm performance and workers' finances), the effects should continue to be observed in large firms. Under our mechanism, by contrast, this is exactly the predicted pattern: one worker's distorted incentives cannot measurably move the output of a firm with hundreds of employees. For the same reason, the small estimates for large firms should not be read as evidence that effects are absent there: the worker-level disruption is likely similar, but too small relative to firm output to detect in firm-level aggregates. The concentration of detectable

effects among small firms does not limit their aggregate relevance: firms with 1–10 employees account for 23% of employment and 16% of value added in our sample (Table 6).

Among exposed firms with 1–10 employees, garnished workers average 22% of the workforce (Table 4). For the typical small exposed firm, garnishment therefore distorts the incentives of much of its workforce at once, and the coworker spillovers of Section 5.2.2 compound the direct effect. Household financial distress, transmitted through worker incentives and amplified within the workplace, thus translates into a significant shock to firm performance and survival.

In contrast to the sharp size gradient, the effects are broadly uniform across industries (Table 14). Worker-level earnings declines are similar across sectors, and firm-level declines appear throughout the industry distribution (estimates are indistinguishable from zero only in sectors with few exposed firms). The occupation panel is particularly telling: earnings effects appear across the skill distribution, among professionals and managers as well as in elementary and service occupations. The transmission from worker distress to firm performance thus does not depend on the labor arrangements of any particular industry or the characteristics of any particular job.

5.3.1 Which Firms Are Hit Hardest? Financial Health and Worker Type

The average firm-level effects are large: value added falls by 0.58 IHS units across all exposed firms and by 0.66 among firms with 1–10 employees (Table 13). Averages of this size raise the question of which firms bear them. A natural candidate is financial health. When a worker withdraws hours and effort, the firm must cover the gap, by paying overtime to colleagues, hiring temporary staff, or accepting delayed deliveries, and each of these requires liquidity. Firms with weak balance sheets have less room to do so, and the coworker results of Section 5.2.2 already indicate that the adjustment is costly.

Figure 17 splits exposed firms by three measures of pre-exposure financial health, each averaged over $t = -5$ to $t = -3$ so that the classification is unaffected by the garnishment itself: leverage (debt to assets), profitability (return on assets), and interest coverage (EBIT over interest expenses). For each measure we compare firms in the bottom and top quartiles. The pattern is the same across all three. Financially weak firms, those with high leverage, low profitability, or low interest coverage, see value added fall by roughly 1.2 to 1.6 IHS units three years after exposure, whereas financially strong firms see declines of about 0.8 to 0.9 IHS units (Panels (a)–(c)). The decline among strong

firms is similar in magnitude to the full-sample path in Panel (a) of Figure 13, which reaches about 0.8 IHS units by year three, while the decline among weak firms is roughly twice as large. The average effect is thus not uniform: it combines a moderate decline among firms that can absorb a worker's withdrawal with a much steeper decline among firms that cannot. Three caveats apply. The heterogeneity sample requires three years of pre-exposure financial data and therefore excludes young firms, so the comparison with the full-sample path is across different samples. Pre-period estimates for the low-profitability and low-coverage groups are noisier than for the other groups. And financial health is correlated with firm size, so part of the gradient may reflect the size effects documented above.

Two further splits inform the mechanism. Panel (d) divides firms by the intensity of the worker's garnishment, measured as the garnished amount relative to the worker's annual income in $t-1$. The firm-level response is nearly identical in the top and bottom quartiles. This is consistent with the incentive mechanism: once a worker is above the protected threshold, the marginal tax on additional earnings is 100% regardless of how much is withheld, so the worker's withdrawal, and hence the disruption to the firm, does not scale with the garnished amount. Panel (e) divides firms by whether the garnished worker is a blue-collar worker or holds a higher-ranked position. The decline in value added is larger when the garnished worker is not blue-collar. Higher-ranked workers are harder to replace at short notice and their tasks are more likely to be complementary to those of their colleagues, so their withdrawal is more disruptive, consistent with the team-production logic of Kremer (1993).

Taken together, the heterogeneity results indicate that the firm-level cost of a garnished worker depends on the firm's ability to absorb the shock and on how easily the worker's contribution can be replaced, rather than on the size of the garnishment itself. Financially constrained small firms employing hard-to-replace workers are where the costs of enforcement for production are concentrated.

6 Mechanism: Incentives versus Stigma

To separate the incentive channel from the stigma channel, we exploit a reform to the statutory subsistence allowance. Effective on June 1^{st}, 2022, the Swedish Enforcement Authority replaced

the two-bracket child schedule of the *normalbelopp* (ages 0–6 and 7+) with a four-bracket schedule (ages 0–6, 7–10, 11–14, and 15+).[16] The reform left the allowance for young children (ages 0–6) unchanged while raising it substantially for older children: the monthly allowance for a child aged 15 or above rose from SEK 3,150 to SEK 4,411, an increase of roughly 40%, with intermediate increases of 22% and 5% for ages 11–14 and 7–10. Figure 18 provides a graphical representation of the reform, while Table A.1 tabulates the changes in the statutory subsistence allowance for the relevant age categories in detail.

Because the protected threshold is the sum of the subsistence allowance and housing costs, the reform raised the threshold one-for-one for garnished workers with older children, reducing the share of their earnings subject to seizure, while leaving workers with only young children essentially unaffected. In our sample, more than 48,100 garnished individuals had children above age 6 and were thus affected by the reform (Table 15).

The reform moves the incentive channel while holding the stigma channel fixed. A worker whose threshold rises faces a lower effective tax on additional earnings, but the employer learns nothing new: the garnishment, and the employer's role in administering it, continue as before, and the change in the withheld amount is statutory, reflecting the ages of the worker's children rather than anything about the worker's quality. The two mechanisms therefore make different predictions. If distorted incentives drive the earnings declines of Section 5, workers with larger threshold increases should raise earnings after the reform; if employer stigma is the main mechanism, threshold increases should produce no earnings response.

6.1 Empirical Design

We study the reform within the population of workers under wage garnishment in 2022, comparing garnished workers whose protected thresholds rose by different amounts depending on the ages of their children. Because child age is predetermined, the reform generates plausibly exogenous variation in garnishment intensity across otherwise similar workers. Our data for this analysis cover calendar year 2022 only, so we identify the effect from within-year variation between the pre-reform months (January–May) and the post-reform months (June–December); the estimates

[16] For details on the reform, see Kronofogdemyndigheten (2021). Schedule changes apply automatically to outstanding garnishment orders: employers withhold according to the prevailing thresholds.

are short-run responses to the reform.

We estimate

$$Y_{it} = \alpha_i + \gamma_t + \beta \left(Post_t \times Treated_i \right) + \varepsilon_{it}, \tag{1}$$

where Y_{it} are the gross monthly wage earnings of garnished worker i in month t, $Post_t$ indicates the post-reform period, $Treated_i$ indicates at least one child aged 7 or above at the reform date, and α_i and γ_t are worker and month fixed effects. We then exploit the full continuous variation in reform exposure by replacing the binary treatment indicator with the worker-specific change in the protected threshold induced by the reform, $\Delta Threshold_i$:

$$Y_{it} = \alpha_i + \gamma_t + \theta \left(Post_t \times \Delta Threshold_i \right) + \varepsilon_{it}. \tag{2}$$

Because a higher threshold relaxes the effective tax only for workers whose earnings are close to or above it, we also estimate versions of both specifications interacting reform exposure with the worker's pre-reform distance to the threshold. The interaction sheds light on whether the reform has its largest effects exactly where the incentive distortion binds. A higher threshold should matter little for workers earning well below it, but considerably for workers close to or above it, where the reform restores the return to additional work.

The key identifying assumption is that the differential threshold change is unrelated to potential outcomes except through garnishment intensity. Child ages are predetermined and cannot be adjusted in response to the reform; worker fixed effects absorb level differences across family types; and month fixed effects absorb aggregate shocks, including the inflation of 2022. Identification thus comes only from the differential threshold increase across the new child-age brackets. The remaining concern is that workers with children of different ages were on different earnings trajectories before the reform. However, Figure 19 shows that before June 2022, earnings evolved in parallel across child-age groups; after the reform, the groups with the largest threshold increases exhibit relative earnings gains.

6.2 Results

Table 16 reports the estimates. Workers with children aged 7 and above increased earnings after the reform relative to workers with only younger children by at least SEK 400 (column 1). These results are difficult to reconcile with stigma as the primary mechanism.[17] Stigma does not vary in this sample: every worker is under garnishment, and every employer knows it. What varies is only the income the worker may keep at the margin, and outcomes respond to precisely that variation, where it binds. We conclude that the adverse labor-market effects of garnishment arise because garnishment changes the return to work itself, not merely because employers learn of the worker's distress.

The reform evidence also informs the interpretation of the spillover and firm results. The coworker effects of Section 5.2.2 are consistent with colleagues responding to garnished workers' reduced labor supply, while the firm effects of Section 5.3 reflect the net outcome of these worker and coworker responses. Because the 2022 reform shows that garnished workers' earnings respond to changes in the return to work even when employer information is held fixed, it establishes distorted work incentives as an important component of this transmission mechanism. Employer stigma may still contribute, particularly to career outcomes when garnishment first begins, but the reform shows that the incentive channel alone can generate economically meaningful changes in workers' labor market participation.

7 Conclusion

This paper identifies a workplace channel through which debt enforcement affects production and firm performance. Wage garnishment creates a form of employee debt overhang: workers bear the cost of additional effort while earnings above the protected threshold service existing debt. In Sweden's largely full-recourse setting, additional earnings can still benefit workers by bringing forward repayment and the end of garnishment. When debt is large relative to repayment capacity, however, this benefit may lie far in the future. Consistent with this logic, earnings declines are greater among workers with high debt-to-income ratios. Garnishment is also associated with reduced con-

[17] Employer stigma also cannot explain the heterogeneity in earnings responses by debt-to-income ratio and income (Figure 7).

tract hours and career progression, and it increases unemployment and sickness absence. The simultaneous decline in hospital contacts and psychotropic prescriptions among garnished workers makes deteriorating measured health an unlikely explanation for their increased absence.

The reform evidence supports the role of work incentives. Following the 2022 increase in protected thresholds, affected workers raise earnings relative to the comparison group while garnishment status and the employer's information set remain unchanged. This demonstrates that repayment rules affect labor market behavior without a new disclosure of financial distress to the employer. Employer responses may still contribute to the effects when garnishment first begins.

These individual responses matter for firms because production is interdependent. When tasks are complementary, one worker's reduced effort can impair the productivity of others; opportunities to reallocate work can also allow colleagues to absorb part of the disruption. Our evidence shows that coworkers increase hours and receive more promotions, consistent with such adjustment. Their sickness absence, hospital contacts, and intake of psychotropic medications also rise, indicating that this adjustment is costly. Firm value added and sales decline overall, and exit becomes more likely. These losses are most visible in small firms, where one garnished worker represents a substantial share of productive capacity, and in firms with weak balance sheets before exposure, which have less capacity to absorb a worker's withdrawal. The combined evidence is consistent with worker spillovers, and costly and incomplete adjustment within the workplace.

Credible debt enforcement supports credit supply and the functioning of debt markets. Our findings identify ex post labor market and organizational costs that should enter the assessment of debt enforcement institutions alongside those benefits. Evaluating alternative policies and repayment schedules requires accounting for creditor recovery, credit availability, workers' incentives, and adjustment within firms. Although our estimates do not establish the overall welfare balance, they show why household debt and its enforcement matter for the use of human capital inside firms and for parties beyond the borrower–creditor relationship.

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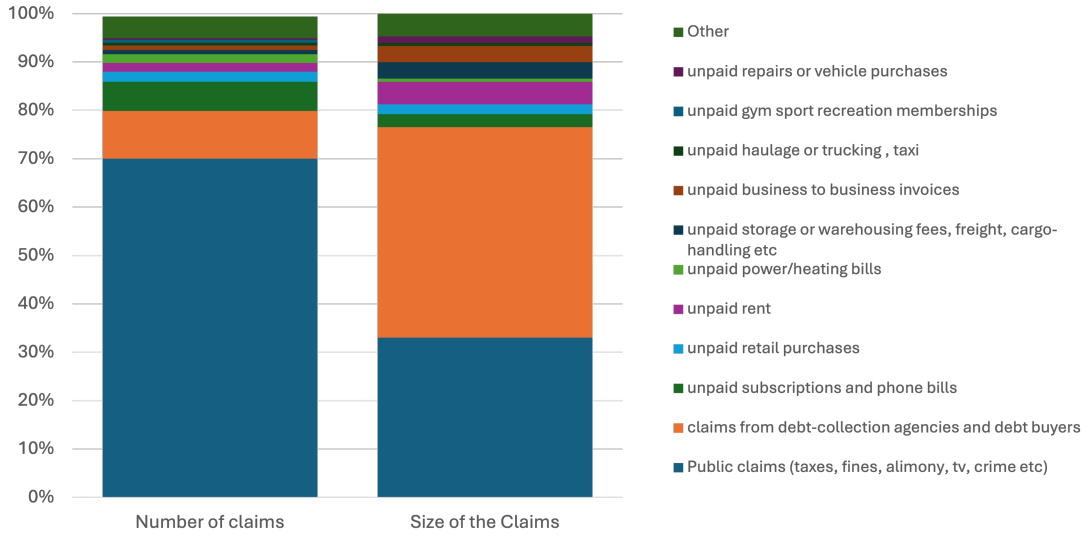
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8 Figures and Tables

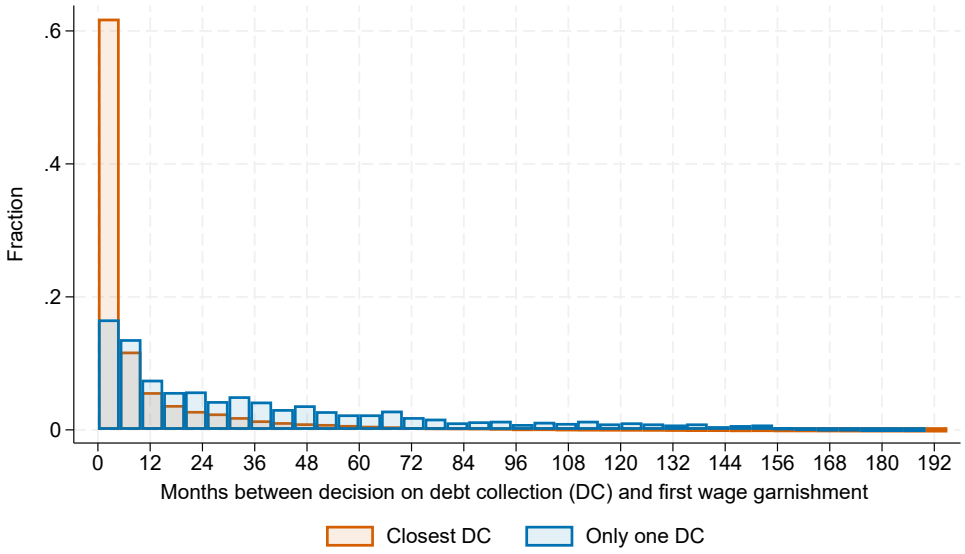
8.1 Figures

Figure 1: Composition of Enforcement Claims at the Swedish Enforcement Authority, 2016



Notes: The figure shows the composition of enforcement claims registered at the Swedish Enforcement Authority (Kronofogden) in 2016, by number of claims (left bar) and by claim value (right bar). Public claims comprise taxes, fines, alimony, and public fees. Claims from debt-collection agencies and debt buyers consist largely of repackaged consumer and bank credit. Source: SEA register and own calculations.

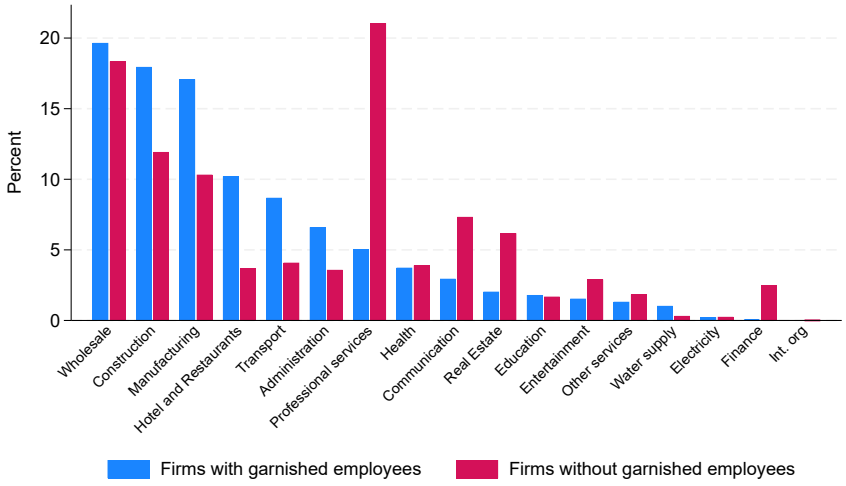
Figure 2: Variation in Timing of Wage Garnishment



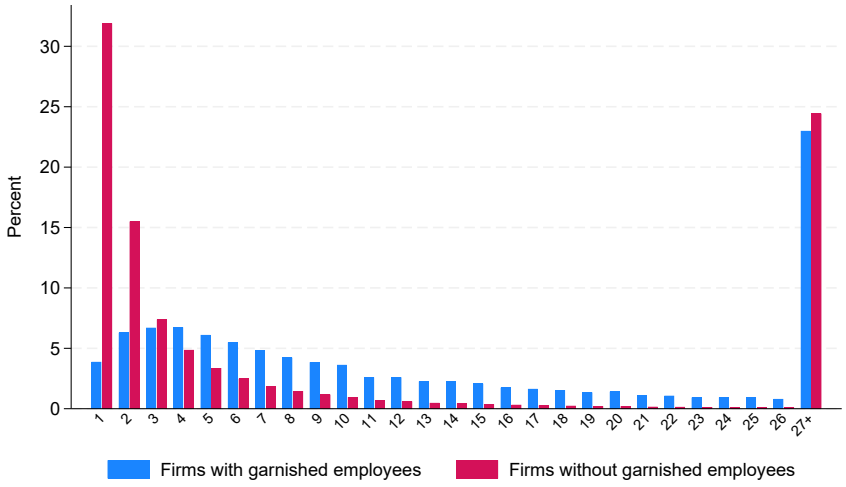
Notes: The figure shows the number of months between the decision on debt collection and the start of the first garnishment. We look at both the time between the closest debt collection decision and first garnishment for all workers, and for a sample of workers who only have one decision on debt collection between 1999 and 2020.

Figure 3: Firm Characteristics

(a) Sector Composition of Exposed Firms

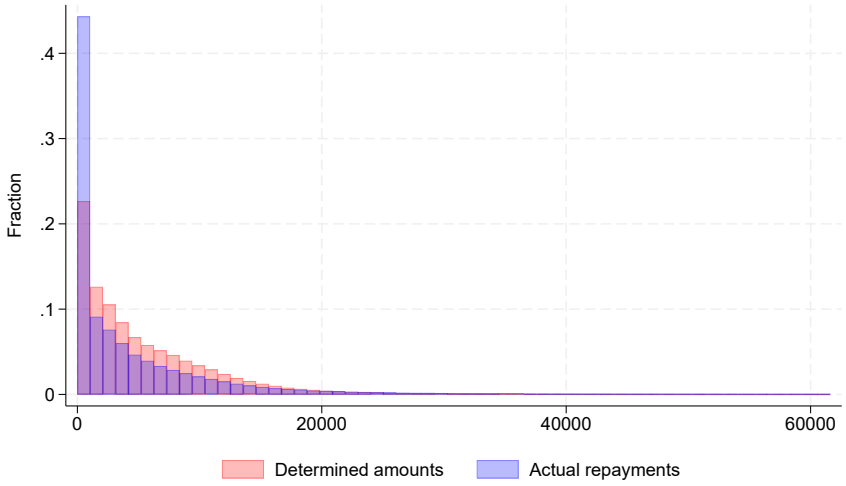


(b) Firm Size Distribution (number of employees)



Notes: Panel (a) shows the distribution of limited liability firms across sectors for firms included in the sample, compared to firms that never experience a garnished worker. Manufacturing and construction dominate, jointly representing roughly one-third of exposed firms. Other common industries include wholesale, transport, hotels and restaurants, and administrative services. Sectors tied to household-facing services or manual labor appear disproportionately represented, while high-skilled sectors are less represented.Panel (b) shows the firm-size distribution is skewed toward smaller firms: the modal firm has 1–3 employees, with rapid decline after size 10. This aligns with results showing stronger firm productivity losses within smaller firms.

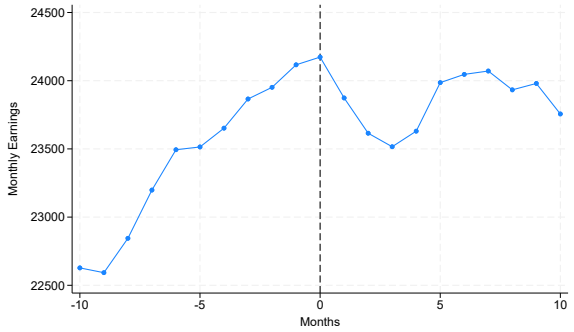
Figure 4: Distribution of Mandated and Actual Garnishment Amounts



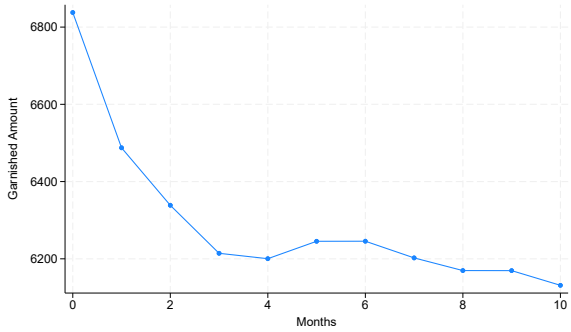
Notes: This figure documents the actual repayments and mandated garnishment amounts 10 months after first wage garnishment started (sample from 2004 to 2021). The x-axis reports the monthly level of garnishment in SEK.

Figure 5: Mean Income and Repayment Flows Around Wage Garnishment

(a) Monthly Earnings



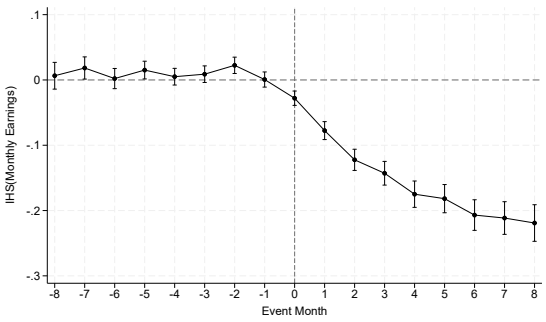
(b) Garnished Amount



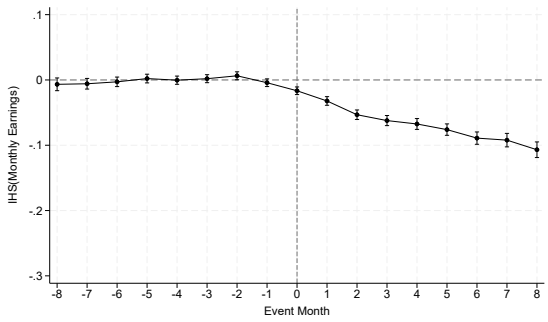
Notes: Panel (a) plots the average monthly earnings in SEK, 10 months pre and post the start of wage garnishment at time 0. Panel (b) plots the average amount garnished in SEK, 10 months post the start of wage garnishment at time 0. Both figures use data on monthly earnings and garnished amounts between 2019-2022. The earnings levels differ from the annual means in Table 1 because the monthly data cover a later period (2019–2022) than the annual sample (2004–2021) and are measured in nominal terms.

Figure 6: Monthly and Annual Dynamic Treatment Effects of Wage Garnishment on Earnings

(a) IHS(Monthly Earnings), Employed in t-1

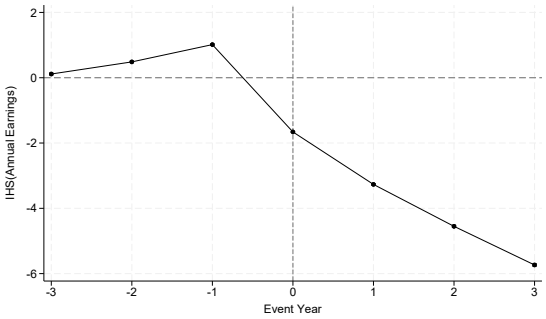


(b) IHS(Monthly Earnings), Always Employed

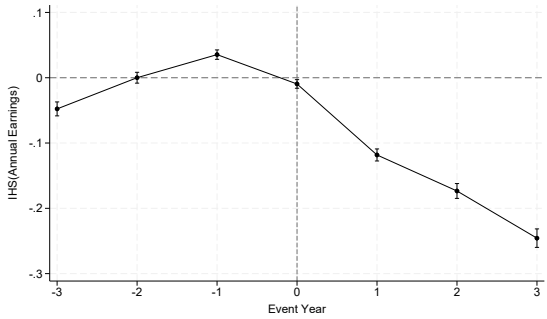


Notes: Each panel reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021). Estimates correspond to group–time average treatment effects aggregated by event time, where event time is defined relative to the month of an individual's first wage garnishment. Points show event-time-specific treatment effects (δ_s), and vertical bars indicate 95% confidence intervals based on standard errors clustered at the individual level. In each calendar month t, treated individuals are compared to individuals who have not yet entered wage garnishment by month t, who serve as the control group.

(c) IHS(Annual Earnings), Employed in t-1



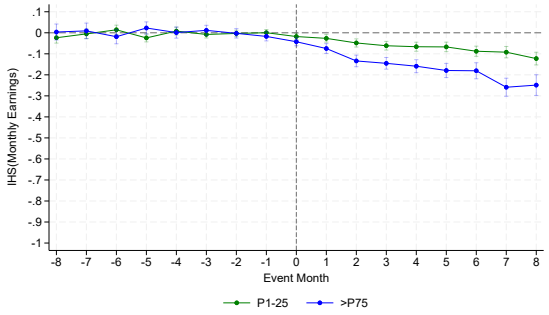
(d) IHS(Annual Earnings), Always Employed



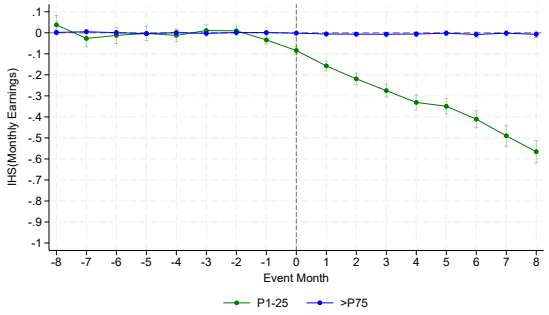
Notes: Each panel reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021). Estimates correspond to group–time average treatment effects aggregated by event time, where event time is defined relative to the year of an individual's first wage garnishment. In Panels (a) and (c) we require that individuals are employed in t-1. In Panels (b) and (d) we restrict the sample to individuals who are always employed. Points show event-time-specific treatment effects (δ_s), and vertical bars indicate 95% confidence intervals based on standard errors clustered at the individual level. In each calendar year t, treated individuals are compared to individuals who have not yet entered wage garnishment by year t, who serve as the control group.

Figure 7: Heterogeneous Monthly Dynamic Treatment Effects of Wage Garnishment on Earnings; by Debt to Income, and Income Rank, Always Employed Workers

(a) IHS(Monthly Earnings), D/I-rank

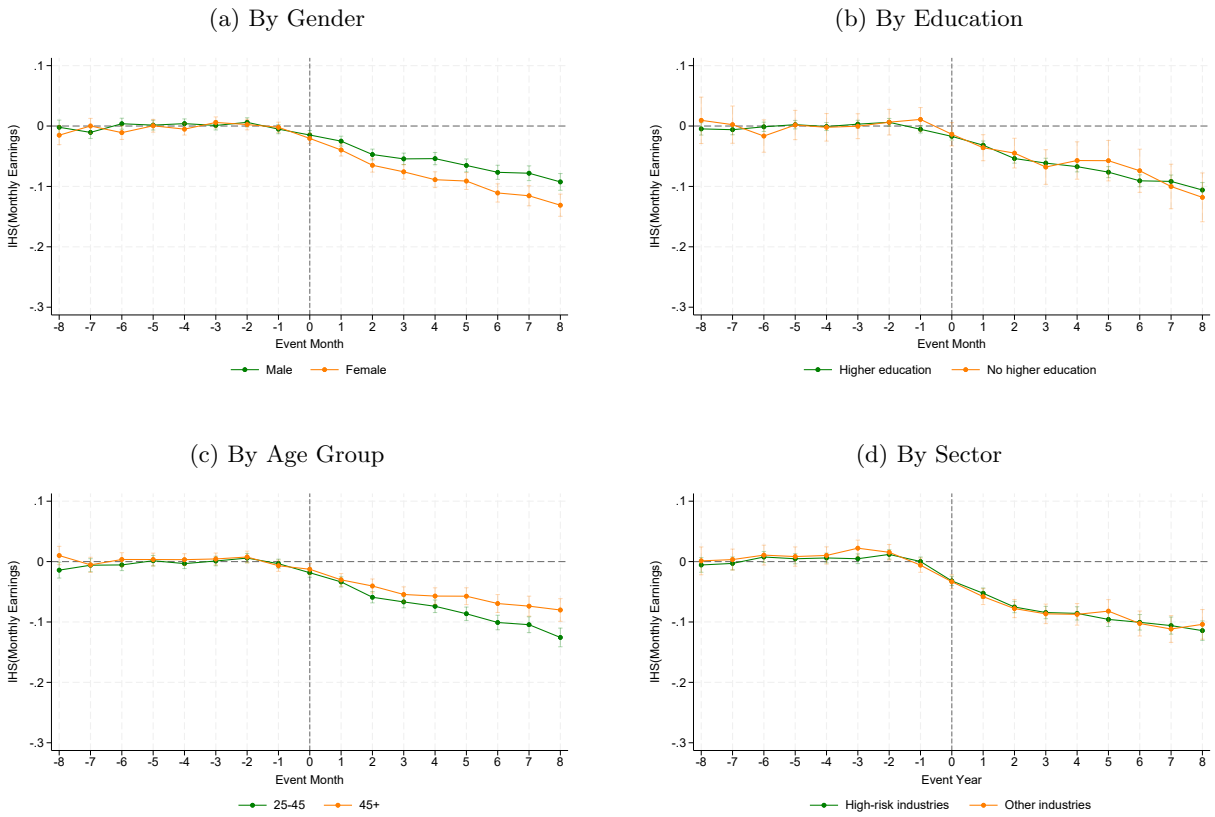


(b) IHS(Monthly Earnings), income ranks



Notes: Each panel reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021). Estimates correspond to group–time average treatment effects aggregated by event time, where event time is defined relative to the month of an individual's first wage garnishment. Points show event-time-specific treatment effects (δ_s), and vertical bars indicate 95% confidence intervals based on standard errors clustered at the individual level. In each calendar month t, treated individuals are compared to individuals who have not yet entered wage garnishment by month t, who serve as the control group. Debt and income ranks are determined in the year before the garnishment event. The sample is restricted to workers that are employed in every period from -5 to 5.

Figure 8: Heterogeneous Monthly Dynamic Treatment Effects of Wage Garnishment on Earnings, by Worker Characteristics, Always Employed

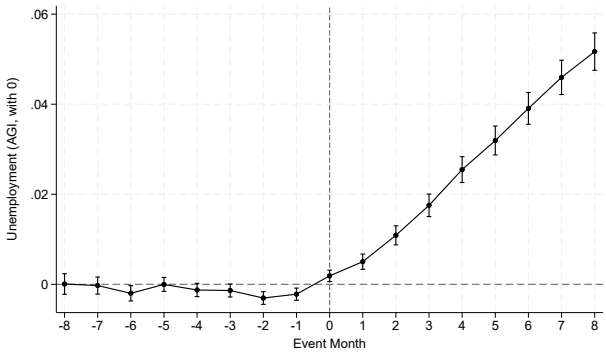


Notes: Each panel reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021). Estimates correspond to group–time average treatment effects aggregated by event time, where event time is defined relative to the month of an individual's first wage garnishment. The panels examine heterogeneity in treatment effects by gender, education, age, and whether the individual works in a sector identified by the Swedish Tax Agency as having a high risk of undeclared work. These sectors include restaurants; hairdressing, beauty care, and body care; industrial, institutional, and consumer laundry services; motor-vehicle service and repair, including body, paint, glass, electrical, and tire services; and wholesale trade in food, beverages, tobacco, grains, seeds, and animal feed. Points show event-time-specific treatment effects (δ_s), and vertical bars indicate 95% confidence intervals based on standard errors clustered at the individual level. In each calendar month t, treated individuals are compared to individuals who have not yet entered wage garnishment by month t, who serve as the control group. The sample is restricted to workers that are employed in every period from -5 to 5.

Figure 9: Monthly and Yearly Dynamic Treatment Effects of Wage Garnishment on Unemployment

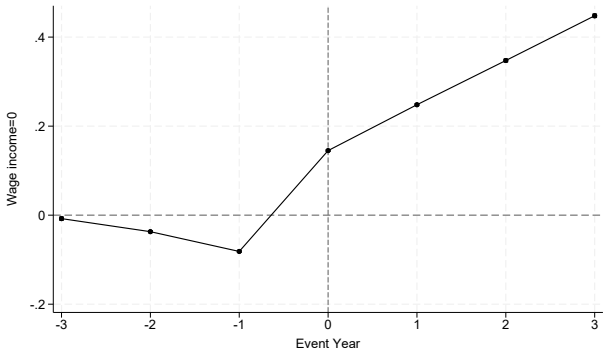
Monthly data, employed in t-1

(a) No wage income



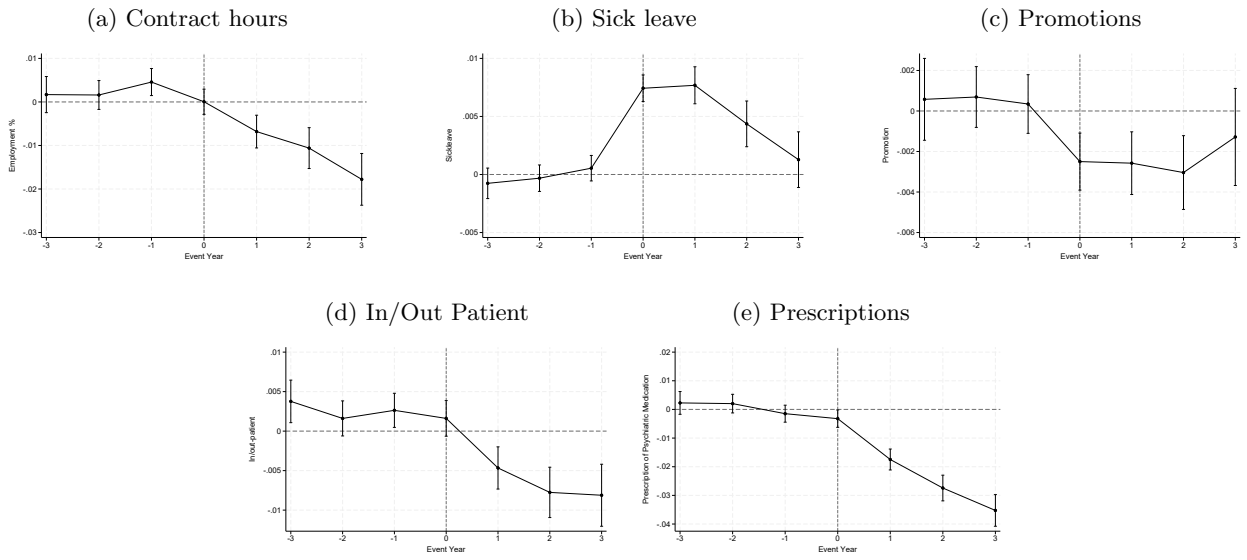
Annual data, employed in t-1

(b) No wage income



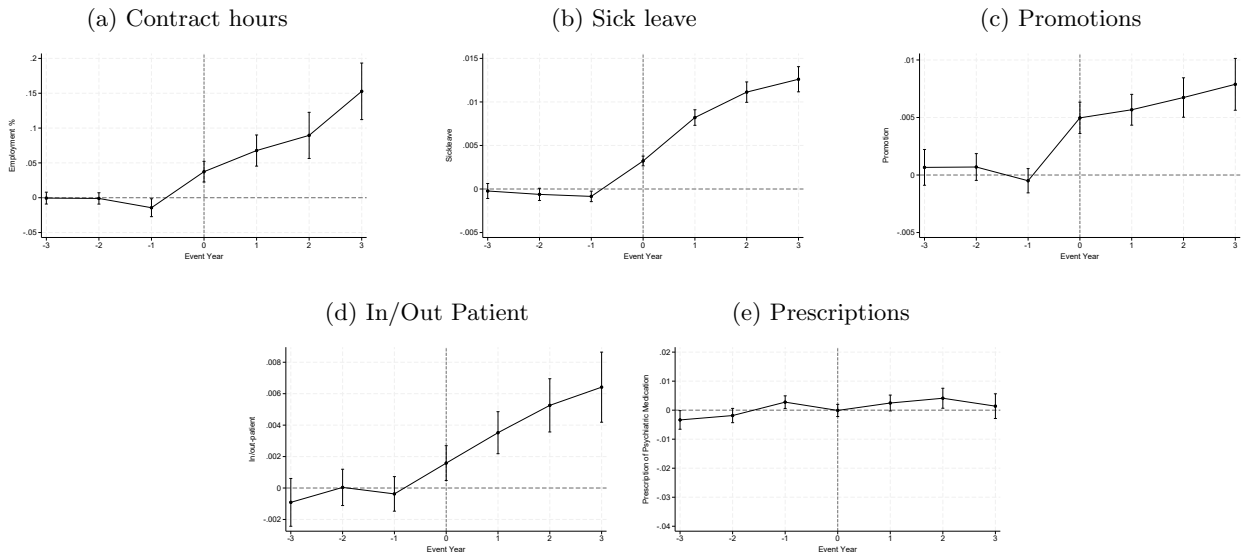
Notes: Each panel reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021), aggregated by event time relative to the year of the first wage garnishment. The figures report effects for garnished workers who are in employed in $t-1$. The outcome variable is an indicator for having no wage income. The top panel presents results using data at the monthly level, and the bottom panel uses data at the annual level. Points show event-time-specific treatment effects, and vertical bars indicate 95% confidence intervals based on standard errors clustered at the individual level. In each time t, treated individuals are compared to individuals not yet treated in t.

Figure 10: Annual Dynamic Treatment Effects of Wage Garnishment on Garnished Workers, Always Employed



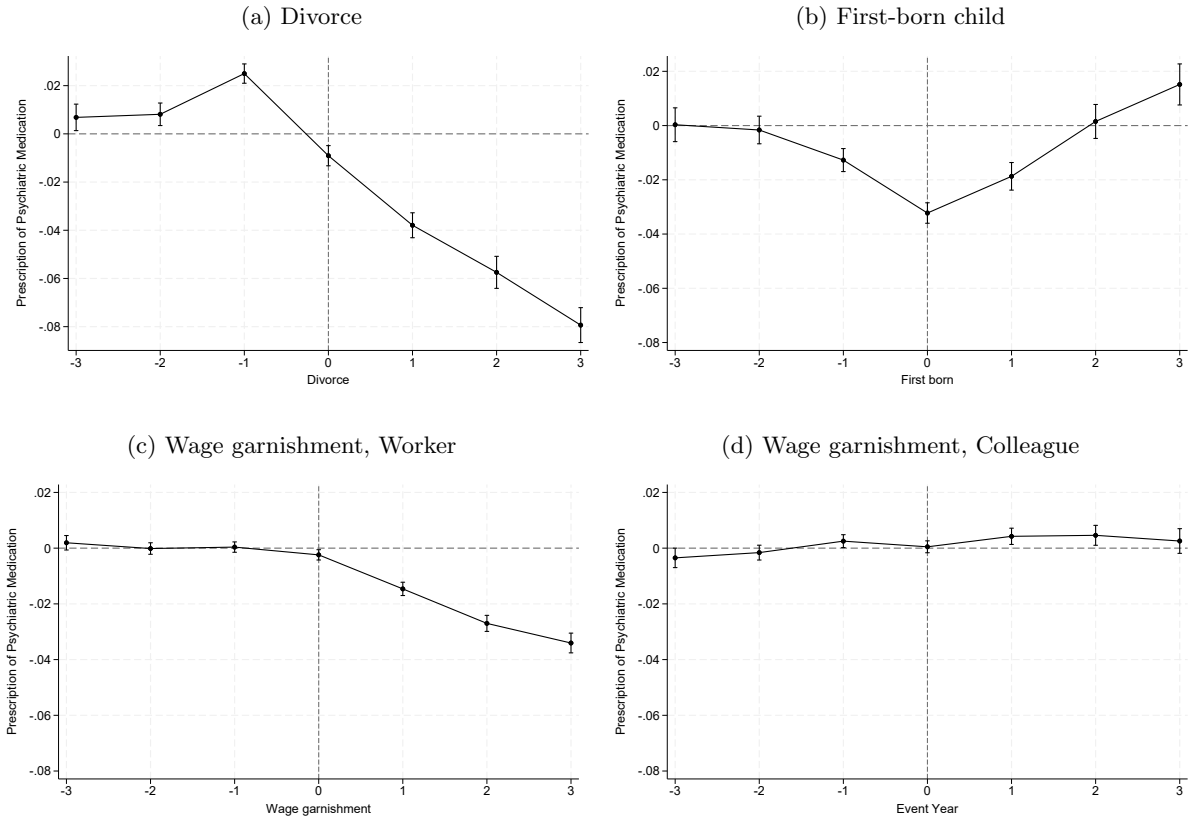
Notes: Each panel reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021), aggregated by event time relative to the year of the first wage garnishment. Estimates correspond to group–time average treatment effects aggregated by event time, where event time is defined relative to the year of an individual's first wage garnishment. Here, we look only at garnished individuals who remain employed during the full period. Points show event-time-specific treatment effects (δ_s), and vertical bars indicate 95% confidence intervals based on standard errors clustered at the individual level. Panel (a) reports contract hours, panel (b) an indicator of being on sick leave, panel (c) an indicator for being promoted, panel (d) report effects on hospital visits and panel (e) reports filled prescriptions of psychotropic medications (N05B: anxiolytics, N05C: hypnotics/sedatives, and N06A: antidepressants). The sample is restricted to workers that are employed in every period from -5 to 5.

Figure 11: Annual Dynamic Treatment Effects of Wage Garnishment on Colleagues of Garnished Workers



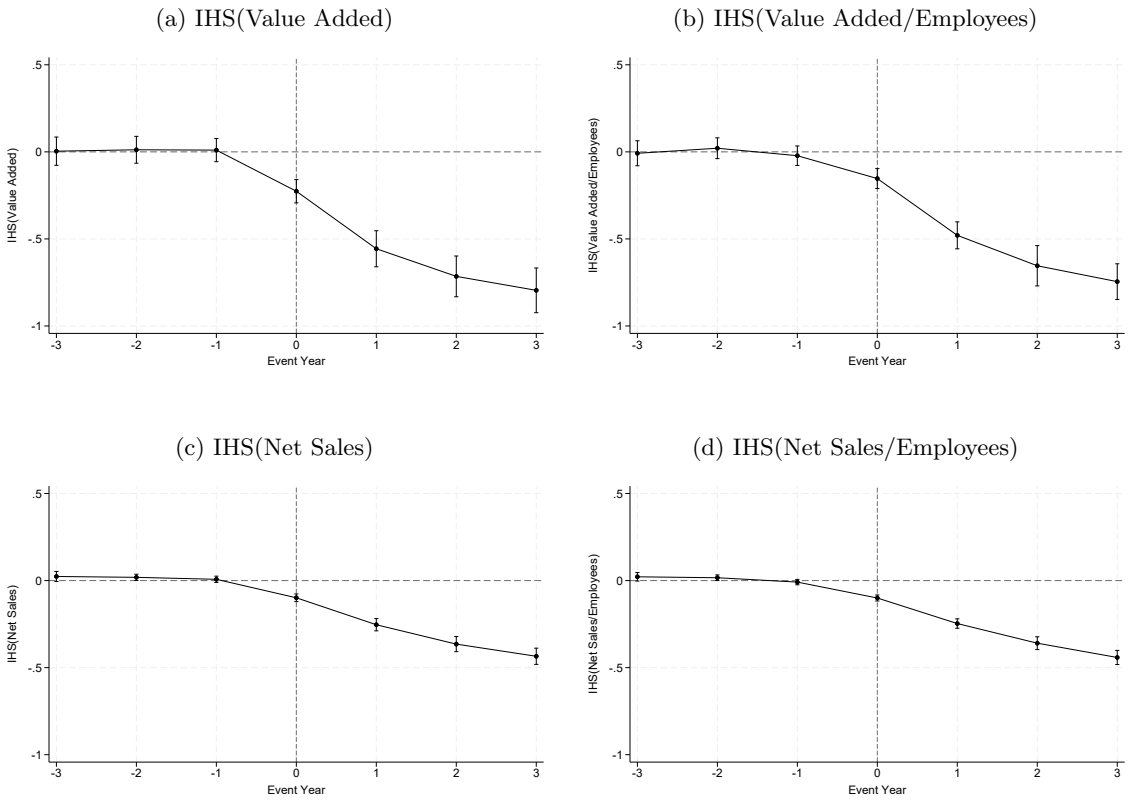
Notes: Each panel reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021), aggregated by event time relative to the year when a worker for the first time experiences that a colleague's wages are subject to garnishment. Estimates correspond to group–time average treatment effects aggregated by event time, where event time is defined relative to the year of a colleague's first wage garnishment. Here, we look only at co-workers at small firms with 10 or fewer employees. Points show event-time-specific treatment effects (δ_s), and vertical bars indicate 95% confidence intervals based on standard errors clustered at the individual level. Panel (a) reports contract hours, panel (b) an indicator of being on sick leave, panel (c) an indicator for being promoted, panel (d) report effects on hospital visits and panel (e) reports filled prescriptions of psychotropic medications (N05B: anxiolytics, N05C: hypnotics/sedatives, and N06A: antidepressants).

Figure 12: Mental Health Response of Other Events on Workers and Colleagues



Notes: Panel (a) and (b) report event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021), aggregated by event time relative to the event time of two other events; divorce and having a first-born child. The outcome measures filled prescriptions of psychotropic medications (N05B: anxiolytics, N05C: hypnotics/sedatives, and N06A: antidepressants). Panel (c) and (d) report the same outcome for the garnishment event for comparison.

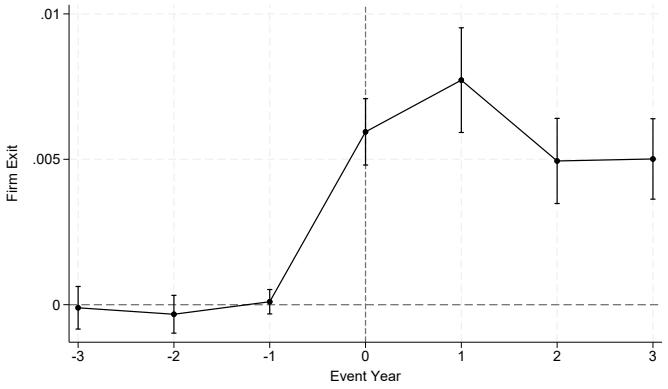
Figure 13: Annual Dynamic Treatment Effects of Wage Garnishment on Revenue and Value Creation at the Firm-Level



Notes: Each panel reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021). Estimates correspond to group–time average treatment effects aggregated by event time, where event time is defined relative to the year of a firm's first wage garnishment. Points show event-time-specific treatment effects (δ_s), and vertical bars indicate 95% confidence intervals based on standard errors clustered at the firm level. At each time t, treated firms are compared to firms who have not yet experienced wage garnishment by year t, who serve as the control group.

Figure 14: Annual Dynamic Treatment Effects of Wage Garnishment on Firm Exit

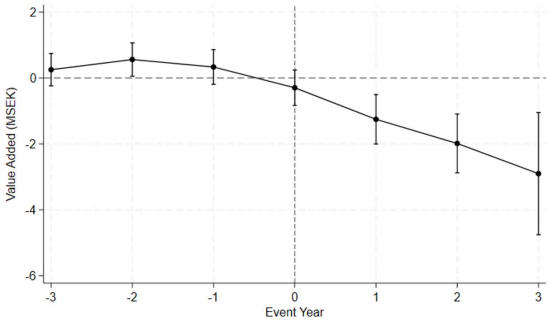
(a) Firm Exit Probability



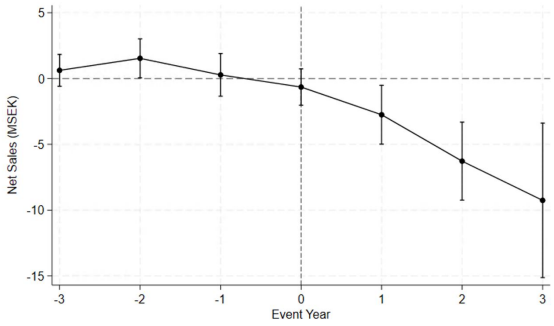
Notes: The figure reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021). Estimates correspond to group–time average treatment effects aggregated by event time, where event time is defined relative to the year of the firm's first in-sample occurrence of a garnished worker. Points show event-time-specific treatment effects (δ_s), and vertical bars indicate 95% confidence intervals based on standard errors clustered at the firm level. In each calendar year t, treated firms are compared to firms whose workers have not yet experienced wage garnishment by year t, who serve as the control group.

Figure 15: Annual Dynamic Treatment Effects of Wage Garnishment on Firm Value Added and Net Sales in Levels, Surviving Firms

(a) Value Added (MSEK)

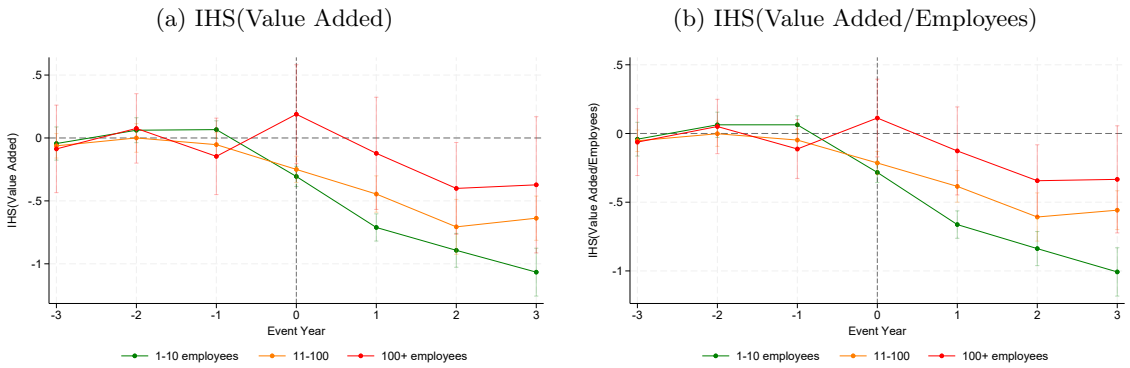


(b) Net Sales (MSEK)



Notes: Each panel reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021), aggregated by event time relative to the year of a firm's first wage garnishment. Outcomes are value added and net sales in millions of SEK (CPI 2021), winsorized at the 1st and 99th percentiles. The sample is restricted to firms observed in every year of the event window. The pre-period means of the outcomes are SEK 27 million for value added and SEK 88 million for net sales. Points show event-time-specific treatment effects (δ_s), and vertical bars indicate 95% confidence intervals based on standard errors clustered at the firm level. At each time t, treated firms are compared to firms that have not yet experienced wage garnishment by year t, who serve as the control group.

Figure 16: Heterogeneous Annual Dynamic Treatment Effects of Wage Garnishment on Firm Value Creation and Revenue, by Firm Size

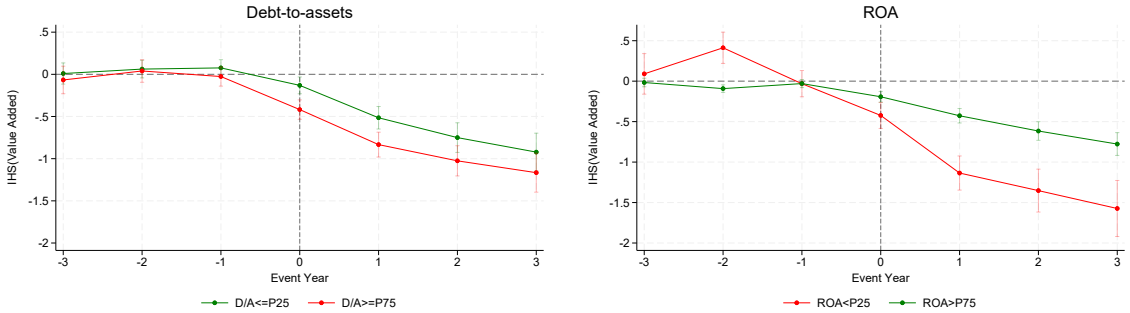


Notes: The figure reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021). We report estimates for firms based on firm size, measured as the average number of employees in t= -5 to -3.

Figure 17: Heterogeneous Annual Dynamic Treatment Effects of Wage Garnishment on Firm Value Added, by Firm Financial Health and Worker Type

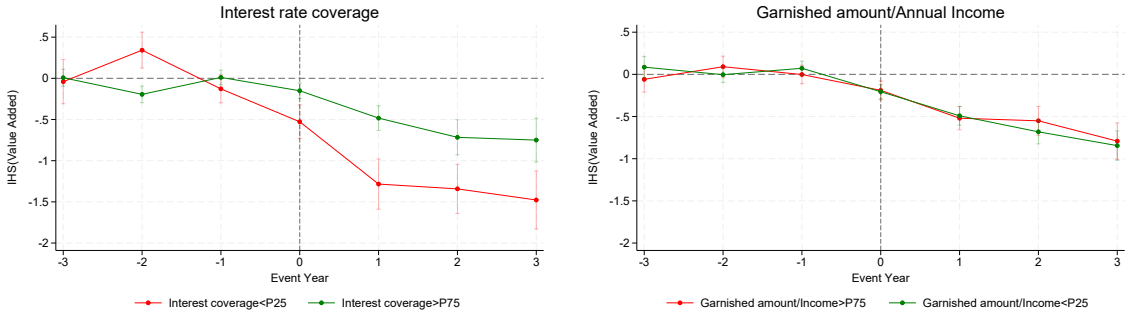
(a) Debt to assets

(b) Return on assets



(c) Interest coverage

(d) Garnished amount relative to income

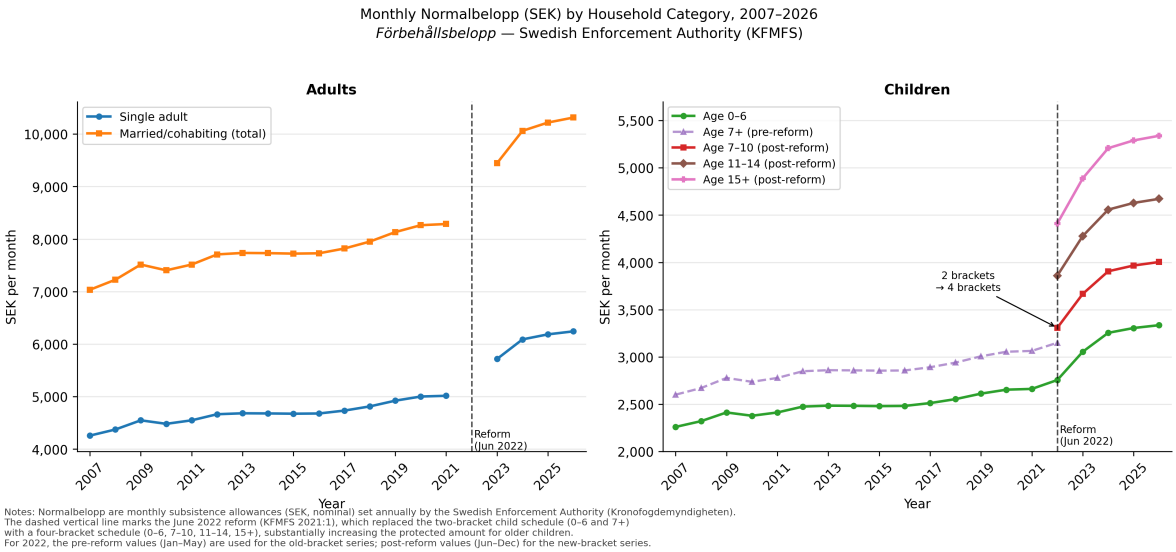


(e) Blue-collar vs. other workers



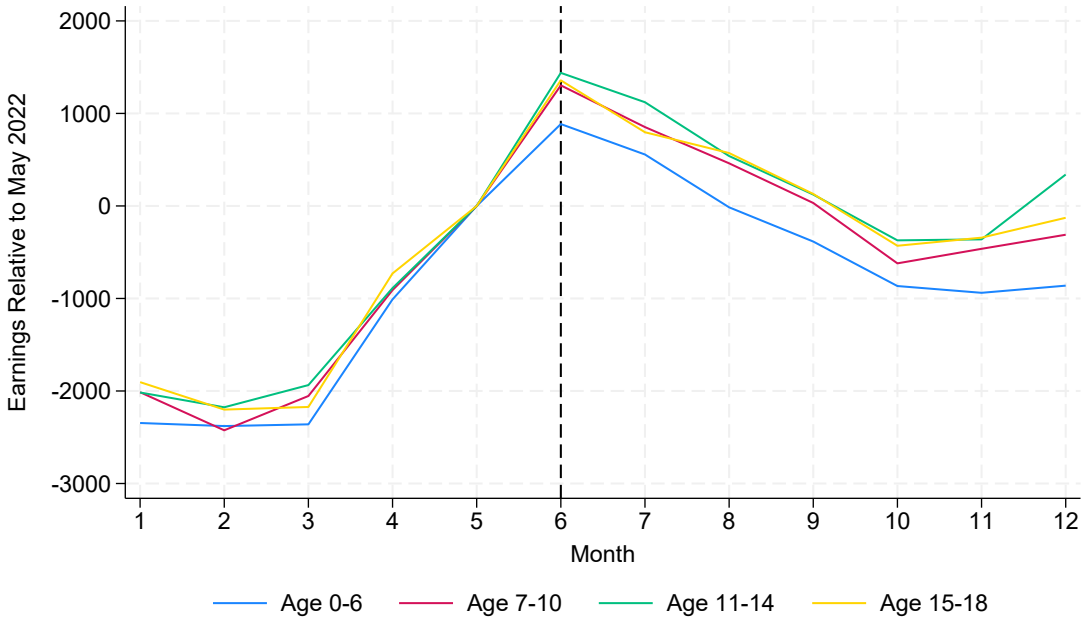
Notes: The figure reports event-study estimates for the inverse hyperbolic sine of firm value added, obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021). Panels (a)–(c) split exposed firms by three measures of pre-exposure financial health: debt to assets, return on assets, and interest coverage (EBIT over interest expenses), each averaged over $t = -5$ to $t = -3$; firms in the bottom and top quartiles are shown. Panel (d) splits firms by the garnished worker's garnished amount relative to annual income in $t - 1$ (bottom and top quartiles). Panel (e) splits firms by whether the garnished worker is a blue-collar worker or holds a higher-ranked position. Points show event-time-specific treatment effects, and vertical bars indicate 95% confidence intervals based on standard errors clustered at the firm level.

Figure 18: Monthly Normalbelopp by Household Category, 2007–2026



Notes: The figure plots monthly statutory subsistence allowances (*normalbelopp*) set annually by the Swedish Enforcement Authority (Kronofogdemyndigheten), separately for adults (left panel) and children by age group (right panel). The dashed vertical line marks the June 2022 reform (KFMFS 2021:1), which replaced the two-bracket child schedule (ages 0–6 and 7+) with a four-bracket schedule (ages 0–6, 7–10, 11–14, and 15+), substantially increasing the protected amount for families with older children. A child aged 15 or above attracted an allowance of SEK 4,411 per month under the new schedule, compared with SEK 3,150 under the old schedule—an increase of approximately 40%. Children aged 0–6 were essentially unaffected (SEK 2,736 versus SEK 2,756). The reform thus generates differential variation in the garnishment threshold across garnished workers depending on the *ages*, not merely the number, of their children.

Figure 19: Monthly Earnings Pre- and Post-Reform in 2022



Notes: Figure 19 shows monthly earnings relative to the pre-reform month, May 2022, for four different age groups.

8.2 Tables

Table 1: Summary Statistics at First Wage Garnishment

	Mean	SD
Female	0.40	0.49
Age	42.12	10.75
Higher education	0.54	0.50
Number of children in the household	0.70	1.11
Married	0.24	0.43
Annual income (SEK)	172,682.26	164,739.29
Blue-collar	0.87	0.34
Monthly garnished amount (SEK)	4,165.38	4,607.23
Duration of first garnishment (months)	14.84	22.22
More than one wage-garnishment spell	0.79	0.41
Applies for debt relief	0.08	0.27
Paid off debt	0.23	0.42
Observations	213,380	

Notes: This table reports summary statistics for all unique individuals in our annual analysis sample. The sample contains one observation per individual, corresponding to the year of the individual's first wage garnishment. Annual income is measured in 2021 SEK.

Table 2: Summary Statistics: Pre- and Post-Period

	Pre-period		Post-period	
	Mean	SD	Mean	SD
Panel A: Employees				
Disposable income (SEK)	194,316.80	212,771.72	203,349.73	146,342.73
Annual earnings (SEK)	145,217.02	159,535.57	150,068.65	163,847.71
Employment (%)	84.03	26.10	84.77	24.75
Sick leave	0.07	0.20	0.08	0.22
Changed employer	0.20	0.40	0.17	0.38
Promotion	0.01	0.09	0.01	0.08
Manager	0.01	0.10	0.01	0.09
Unemployed	0.27	0.45	0.26	0.44
Hospital visits	8.76	38.89	12.37	46.28
In/out-patient	0.11	0.31	0.14	0.35
Observations	1,053,874		1,073,084	
Unique individuals	213,220		213,380	
Panel B: Firms				
Firms with 1–10 employees				
Employees	4.99	2.73	5.14	2.74
Net sales (MSEK)	10.13	121.18	9.27	20.83
Annual result (MSEK)	0.26	4.71	0.39	18.98
Value added (MSEK)	3.41	66.09	2.99	6.09
Number of garnished employees	0.00	0.00	0.30	0.48
Observations	60,030		68,351	
Firms with 11+ employees				
Employees	50.05	172.62	106.97	479.10
Net sales (MSEK)	150.44	1,186.97	315.24	2,411.98
Annual result (MSEK)	9.04	277.69	15.73	377.05
Value added (MSEK)	42.14	273.20	83.52	563.74
Number of garnished employees	0.00	0.00	0.51	2.12
Observations	59,072		97,880	

Notes: This table reports means and standard deviations of employee- and firm-level characteristics in the pre- and post-period. The pre-period includes the five years prior to the event ($-5 \leq t \leq -1$). The post-period includes the event year and the subsequent five years ($0 \leq t \leq 5$). Firm outcomes are shown separately for firms with 1–10 employees and firms with 11 or more employees. All amounts are expressed in SEK (CPI 2021).

Table 3: Summary statistics of firms (averages, 2004–2020)

	Garnished employees		No garnished employees	
	Mean	SD	Mean	SD
Employees	45.09	(288.94)	2.26	(8.36)
Net sales (MSEK)	134.91	(1526.19)	6.13	(97.03)
Annual result (MSEK)	6.51	(181.79)	0.98	(54.04)
Value added (MSEK)	36.38	(346.15)	1.78	(22.71)
Firm Exits	0.01	(0.05)	0.02	(0.09)
Number of garnished employees	0.23	(1.33)	—	
Observations	41,620		720,325	

Notes: The table summarizes firm-level characteristics separately for firms with at least one garnished employee and those without. The sample is limited to limited liability corporations. Values represent averages over 2004–2021; standard deviations presented in parentheses. Monetary variables are measured in million SEK (CPI 2021).

Table 4: Share of Garnished Workers by Firm Size

	Mean	SD	Median	N
1–10 employees				
Share garnished workers	0.22	0.21	0.14	12,437
Number of garnished workers	1.03	0.18	1.00	12,437
11–100 employees				
Share garnished workers	0.05	0.06	0.04	9,496
Number of garnished workers	1.08	0.32	1.00	9,496
100+				
Share garnished workers	0.01	0.04	0.01	781
Number of garnished workers	1.17	0.48	1.00	781
Total				
Share garnished workers	0.14	0.18	0.08	22,714
Number of garnished workers	1.06	0.26	1.00	22,714

Notes: The table reports summary statistics by firm size category. Firm size is measured as the average number of employees between t-3 and t-5 before they experience their first wage-garnished worker between 2004 and 2021. The statistics reflect averages of the number (and share) of garnished workers across firms within each size category at that point in time.

Table 5: Industry Distribution of Firms with Garnished Workers (1-5 employees)

Industry	Freq.	Percent
Construction	23,814	19.66
Wholesale	22,836	18.85
Manufacturing	16,876	13.93
Hotel & Restaurants	13,623	11.25
Transport	13,308	10.99
Administration	7,606	6.28
Professional Services	6,198	5.12
Real Estate	3,455	2.85
Health	2,842	2.35
Other services	2,687	2.22
Entertainment	2,553	2.11
Communication	2,467	2.04
Education	1,697	1.40
Water supply	1,005	0.83
Finance	104	0.09
Electricity	44	0.04
Int. org	1	0.00
Total firm × year observations	148,462	

Notes: The table reports the industry distribution of firms employing between one and five garnished workers. Frequencies correspond to firm–year observations, and percentages reflect each industry's share of the total sample. Industries such as construction, wholesale, and manufacturing account for the largest shares of firms with garnished employees, while sectors such as finance, electricity, and international organizations represent only a negligible fraction of the sample.

Table 6: Summary Statistics by Firm Size

	All Firms		1–10 Employees			11–100 Employees		
	Mean	SD	Mean	SD	Share	Mean	SD	Share
Employment	2,423,847	(206,917)	546,493	(54,941)	22.5%	733,154	(78,692)	30.2%
Value added	2,207,491	(323,626)	357,653	(60,765)	16.2%	577,673	(97,463)	26.2%
Number of firms	388,088	(89,009)	204,239	(34,975)	52.6%	27,840	(2,931)	7.2%

Notes: The table reports averages over the study period, 2004–2020. Monetary values are measured in SEK 2021. The share columns report each size group's share of the corresponding total among all firms. Standard deviations are reported in parentheses.

Table 7: Weibull Hazard Models of First Wage Garnishment

	(1)	(2)	(3)	(4)
Female	-0.022**	-0.035***	-0.018	-0.018
	(0.011)	(0.011)	(0.012)	(0.012)
Age	0.003***	0.003***	0.002***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Married	0.003	0.004	0.004	0.005
	(0.011)	(0.011)	(0.011)	(0.011)
Foreign born	-0.036*	-0.030	-0.013	0.002
	(0.020)	(0.020)	(0.020)	(0.021)
Children	-0.014***	-0.014***	-0.016***	-0.016***
	(0.005)	(0.005)	(0.005)	(0.005)
High education	-0.061***	-0.061***	-0.056***	-0.057***
	(0.014)	(0.015)	(0.015)	(0.015)
Return on assets		-0.022	-0.014	-0.009
		(0.019)	(0.019)	(0.020)
Solidity		0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)
Debt ratio		-0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
IHS net sales		0.028***	0.022***	0.023***
		(0.002)	(0.002)	(0.003)
IHS value added		0.002	0.002	0.002
		(0.002)	(0.002)	(0.002)
$\ln(p)$	-0.181***	-0.178***	-0.177***	-0.176***
	(0.003)	(0.003)	(0.003)	(0.003)
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	No
Industry×Year FE	No	No	No	Yes
Observations	1,277,116	1,277,116	1,277,116	1,277,116

Notes: Weibull proportional hazard models for the duration between the first debt-collection decision and the first wage-garnishment decision. Positive coefficients imply a higher hazard (faster entry into garnishment). All specifications include year and municipality fixed effects; columns (3)–(4) additionally include industry fixed effects and industry-by-year fixed effects, respectively. Controls are measured one year prior to the debt-collection decision. The Weibull log shape parameter, $\ln(p)$, is reported at the bottom of the table. Values below zero imply $p < 1$, indicating a declining hazard rate over time. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Effect of Wage Garnishment on Monthly Outcomes

	Employed in $t-1$		Always employed
	Earnings (SEK)	Earnings (IHS)	Earnings (IHS)
ATT	-1,244.997***	-0.091***	-0.065***
	(61.642)	(0.003)	(0.003)
Mean, $t-1$	25,797	–	–
Observations	944,044	935,741	650,942
Clusters	62,353	62,344	40,447

Notes: The table reports staggered difference-in-differences estimates. Cohort-specific estimates are aggregated following Callaway and Sant'Anna (2021), ensuring that treated individuals are compared only to not-yet-treated individuals at the same event time. Coefficients represent the average change in monthly individual outcomes after first exposure to a garnishment. Standard errors are clustered at the individual level. The table reports average treatment effects (ATT) on individual outcomes in the months after first garnishment. Standard errors in parentheses. Observations refer to individual-month cells used in estimation. Clusters refers to the individual clusters.

Table 9: Effect of Wage Garnishment on Annual Outcomes

	Employed in $t-1$			Always Employed								
	IHS(Annual Earnings)	IHS(Capital Income)	Entrepreneur	IHS(Annual Earnings)	IHS(Annual Earnings), Non-Divorced	IHS(Annual Earnings), No Health Shock	IHS(Capital Income)	Entrepreneur	Employment	Sick Leave	Promotion	In/Out Patient
ATT	-3.543***	0.656***	0.008***	-0.148***	-0.149***	-0.135***	0.797***	-0.003***	-0.010***	0.005***	-0.004***	-0.005***
	(0.013)	(0.018)	(0.001)	(0.004)	(0.005)	(0.004)	(0.026)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Mean, $t-1$			0.017					0.011	0.868	0.038	0.015	0.065
Observations	1,383,289	1,383,289	1,383,289	650,675	533,244	508,401	650,675	650,675	273,272	650,675	650,675	650,675
Clusters	175,314	175,314	175,314	85,364	70,771	66,842	85,364	85,364	50,386	85,364	85,364	85,364

Notes: This table reports average treatment effects (ATT) from the staggered adoption design. The sample consists of garnished workers between 2004 and 2021, where the control group consists of not-yet-treated workers. Columns (1)–(3) restrict the sample to workers employed in the year before first wage garnishment, while columns (4)–(13) restrict the sample to workers who remain employed throughout the event window. Column (5) restricts the sample to workers who do not divorce during the event window, and column (6) restricts the sample to workers who do not experience a health shock. Income outcomes are expressed in inverse hyperbolic sine (IHS) units. Entrepreneur is an indicator for entrepreneurship. Standard errors are clustered at the individual level and reported in parentheses. Observations refer to individual-year cells. Clusters refers to the number of individual clusters. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Effect of Wage Garnishment on Household and Spouse Income

Garnished worker:	Employed in $t-1$			Always Employed		
	IHS(Household Disposable Income)	IHS(Spouse Annual Earnings)	IHS(Spouse Annual Earnings), Spouse Not Garnished	IHS(Household Disposable Income)	IHS(Spouse Annual Earnings)	IHS(Spouse Annual Earnings), Spouse Not Garnished
ATT	-0.357***	-0.324***	-0.187***	-0.032***	-0.060*	0.029
	(0.004)	(0.027)	(0.030)	(0.002)	(0.036)	(0.041)
Observations	1,383,289	445,432	364,299	650,675	228,103	185,611
Clusters	193,993	62,073	50,795	85,364	29,760	24,289

Notes: This table reports average treatment effects (ATT) from the staggered adoption design. The sample consists of garnished workers between 2004 and 2021, where the control group consists of not-yet-treated workers. Columns (1)–(3) restrict the sample to workers employed in the year before first wage garnishment, while columns (4)–(6) restrict the sample to workers who remain employed throughout the event window. In columns (2), (3), (5), and (6), the outcome variable is the annual earnings of the spouse; the sample consists of spouses who were married to or cohabiting with the garnished worker, with common children, in $t-1$. Columns (3) and (6) further restrict the sample to spouses who are not themselves subject to wage garnishment at $t=0$. All outcomes are expressed in inverse hyperbolic sine (IHS) units. Standard errors are reported in parentheses. Observations refer to individual-year cells. Clusters refers to the number of individual clusters. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 11: Effect of Wage Garnishment on the Probability of Having No Wage Income

	Employed in $t-1$	
	Annual data	Monthly data
ATT	0.280***	0.016***
	(0.001)	(0.000)
Mean, $t-1$	0	0
Observations	1,383,289	944,044
Clusters	175,314	62,353

Notes: The table reports staggered difference-in-differences estimates of the effects of first exposure to wage garnishment on the probability of having no wage income. The sample is restricted to individuals who are employed in $t-1$. Column (1) measures the outcome at the annual level, while column (2) measures the outcome at the monthly level. Cohort-specific estimates are aggregated following Callaway and Sant'Anna (2021), ensuring that treated individuals are compared only to not-yet-treated individuals at the same event time. The reported ATT is the average treatment effect in post-treatment periods. No wage income is an indicator equal to one if the individual has zero wage income in the relevant year or month. Standard errors are clustered at the individual level and reported in parentheses. Observations refer to individual-year cells in column (1) and individual-month cells in column (2). Clusters are the number of individual clusters in each regression.

Table 12: Effects on Colleagues

	In/out-patient	Sick-leave	Contract hours	Promotion	Prescriptions
ATT	0.003***	0.009***	0.092***	0.006***	0.001
	(0.001)	(0.000)	(0.008)	(0.001)	(0.001)
Mean, $t-1$	0.027	0.014	0.737	0.010	0.078
Observations	1,311,060	1,233,215	46,398	1,211,741	692,697
Clusters	163,551	159,594	15,187	160,456	

Notes: The table reports staggered difference-in-differences estimates of the effects of first exposure of a colleague subject to wage garnishment. The sample is restricted to colleagues employed at the same firm as the garnished worker in $t-1$. Cohort-specific estimates are aggregated following Callaway and Sant'Anna (2021), ensuring that treated individuals are compared only to not-yet-treated individuals at the same event time. The reported ATT is the average treatment effect in post-treatment periods. Standard errors are clustered at the individual level and reported in parentheses. Observations refer to individual-year cells.

Table 13: Effect of Wage Garnishment on Firm Outcomes

	All firms (1+ employee)							1-10 employees	11-100 employees
(IHS)	Value Added (IHS)	Net Sales (IHS)	Value Added	Net Sales /Employees (IHS)	Net Sales /Employees (IHS)	Value Added (MSEK)	Firm (MSEK)	Value Added Exits	Value Added (IHS)
ATT	-0.577***	-0.278***	-0.509***	-0.272***	-3.473***	-0.975***	0.006***	-0.659***	-0.451***
	(0.027)	(0.012)	(0.027)	(0.010)	(0.355)	(0.106)	(0.000)	(0.044)	(0.045)
Mean, $t-1$	–	–	–	–	47.088	14.869	0.0004		
Observations	259,752	259,752	167,692	167,692	257,808	255,265	259,752	89,390	71,973
Clusters	40,993	40,993	23,243	23,243	40,789	40,559	40,993	12,763	9,672

Notes: The table reports staggered difference-in-differences estimates. Cohort-specific estimates are aggregated following Callaway and Sant'Anna (2021), ensuring that treated firms are compared only to not-yet-treated firms at the same event time. Coefficients represent the average change in annual firm outcomes after first exposure to a garnished employee. Standard errors are clustered at the firm level. Outcomes are in inverse hyperbolic sine units. Mean in t-1 presents the mean of the outcome in the year before the event (only for indicator variables). Observations refer to firm-year cells. Clusters refers to the number of firm clusters.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 14: Average Treatment Effects by Sector and Occupation

Outcome:	Firms		Workers, monthly	
	IHS(Value Added)		IHS(Monthly Earnings)	
	ATT	Observations	ATT	Observations
Panel A. Sector				
Administration	-0.565***	15,783	-0.164***	100,650
	(0.083)		(0.017)	
Communication	-0.885***	6,930	-0.154***	11,109
	(0.265)		(0.058)	
Construction	-0.487***	42,508	-0.169***	84,729
	(0.042)		(0.025)	
Education	-0.174	4,164	-0.111	10,185
	(0.206)		(0.079)	
Electricity	0.132	497	-0.256	991
	(0.882)		(0.175)	
Entertainment	-0.570*	4,113	-0.156*	5,266
	(0.315)		(0.082)	
Finance	2.794	134	0.119	126
	(4.076)		(0.126)	
Health	-0.686***	9,565	-0.171***	57,725
	(0.101)		(0.019)	
Hotel and Restaurants	-0.613***	23,613	-0.158***	56,160
	(0.086)		(0.023)	
Manufacturing	-0.360***	50,641	-0.081***	89,907
	(0.059)		(0.019)	
Other services	-0.698***	2,819	-0.214***	7,328
	(0.235)		(0.071)	
Professional services	-0.683***	11,654	-0.130***	19,933
	(0.157)		(0.050)	
Real Estate	-0.474	5,246	-0.069	8,472
	(0.341)		(0.053)	
Transport	-0.506***	24,896	-0.147***	58,537
	(0.067)		(0.025)	
Water supply	-0.415***	2,887	-0.240**	4,641
	(0.110)		(0.105)	
Wholesale	-0.408***	48,153	-0.118***	92,011
	(0.069)		(0.018)	
Panel B. Occupation				
			ATT	Observations
Agriculture and Forestry			-0.057	10,802
			(0.069)	
Associate Professionals			-0.096***	57,399
			(0.028)	
Clerical Support			-0.119***	74,622
			(0.020)	
Construction			-0.130***	116,545
			(0.021)	
Elementary Occupations			-0.145***	125,698
			(0.017)	
Managers			-0.107*	16,756
			(0.061)	
Manufacturing			-0.098***	97,578
			(0.021)	
Professionals			-0.141***	57,779
			(0.025)	
Service and Sales			-0.152***	327,215
			(0.010)	

Notes: The table reports average treatment effects (ATT). Standard errors are shown in parentheses. The employee results are measured at the worker-month level; the firm results are measured at the firm level using annual data. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 15: Summary Statistics by Child Age Group and Reform Period

	Pre-reform				Post-reform			
	Age 0–6	Age 7–10	Age 11–14	Age 15–18	Age 0–6	Age 7–10	Age 11–14	Age 15–18
Monthly earnings (SEK)	25,043	25,483	26,342	27,427	26,159	26,972	28,023	28,873
	(14,809)	(13,525)	(14,280)	(16,626)	(14,105)	(14,409)	(27,718)	(15,062)
Protected amount (SEK)	11,501	11,900	12,156	12,138	11,693	12,097	12,439	12,544
	(8,503)	(8,451)	(8,752)	(8,642)	(8,375)	(8,628)	(8,768)	(8,712)
Garnishment amount (SEK)	6,375	6,214	6,429	6,560	6,182	6,025	6,030	5,999
	(6,805)	(6,364)	(6,801)	(6,981)	(6,650)	(6,375)	(7,440)	(6,817)
Age	34.4	37.9	42.6	47.2	34.4	37.9	42.6	47.2
	(7.29)	(7.83)	(7.94)	(7.29)	(7.29)	(7.84)	(7.96)	(7.31)
Female	0.37	0.39	0.41	0.43	0.37	0.39	0.41	0.43
	(0.48)	(0.49)	(0.49)	(0.50)	(0.48)	(0.49)	(0.49)	(0.50)
Higher education	0.11	0.13	0.15	0.16	0.11	0.13	0.15	0.16
	(0.32)	(0.34)	(0.36)	(0.37)	(0.32)	(0.34)	(0.35)	(0.37)
Observations	25,416	47,312	64,997	95,669	36,176	67,151	92,525	135,712
Unique observations	5,941	11,031	15,075	22,202	5,941	11,031	15,075	22,202

Notes: The table reports summary statistics for workers subject to wage garnishment in 2022. Individuals are grouped by the age of their oldest child, which determines the reform-induced increase in the protected income threshold. The pre-reform period covers January–May 2022, while the post-reform period covers June–December 2022. The number of observations show the number of month-individual observations per age group. Unique observations show the number of individuals with children in the age group in 2022. Standard deviations are reported in parentheses.

Table 16: Reduced Form: Effect of the Reform on Monthly Earnings

	Binary Treatment (Child Age ≥ 7)	
	(1)	(2)
Post × Treated	399.694***	572.460***
% of pre-reform mean*	1.50%	2.15%
Sample	Full	Above threshold
Observations	434,354	374,174
Worker FE	Yes	Yes
Month FE	Yes	Yes

Notes: The outcome is monthly wage earnings. The sample consists of individuals with children below age 18 who were subject to wage garnishment in 2022. The reform increased the protected income threshold in June 2022. Columns (1)–(2) use a binary treatment indicator equal to one if the individual has a child aged 7–18 prior to the reform. In column (2) we restrict the sample to only include individuals above the threshold. All specifications include individual and time fixed effects. *The pre-reform mean is 26,641 for the treated group. Standard errors clustered at the individual level.

Appendix

A Institutional Background

Table A.1: Monthly Normalbelopp by Household Category, 2007–2026

Year	Single adult	Married/ cohabiting[a]	*Pre-reform child brackets* Age 0–6	*Pre-reform child brackets* Age 7+[†]	*Post-reform child brackets* Age 0–6	*Post-reform child brackets* Age 7–10	*Post-reform child brackets* Age 11–14	*Post-reform child brackets* Age 15+
2007	4,258	7,035	2,260	2,601	–	–	–	–
2008	4,374	7,226	2,321	2,671	–	–	–	–
2009	4,549	7,515	2,413	2,778	–	–	–	–
2010	4,482	7,405	2,378	2,737	–	–	–	–
2011	4,549	7,515	2,413	2,778	–	–	–	–
2012	4,665	7,708	2,475	2,849	–	–	–	–
2013	4,683	7,736	2,485	2,860	–	–	–	–
2014	4,680	7,732	2,483	2,858	–	–	–	–
2015	4,675	7,723	2,480	2,855	–	–	–	–
2016	4,679	7,729	2,482	2,857	–	–	–	–
2017	4,734	7,820	2,512	2,891	–	–	–	–
2018	4,814	7,952	2,554	2,940	–	–	–	–
2019	4,923	8,133	2,612	3,007	–	–	–	–
2020	5,002	8,264	2,654	3,055	–	–	–	–
2021	5,016	8,287	2,662	3,064	–	–	–	–
Transition year 2022: pre-reform schedule (Jan–May) / post-reform schedule (Jun–Dec)								
2022[b] (Jan–May)	5,157	8,520	2,736	3,150	–	–	–	–
2022[b] (Jun–Dec)	5,158	8,520	–	–	2,756	3,308	3,860	4,411
Post-reform schedule (full year)								
2023	5,717	9,445	–	–	3,055	3,667	4,279	4,889
2024	6,090	10,061	–	–	3,255	3,906	4,558	5,208
2025	6,186	10,219	–	–	3,306	3,967	4,629	5,290
2026	6,243	10,314	–	–	3,336	4,004	4,672	5,339

Notes: Monthly normalbelopp in SEK (nominal), set annually by the Swedish Enforcement Authority (Kronofogdemyndigheten) under KFMFS regulations. The normalbelopp is the statutory subsistence allowance used to determine the protected income threshold (*förbehållsbelopp*) below which wages cannot be garnished.

[a] Total allowance for a cohabiting couple; each partner's individual component is half this amount (SEK 5,110 per person in 2025).

[†] The pre-reform child bracket "Age 7+" applies from the calendar year the child turns 7.

[b] KFMFS 2021:1 introduced a four-bracket child schedule effective 1 June 2022, replacing the two-bracket schedule. The new brackets substantially increase the protected amount for older children: a child aged 15 or above received SEK 4,411 per month under the new schedule, compared to SEK 3,150 under the old schedule — an increase of approximately 40%. The reform is used in this paper to construct exogenous variation in garnishment intensity across garnished workers with different numbers and ages of children.

Sources: KFMFS 2006:1 through KFMFS 2024:1 (Kronofogdemyndighetens författningssamling).

B A Static Benchmark for Wage Garnishment and Labor Supply

This appendix provides a deliberately static illustration of the immediate incentive distortion created by wage garnishment. The framework isolates the kink in current disposable income at the protected threshold and its implications for effort and career investments. It abstracts from debt dynamics, including the fact that additional repayments reduce outstanding debt and can shorten the duration of garnishment. It is therefore not intended to characterize heterogeneity in responses by debt burden, income, or repayment horizon.

Setup

A worker chooses effort $e \geq 0$ to produce earnings $w(e)$, with $w'(e) > 0$ and $w''(e) < 0$, and incurs a convex cost $c(e)$ with $c'(e) > 0$ and $c''(e) > 0$. Absent garnishment, the worker solves $\max_e w(e) - c(e)$, yielding the efficient effort level e^* defined by $w'(e^*) = c'(e^*)$.

Garnishment as a kinked tax on effort

Under wage garnishment, every krona earned above the protected threshold $\bar{w}$ (the *förbehållsbelopp*) is seized. Disposable income becomes $d(e) = \min\{w(e), \bar{w}\}$, and the worker solves

$$\max_e \ \min\{w(e),\, \bar{w}\} - c(e). \tag{3}$$

Let $\bar{e}$ denote the effort level satisfying $w(\bar{e}) = \bar{w}$. The marginal net return to effort is $w'(e) - c'(e)$ for $e < \bar{e}$ and $-c'(e) < 0$ for $e > \bar{e}$: effort above $\bar{e}$ is strictly dominated, since additional earnings are fully seized while effort remains costly.

Bunching at the threshold

Within this static benchmark, workers whose unconstrained optimum lies above the threshold bunch at it. If $e^* < \bar{e}$, the worker is unaffected and chooses $e^g = e^*$. If $e^* \geq \bar{e}$, the marginal net return to effort is positive up to $\bar{e}$ and strictly negative beyond, so the worker optimally chooses $e^g = \bar{e}$, earning exactly $\bar{w}$. This implication provides a simple benchmark for interpreting the bunching patterns documented in Figure 4: realized repayments cluster sharply at the statutory minimum

deduction, and the distribution of earnings exhibits a spike at the threshold.

Career effects: promotions and search

The same kink implies that workers bunching at $\bar{e}$ optimally forgo costly career investments. Consider a promotion that raises the wage schedule from $w(\cdot)$ to $w^P(\cdot) > w(\cdot)$ but requires incremental effort Δe. A worker at the threshold earns $\bar{w}$ either way, so the net payoff to promotion effort is $-c'(\bar{e})\Delta e < 0$: the worker bears the full effort cost with no gain in disposable income. The same logic applies to on-the-job search for a higher-paying position: any wage premium Δw is fully garnished, so search effort at cost $\kappa(s) > 0$ yields a strictly negative net return. Within the static benchmark, the same logic makes costly investments in promotion or job search less attractive when the resulting increase in current earnings is fully garnished. This provides a simple incentive rationale for examining career progression in the empirical analysis. Employer responses to garnishment may, of course, reinforce these effects.

The role of full recourse

Full recourse makes the static benchmark incomplete in one important respect. Unlike in a setting in which debt can be discharged through default (Donaldson et al., 2019), additional earnings under Swedish wage garnishment reduce outstanding debt and may bring forward the end of garnishment. The value of this future benefit depends on the worker's repayment horizon and is greater when outstanding debt is small relative to repayment capacity. The static benchmark abstracts from this dynamic consideration and isolates only the immediate effect of garnishment on the return to additional earnings. The repayment-horizon mechanism is discussed in Section 1 and examined empirically through heterogeneity by debt relative to income.

Predictions across workers and firms

One implication of the static benchmark is particularly useful for the reform analysis. A change in the protected threshold should affect current work incentives primarily for workers whose earnings are close to or above that threshold. This observation motivates the interacted specifications in Section 6, which allow the reform response to vary with pre-reform earnings relative to the protected threshold.

C Avoidance: Other Margins of Response

Garnishment is enforced through the employer and applies to wage income. The same incentive that leads workers to reduce earnings may therefore also lead them to move resources into forms that garnishment does not reach: capital income, business income, undeclared work, or the earnings of other household members. These responses are not a threat to our findings; they are additional consequences of enforcement, and each reduces creditor recovery without any offsetting gain in production. Having documented the effects on workers, coworkers, and firms, we now document these responses and ask how large they are relative to the labor supply response.

Capital income and business income

Figure C.1 plots event-study estimates for the inverse hyperbolic sine of capital income and for an indicator of having positive business income; columns (2), (3), (8), and (9) of Table 9 report the corresponding average effects.[18] Capital income is flat before onset and then rises steadily, reaching roughly 1.2 IHS units three years after garnishment begins in both samples (Panels (a) and (b)). Because capital income is zero for most garnished workers before onset, an increase of this size is consistent with a modest share of workers moving from zero to small positive amounts, and it need not indicate large sums; capital income also includes realized capital gains, so part of the increase may reflect asset sales used to repay debt rather than income shifting. The share of workers with positive business income rises among workers employed in the year before garnishment, from zero to 2.3 percentage points by year three against a pre-period mean of 1.7% (Panel (c)), but does not increase among always-employed workers (Panel (d)). Workers who leave wage employment after garnishment thus disproportionately start reporting business income, which is not withheld at source, whereas workers who stay in their jobs do not take up side businesses. The magnitude, however, is small relative to the labor supply response: the 0.8 percentage point average increase compares with a 28 percentage point increase in the probability of having no wage income (Table 11). Since the indicator counts any positive business income, this is an upper bound on entry into self-employment proper, and it can account for only a small fraction of the exits from wage

[18] Business income is income from business activity (*inkomst av näringsverksamhet*) as reported in the income and tax register (IoT). The indicator equals one if this income is positive in a given year, so it captures any self-employment or sole-proprietorship activity, including small side activities, and not only workers whose main income comes from a business.

employment.

Household income

Garnishment also changes the incentives of the debtor's spouse. For a cohabiting debtor, the SEA computes the protected amount jointly: the household's common costs (the couple's and children's *normalbelopp* plus housing) are split in proportion to the partners' net incomes, and the debtor keeps only their share. Every krona earned by the spouse thus lowers the debtor's protected amount and raises the amount garnished, so garnishment effectively taxes the spouse's earnings as well. Table 10 shows that spouses respond: among workers employed in the year before garnishment, the earnings of spouses who are not themselves garnished fall by 0.19 IHS units, and household disposable income falls by 0.36 IHS units. Among always-employed workers the spouse response is small and insignificant, and household disposable income falls by 0.03 IHS units. The household as a whole therefore loses income; the spouse's response adds to the debtor's rather than offsetting it.[19]

Undeclared work

Garnished workers may also move earnings off the books. We cannot observe informal income directly, but we can ask whether the decline in reported earnings is larger where undeclared work is easier. Panel (d) of Figure 8 splits the monthly earnings response by whether the worker is employed in a sector the Swedish Tax Agency classifies as high-risk for undeclared work (restaurants, hairdressing and beauty care, laundry services, vehicle repair, and wholesale of food and beverages). The earnings declines are similar in high-risk and other sectors, so substitution toward informal work is unlikely to be the main driver of the decline in reported earnings, although we cannot rule out that it contributes in both groups.

Implications

Garnished workers do shift some resources beyond the reach of enforcement, through capital income, business income among those who leave wage employment, and reduced spousal earnings. None

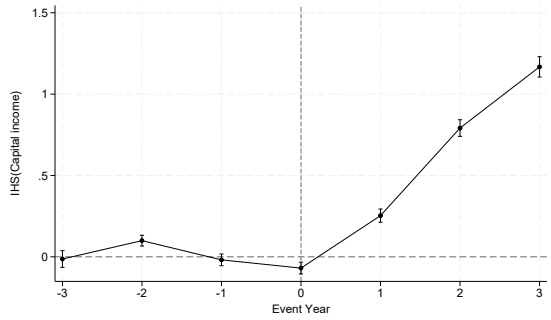
[19] The same rule implies that separation restores the debtor's single-household protected amount, which is one reason divorce may respond to garnishment; Section 4.1 shows that our estimates are unchanged when divorcing workers are excluded.

of these margins is large enough to account for the earnings decline, and none can produce the coworker and firm results documented above: the increase in coworkers' hours, sick leave, and hospital contacts (Section 5.2.2) and the decline in firm output (Section 5.3) require that garnished workers actually withdraw labor from the firm. Avoidance is instead a further cost of enforcement, borne by creditors, that comes on top of the production losses we document.

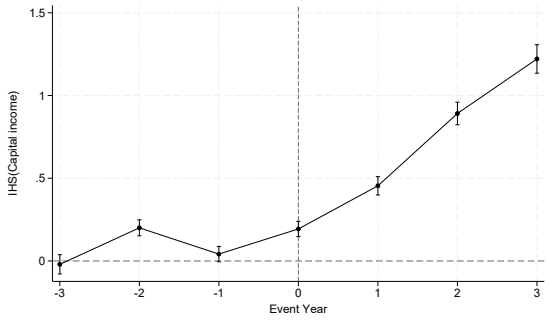
The effects documented so far combine two channels: workers may supply less labor because additional earnings are seized, and employers, who are notified of the garnishment, may respond by entrusting garnished workers with fewer tasks, hours, or promotions. The next section separates these channels.

Figure C.1: Annual Dynamic Treatment Effects of Wage Garnishment on Capital Income and Business Income

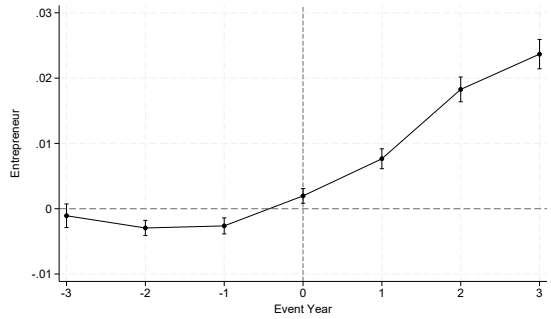
(a) IHS(Capital income), employed in t-1



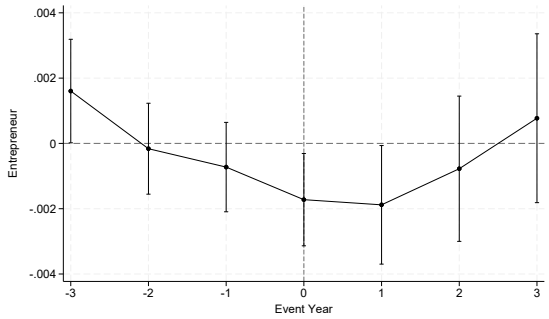
(b) IHS(Capital income), always employed



(c) Business income > 0, employed in t-1



(d) Business income > 0, always employed



Notes: Each panel reports event-study estimates obtained using the staggered-adoption difference-in-differences estimator of Callaway and Sant'Anna (2021), aggregated by event time relative to the year of an individual's first wage garnishment. Panels (a) and (c) restrict the sample to individuals employed in the year before garnishment; Panels (b) and (d) restrict the sample to individuals who remain employed throughout the event window. Panels (a) and (b) report the inverse hyperbolic sine of annual capital income; Panels (c) and (d) report an indicator for positive income from business activity (*inkomst av näringsverksamhet*, from the income and tax register). Points show event-time-specific treatment effects (δ_s), and vertical bars indicate 95% confidence intervals based on standard errors clustered at the individual level. In each calendar year t, treated individuals are compared to individuals who have not yet entered wage garnishment by year t, who serve as the control group.