UNEMPLOYMENT INSURANCE GENEROSITY AND HEALTHCARE USE: EVIDENCE FROM SWEDEN*

Miika Päällysaho†

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Abstract

Unemployment often leads to worse health and greater healthcare use, which can create sizable fiscal externalities through publicly financed healthcare systems. However, little is known about whether unemployment insurance (UI) mitigates these costs. I study how the generosity of UI affects recipients' healthcare use using Swedish administrative data. My measure of healthcare use captures inpatient and outpatient visits as well as prescription drug purchases, reflecting total system costs rather than out-of-pocket expenses. Exploiting caps in the daily benefit amount in a regression kink design, I find little evidence that more generous unemployment benefits affect the total costs of recipients' healthcare use. This conclusion holds across gender and age groups, types of healthcare use, and among individuals with and without a partner. In response to a 1 SEK increase in unemployment benefits, estimates from my preferred specification can rule out changes in total healthcare costs greater than 0.08 SEK during the first 40 weeks since the start of the unemployment spell. The results suggest that in the Swedish context, characterized by generous social insurance and publicly funded healthcare, modest adjustments to UI generosity do not affect the public health costs of unemployment.

Keywords: administrative data, healthcare, regression kink design, social insurance
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†VATT Institute for Economic Research (Helsinki, Finland). Email: miika.paallysaho@vatt.fi.

1 Introduction

An extensive literature in the social sciences documents that job loss and unemployment are stressful events detrimental to mental and physical health.[1] These adverse health consequences are costly not only for the affected individuals, but they can also generate fiscal externalities through increased healthcare use. One of the rationales for social insurance programs, such as unemployment insurance (UI), is to financially support individuals facing adverse shocks such as unemployment (Chetty and Finkelstein 2013; Diamond 1977).

Although financial resources are strongly associated with health[2], evidence on whether UI can alleviate the negative health impacts of unemployment is still scarce. Knowing how UI affects healthcare use sheds light on whether the negative health impacts mainly reflect the income loss after job loss, or whether they reflect factors that affect independently of income, such as social stigma or the loss of social contacts and identity (as emphasized by e.g., Jahoda 1982). Any health-related fiscal externalities alleviated by UI should also be considered when determining the optimal level of UI (e.g., Chetty 2006). These fiscal externalities could be large because individuals typically only pay a small share out of pocket of the total costs of the healthcare they use.[3]

In this paper, I study how the generosity of UI affects benefit recipients' healthcare use using Swedish administrative data. Using variation in the generosity of unemployment benefits created by benefit caps in a regression kink design, I find little evidence that more generous unemployment benefits affect healthcare use. This finding is robust to different specification choices, and I find little heterogeneity in the effects across socioeconomic groups and types of healthcare use.

My analysis builds on individual-level register data on unemployment spells, unemployment benefit payments, and healthcare use. The estimation sample consists of around 340,000 unemployment spells starting between March 2005 and July 2014. For each spell, I match information on weekly unemployment benefit payments and detailed information on inpatient and outpatient care visits and prescription drug purchases for the first 40 weeks since the start of the unemployment spell.

1. For example, see Jahoda (1982), Dooley et al. (1996), Sullivan and Von Wachter (2009), Wanberg (2012), Marcus (2013), Brand (2015), Schiele and Schmitz (2016), and Gathmann et al. (2025). See Picchio and Ubaldi (2023) for a meta-analysis of the literature on health and unemployment.

2. For reviews, see Cutler et al. (2012) and Lleras-Muney et al. (2025).

3. For example, in 2016, out-of-pocket costs paid by households in OECD countries accounted for 6% of total inpatient care expenses, 18% of outpatient care expenses, and 25% of prescription drug expenditures (OECD 2019, Figure 2).

My primary outcome measure is the total costs of healthcare use, which captures the costs of inpatient and outpatient care visits and prescription drug purchases. For drug purchases, I observe out-of-pocket costs and total costs, including costs covered by prescription drug insurance. For inpatient and outpatient care visits, I measure the total costs of a visit by combining information on the length and the Major Diagnostic Category (MDC) of the visit with data on the national average per-day costs of inpatient and outpatient visits for each MDC. The cost measure aims to capture the full costs of the resources used (medications, materials, operations, etc.) during the visit as well as underlying costs such as personnel and administrative costs.

To obtain exogenous variation in the generosity of unemployment benefits, I use a regression kink (RK) design that exploits caps in the amount of daily benefits. This non-linear policy rule creates a kink in the relationship between daily benefits and the pre-unemployment daily wage at the point where the individual reaches the benefit cap. The benefit cap was fairly low during my study period, so that roughly only one in four in my estimation sample has a daily wage below the kink point. Provided that individuals on both sides of the kink point are similar in terms of other determinants of healthcare use, I can attribute any kinks in the relationship between healthcare use and the daily wage to a causal effect of unemployment benefits on healthcare use. I provide evidence in favor of this assumption by showing that pre-determined covariates, predicted healthcare use, healthcare use before unemployment, and the density of the daily wage evolve smoothly around the kink point.

My main finding is that the generosity of unemployment benefits has little effect on healthcare use. The estimates are precise: over the first 40 weeks since the start of the unemployment spell, the 95 percent confidence intervals for my preferred specification can rule out changes in the total costs of healthcare use greater than 0.08 SEK per a 1 SEK increase in unemployment benefits.

The estimates are similarly precise when looking at different margins of healthcare use. Specifically, the 95 percent confidence intervals can rule out changes in costs of inpatient and outpatient visits greater than 0.18 SEK and changes in drug purchase costs greater than 0.02 SEK per a 1 SEK increase in benefits.[4] I do not find effects when measuring healthcare use in terms of the number of in-/outpatient care visits or at the extensive margin, either.

Moreover, I fail to find statistically significant effects on healthcare use when focusing separately on (i) men vs. women, (ii) younger vs. older benefit recipients, (iii) recipients with

4. These bounds do not add up to the bound for total healthcare costs because I choose the estimation bandwidth separately for each outcome, as recommended by Cattaneo and Vazquez-Bare (2017).

vs. without a partner, or (iv) different types of in-/outpatient visits and drug purchases. Overall, my findings indicate that in the Swedish context, characterized by generous social insurance and public health systems, slightly raising the level of unemployment benefits does not affect the costs of beneficiaries' healthcare use and hence does not alleviate the public health costs of unemployment.

Related literature. My findings contribute to a growing literature on the effects of social insurance and cash transfer programs on the health and healthcare use of the recipients and their family members (see, e.g. Levy and Meltzer 2008; Ziebarth 2018; Sun et al. 2021, for reviews). Previous studies have analyzed the health and health expenditure effects of sickness and disability insurance[5], health insurance[6], pensions[7], and social assistance[8].

The literature on how UI affects health and healthcare use is more limited. Closest to my paper, Kuka (2020) uses between-state policy variation in the U.S. to show that more generous UI increased health insurance coverage and expenditures, while Ahammer and Packham (2023) find that a nine-week extension to potential unemployment benefit duration in Austria reduced opioid and antidepressant expenditures among women but not among men.

Relative to the above studies, I use a more comprehensive measure of healthcare use that captures the full costs of inpatient and outpatient care use and drug purchases, including costs covered by the healthcare system. My finding that more generous unemployment benefits have little effect on healthcare use contrasts with the findings of Kuka (2020) and Ahammer and Packham (2023), which could be because my research design relies on variation in benefit level instead of potential benefit duration or due to institutional differences.

More broadly, I contribute to the large literature on the effects of job loss on health outcomes and public health costs.[9] An important yet understudied question is to what extent UI can mitigate the adverse health effects of job loss.[10] My findings suggest that, in the Swedish context where public healthcare is highly subsidized, more generous unemployment benefits have little effect on recipients' healthcare use.

5. See e.g., Black et al. (2024), Gelber et al. (2023), and Wikström (2024).

6. See e.g., Card et al. (2009), Brot-Goldberg et al. (2017), and Goldin et al. (2021).

7. See e.g., Salm (2011), Cheng et al. (2018), and Miglino et al. (2023).

8. See e.g., Snyder and Evans (2006), Barham and Maluccio (2013), Aizer et al. (2016), Hoynes et al. (2016), and Hämäläinen et al. (2025).

9. See e.g., Sullivan and Von Wachter (2009), Eliason and Storrie (2009a), and Kuhn et al. (2009).

10. An exception is Amorim et al. (2024) who find using a tenure-based regression discontinuity design that access to UI partly offsets the effects of job loss on mortality and hospitalizations in Brazil, a country with much less generous public health and social insurance systems compared to Sweden.

Outline. The paper proceeds as follows. Section 2 gives an overview of the Swedish unemployment insurance and healthcare systems. Section 3 describes my data sources, analysis sample, and key variables. Section 4 discusses the research design. Section 5 presents the main findings. Section 6 concludes. Additional discussion and results are collected in an Appendix.

2 Context

This section gives overviews of the Swedish unemployment insurance and healthcare systems.

2.1 Unemployment Insurance

To qualify for unemployment insurance (UI), individuals need to be registered at the Public Employment Service, fulfill a work history requirement, actively seek work, and be prepared to take a suitable offer for a job or a labor market program. Individuals can receive unemployment benefits for up to 300 days (5 payment days per week for 60 weeks), after which they have to participate in active labor market programs to continue receiving benefits.

During the period I study, statutory UI was provided by 27 UI funds ("a-kassa") that were typically affiliated with trade unions. Although contributions to the UI funds are voluntary, membership rates were relatively high, ranging from 70–83 percent of the labor force aged 16–64 (IAF 2024b).[11]

Statutory UI consists of two parts. Basic benefits ("grundersättning") provide a fixed amount regardless of the individual's pre-unemployment earnings. During my study period, basic benefits were equal to 320 SEK per day, or roughly 25–31 percent of the median wage.[12]

My focus is on income-based benefits ("inkomstrelaterad ersättning") available to individuals who have contributed continuously to a UI fund in the twelve months before unemployment. Income-based benefits replace a fraction of the individual's pre-unemployment daily wage up to a cap. This policy rule creates a kink in the relationship between benefits and pre-unemployment earnings, as discussed in Section 4.[13]

11. Membership rates fell by more than 10 percentage points following reforms in 2007–2008 that sharply increased membership premia and introduced an additional "unemployment fee" that partly tied the premia of each fund to the average unemployment rate of its members. Since 2007 membership rates have remained stable at around 70 percent, even though the unemployment fee was repealed in 2014. See Kolsrud (2018) and Landais et al. (2021).

12. Own calculations based on data from Statistics Sweden (2024a).

13. Unions can also offer members their own, non-statutory UI schemes that top up statutory UI. Between 69 and 78 percent of the labor force belonged to a union during the period I study (Kjellberg 2019, Table 3),

2.2 Healthcare System

Sweden has a national healthcare system, financed primarily by taxes, that highly subsidizes healthcare visits and prescription drug purchases (e.g., Anell et al. 2012; Björvang et al. 2023).[14] For example, in 2016, out-of-pocket costs paid by households accounted for 1 percent of total inpatient care expenditures, 14 percent of outpatient care expenditures, and 28 percent of prescription drug expenditures (OECD 2019, Figure 2).

Patient fees for healthcare visits are relatively low, being at most 100 SEK for inpatient visits and 350 SEK for specialized outpatient visits, and varying from 100–250 SEK across counties for primary care visits in 2017 (Pontén et al. 2017). All residents are automatically covered by a public and uniform prescription drug insurance scheme where the share of out-of-pocket costs declines with total annual expenditures (see e.g., Wikström 2023). Both patient and prescription drug fees have ceilings for out-of-pocket expenditures that reset 12 months after the first visit/purchase of the coverage period. In 2017, the ceiling for prescription drug expenditures was 2200 SEK while the ceiling for patient fees ranged from 900–1100 SEK across counties (Pontén et al. 2017).

3 Data

This section describes the administrative data sources and how I define the analysis sample and key variables. See Appendix A for a more detailed discussion.

3.1 Administrative Data

Unemployment spells. My analysis builds on administrative data on registered unemployment spells obtained from the *Hist_Aktso*, *Insper*, and *Sokatper* registers of the Swedish Public Employment Service (PES) (AF 2024a, 2024b, 2024c). For each spell, I observe the dates when the spell is registered and deregistered at the PES, transitions between different job seeker categories during the spell (open unemployment, participation in a given labor market program, etc.), and the reason for deregistering the spell.[15]

and 70 percent of union members were eligible for non-statutory UI in 2009 (Lindquist and Wadensjö 2011, p. 17). Unfortunately, I do not observe in my data which union (if any) an individual belongs to or if she is eligible for or receives non-statutory UI. My analysis therefore only focuses on statutory UI.

14. Individuals generally pay the full price for over-the-counter drugs and drugs not covered by the reimbursement scheme.

15. Appendix Table 1 summarizes how I map job seeker categories and deregistration codes to employment, unemployment, participation in a labor market program, other individuals registered at the PES, and

I define the start date of an unemployment spell as the date it is registered by the PES and consider the spell to end when it is deregistered by the PES (e.g., due to finding employment, exiting from the labor force or to another social insurance program, or starting an education program not offered by the PES).[16]

Unemployment benefit payments. To each unemployment spell, I match data on weekly unemployment benefit payments from the *ASTAT* database of the Swedish Unemployment Insurance Inspectorate (IAF 2024a). The database draws from the administrative system where UI fund employees report payments made to beneficiaries and the information used as the basis for these payments. For each payment week, I observe the number of payment days, the daily benefit amount, the pre-unemployment daily wage used as the basis for the benefit payments, and the scheme (basic vs. income-based benefits) under which payments were made.

I aggregate the resulting matched "unemployment spell × unemployment benefit payment week" data to the unemployment spell level. For each spell, I calculate the average daily wage, the average daily unemployment benefit amount, and the average replacement rate (average daily benefits divided by the average daily wage) for the first 200 payment days of the of the unemployment spell.

Socioeconomic background. For each unemployment spell, I match information on the individual's socioeconomic background (age, gender, educational attainment, if the person is married or cohabiting, having children under age 18 at home, county of residence, and industry of highest-paying employer if she had any) using data from the *Longitudinal Integrated Database for Health Insurance and Labour Market Studies* (LISA), *Total Population Register* (RTB), and *Register-Based Labor Market Statistics* (RAMS) databases of Statistics Sweden (2022, 2023a, 2023b). I measure these covariates at the end of the last calendar year before the start of the unemployment spell. I use these covariates in Section 5 to assess the validity of the research design and for heterogeneity analyses.

Healthcare use. To measure healthcare use, I draw from two registers of the National Board of Health and Welfare (Socialstyrelsen). First, I obtain data on inpatient care and outpatient

individuals deregistered from the PES.

16. This definition for the end of an unemployment spell differs slightly from Kolsrud et al. (2018), who define a spell as ending if the person finds any type of employment (including subsidized employment) or begins an active labor market program while still receiving unemployment benefits and being registered at the PES.

care visits from the *National Patient Register* (Socialstyrelsen 2022b, 2022c). Primary care visits are not included. For each visit, I observe dates of admission and discharge (the latter only for inpatient care), the main diagnosis code, and the associated Major Diagnostic Category (MDC). Second, I obtain data on prescription drug purchases from outpatient pharmacies from the *National Prescribed Drug Register* (Socialstyrelsen 2022d). For each purchase, I observe the purchase date and the disaggregated total costs of the purchase.

3.2 Sample Definition

My analysis uses data on the universe of unemployment spells with a start date between March 5, 2007 and July 14, 2014.[17] I focus on spells where the individual had turned age 20-64 in the calendar year before the start of the spell because eligibility for income-based unemployment benefits begins after turning age 20 and ends after turning age 65. I restrict attention to spells where the individual has a pre-unemployment daily wage between 150 SEK and 1,800 SEK, exclude spells during which the individual only receives basic benefits, and exclude spells for which I cannot match information on pre-unemployment socioeconomic characteristics, except for employer industry which I allow to be missing.

In the analysis, I focus on the first 40 calendar weeks of the unemployment spell.[18] During the period I study, the income-based UI scheme replaced 80 percent of the individual's pre-unemployment daily wage up to a cap of 680 SEK per day.[19] Individuals therefore reach the benefit cap with a daily wage of 850 SEK or higher, corresponding to the 27$^{\text{th}}$ percentile of the daily wage distribution in my analysis sample (Figure 1, Panel B). The benefit cap was fairly low, replacing roughly 53–65 percent of the median monthly wage.[20]

3.3 Variable Definitions

I use the administrative registers to construct the following main variables for the analysis. Unless stated otherwise, I deflate all cost variables using the overall consumer price index (Statistics Sweden 2024b) with 2020 as the reference year.

17. I focus on this period because it is the longest one during which the rules of the income-based UI scheme remained unchanged and for which I can measure the main outcomes.

18. This ensures that the parameters (replacement rate and benefit cap) of the income-based UI scheme are fixed.

19. As noted in Section 2, unemployment benefits are initially granted for a maximum of 300 payment days (60 payment weeks). During the period I study, the replacement rate falls from 80 to 70 percent while the benefit cap remains at 680 SEK per day after the first 40 payment weeks (200 payment days).

20. Own calculations based on data from Statistics Sweden (2024a).

Daily wage and daily benefit. I observe the daily wage and daily benefits, two key variables for my research design, directly in the administrative data. Daily wage is calculated by the PES based on the individual's earnings history before the unemployment spell, after which the daily benefit amount is determined based on the daily wage and the number of payment days the individual has used up during the unemployment spell. I measure both variables in nominal terms because unemployment benefits are not indexed.

Inpatient and outpatient care use. My first measure of healthcare use measures *the number and total costs of inpatient and outpatient care visits* that the individual has over the first 40 calendar weeks since the start of the unemployment spell. I compute the costs of a visit by combining information on its MDC code with information on the average per-day costs of inpatient and outpatient care visits associated with that MDC.[21] I measure these costs using data collected from the Swedish Association of Local Authorities and Regions (SKR 2023) and the National Board of Health and Welfare (Socialstyrelsen 2023), using 2020 as the reference year (see Appendix A for details).[22] Appendix Table 2 shows all 29 MDC codes used during the period I study, along with their average per-day costs, separately for inpatient and outpatient care visits.

This cost measure captures the broad costs of resources used during the healthcare visits that the individual has over a given time period. National guidelines stress that regions should attribute costs as closely as possible to a unique patient and healthcare visit. Relevant costs include costs of operations and procedures (surgeries, intensive care unit, X-rays, radiology, anesthesia, etc.), lab examinations, and costs of medications and materials, but also underlying costs such as those related to management, administration, facilities, and other support functions. See SKR (2020) for a detailed discussion of the principles and guidelines for the cost calculation.[23]

21. Major Diagnostic Categories are groupings of Diagnosis Related Groups (DRG), which in turn group healthcare visits to categories deemed similar in terms of resource use and hence costs based on diagnoses, operations, and patient characteristics such as age and gender. The DRG system is also used to monitor the cost-effectiveness and resource allocation of the healthcare system in many countries, including Sweden and the United States. See Socialstyrelsen (2022a).

22. I calculate the average per-day costs using information on costs per DRG point (which measures the average overall costs of providing a unit of care), the average length of stay of inpatient and outpatient care visits for each MDC, and average weights for each MDC. I use the weights to scale costs per DRG point to get the average costs of inpatient and outpatient care visits for each MDC code and then divide by the average length of stay to arrive at average per-day costs. All averages are calculated at the national level. See Appendix A for details.

23. A drawback of this cost measure is that the MDC classification is coarse: it groups the roughly 800 DRG codes for inpatient care and the roughly 600 DRG codes for outpatient care to 29 categories. Unfortunately, I do not observe the DRG codes associated with inpatient and outpatient care visits in the data.

Drug purchases. As the second measure of healthcare use, I measure *the costs of drug purchases* by the individual over the first 40 calendar weeks since the start of the unemployment spell. I distinguish between the total costs of the purchased drugs, out-of-pocket costs paid by the individual, and costs covered by prescription drug insurance.

Total costs of healthcare use. As the third measure of healthcare use, I measure *the total costs of healthcare use* over the first 40 calendar weeks since the start of the unemployment spell. I define this as the sum of the total costs of inpatient and outpatient care visits and the total costs of drug purchases. For this and the other two cost measures, I winsorize costs above the 99th percentile since healthcare expenditures are typically highly right-skewed, a well-known fact in health economics (see e.g., Karlsson et al. 2024). I discuss the sensitivity of my estimates to different choices on the level of winsorization in Section 5.4.

3.4 Summary Statistics

Table 1 presents descriptive statistics for the analysis sample as well as the Swedish population aged 20–64. The analysis sample includes 340,955 unemployment spells affecting 320,592 individuals. Relative to the population, individuals in the analysis sample are younger, less likely to be married or cohabiting, less likely to have higher education, more likely to have worked in the manufacturing sector, and have similar gross earnings in the previous calendar year. However, their costs of healthcare use and drug purchases in the calendar year before the start of the unemployment spell were somewhat lower than for the population.

4 Research Design

Intuition. I use a regression kink (RK) design that leverages variation in the generosity of unemployment benefits created by the non-linear relationship between benefits and earnings prior to unemployment. Under the income-based UI scheme, the daily benefit amount is a piecewise linear function of the pre-unemployment daily wage, replacing a constant fraction of the daily wage up to a cap.

This non-linear policy rule produces a kink in the relationship between daily benefits and pre-unemployment daily wage at the wage at which the individual reaches the benefit cap. Provided that individuals on either side of the kink are ”similar” in terms of other factors correlated with the outcomes of interest (say, costs of healthcare use), I can attribute any kinks observed in the relationship between outcomes and the daily wage to a causal effect of unemployment benefits. I discuss the assumptions necessary for causal interpretation in detail below.

I use a fuzzy instead of a sharp RK design because unemployment benefit payments do not in practice perfectly align with the policy rule. Apart from measurement error, this non-compliance arises because individuals may face sanctions (payment reductions or suspensions) due to e.g. inactive job search or failing to apply for a suitable job (Act 1997:238 §43, cf. SFS 1997).[24]

Identification. Following Card et al. (2015), I consider the non-separable model

$$Y = y(B^*, W^*, U),$$

where Y is an outcome of interest, B^* is the observed daily benefit amount (the treatment variable), W^* is the observed pre-unemployment daily wage (the running variable), and U is a potentially multidimensional error term. The object of interest is the causal effect of a small increase in benefits B^* on the outcome Y, that is, the partial derivative $\frac{\partial y(B^*,W^*,U)}{\partial B^*}$ of Y with respect to B^*.

Under perfect compliance, received benefits B^* would be determined by the policy rule $\rho \min(W, \overline{w})$, where W is the actual daily wage, ρ is the replacement rate, and $\overline{w} = \overline{b}/\rho$ is the daily wage at which individuals reach the benefit cap $\overline{b}$ (the kink point). However, due to non-compliance, observed payments may differ from payments predicted by the policy rule,

$$B^* = b(W, \varepsilon),$$

where the vector ε captures sources of non-compliance and is potentially correlated with U and hence Y. Similarly, I allow for measurement error in the daily wage, that is, $W^* = W + e$ for e an error term.

The parameter of interest is the fuzzy RK estimand

$$\tau = \frac{\beta^+ - \beta^-}{\kappa^+ - \kappa^-} = \frac{\lim_{w_0 \to \overline{w}^+} \frac{d\,\mathrm{E}[Y|W^*=w^*]}{dw^*}\Big|_{w^*=w_0} - \lim_{w_0 \to \overline{w}^-} \frac{d\,\mathrm{E}[Y|W^*=w^*]}{dw^*}\Big|_{w^*=w_0}}{\lim_{w_0 \to \overline{w}^+} \frac{d\,\mathrm{E}[B^*|W^*=w^*]}{dw^*}\Big|_{w^*=w_0} - \lim_{w_0 \to \overline{w}^-} \frac{d\,\mathrm{E}[B^*|W^*=w^*]}{dw^*}\Big|_{w^*=w_0}}, \tag{1}$$

24. Information on payments comes from a system where UI fund employees report payments, so measurement errors should be minimal. Sanctions are also rare: only 0.54 percent of spells in the analysis data are such that the individual faces a payment suspension or reduction at least once.

where β^{+} and β^{-} are the slopes of the conditional mean of Y to the right and to the left of the kink point $\overline{w}$, and κ^{+} and κ^{-} are the slopes of the conditional mean of B^{*} to the right and the left of the kink point. That is, the RK estimand (1) is equal to the change in the slope of the conditional mean of the outcome Y at the kink point, divided by the change in the slope of the conditional mean of the treatment variable B at the kink point.

Card et al. (2015, Section 2.2.2. and Proposition 2) provide conditions sufficient for the RK estimand (1) to identify a weighted average of marginal effects of B on Y, with larger weights on groups with larger kinks in B at the kink point, groups more likely to be at the kink point, and groups with less measurement error in the assignment variable B (i.e., less non-compliance). In addition to certain regularity conditions, identification relies on three key assumptions.

1. *First stage.* The average replacement rate (slope of the conditional mean of the treatment variable B) changes at the kink point and there is a non-negligible population at the kink point $\overline{w}$.

2. *Monotonicity.* The direction of the kink in the treatment variable is the same for the whole population, that is, $\lim_{w_0 \to \overline{w}^{+}} \frac{\partial B(w,\varepsilon)}{\partial w} \leq \lim_{w_0 \to \overline{w}^{-}} \frac{\partial B(w,\varepsilon)}{\partial w}$ for all ε.

3. *Smooth density of W.* The density of the actual daily wage W, conditional on the vector of unobserved heterogeneity (U, ε), is continuously differentiable in a neighborhood of the kink point $\overline{w}$.

To assess the first two assumptions, I test for the existence of a first-stage kink (Card et al. 2015, Remark 4). The third assumption rules out deterministic sorting just above or below the kink point $\overline{w}$. I assess the validity of this assumption in Section 5 by checking for discontinuities and kinks in the densities of the daily wage W^{*} and in the conditional means of pre-determined covariates around the kink point $\overline{w}$ (Card et al. 2015, Corollaries 1–2). I also check for any kinks in the outcomes of interest when measured *before* the start of the unemployment spell.

Estimation and inference. Following the standard in the literature, I implement the fuzzy RK design via local polynomial estimation (e.g., Card et al. 2015, 2017; Gelber et al. 2023). The fuzzy RK estimator of τ in (1) is

$$\hat{\tau} = \frac{\hat{\beta}_1^{+} - \hat{\beta}_1^{-}}{\hat{\kappa}_1^{+} - \hat{\kappa}_1^{-}}, \tag{2}$$

where $\hat{\beta}_1^s$ and $\hat{\kappa}_1^s$ for $s \in \{+,-\}$ solve the least squares problems

$$\hat{\beta}^s = \min_{\{\tilde{\beta}_j^s\}} \sum_{i=1}^{n^s} \left\{ Y_i^s - \sum_{j=0}^{p} \tilde{\beta}_j^s (W_i^{*s} - \overline{w})^j \right\}^2 K\left(\frac{W_i^{*s} - \overline{w}}{h}\right),$$

$$\hat{\kappa}^s = \min_{\{\tilde{\beta}_j^s\}} \sum_{i=1}^{n^s} \left\{ B_i^{*s} - \sum_{j=0}^{p} \tilde{\beta}_j^s (W_i^{*s} - \overline{w})^j \right\}^2 K\left(\frac{W_i^{*s} - \overline{w}}{h}\right),$$

where $s = -$ denotes quantities to the left and $s = +$ to the right of the kink point, p is polynomial order, K is the kernel function, and h is the bandwidth.

Following Card et al. (2017) and Gelber et al. (2023), my baseline estimates are based on a local linear estimator[25] ($p = 1$), a uniform kernel[26] (i.e., $K(c) = \frac{1}{2}1\{|c| < 1\}$), and a mean squared error (MSE) optimal bandwidth (Calonico et al. 2014a, 2014b).[27] I choose the bandwidth separately for each outcome variable, following the recommendation by Cattaneo and Vazquez-Bare (2017, p. 143). In Section 5 I probe the sensitivity of the estimates to the choice of bandwidth, polynomial order, and kernel. I also compare estimates with and without adjusting for pre-determined covariates following Calonico et al. (2019).[28]

For the results in the main text, I only report bias-corrected RK estimates that correct for the asymptotic bias of the RK estimator under an MSE-optimal bandwidth (see Calonico et al. 2014b). I use a quadratic bias estimator and robust standard errors that account for sampling variation in the bias estimator. Since the same person can have more than one unemployment spell in my analysis sample, I cluster standard errors at the individual level. In the Appendix, I also present conventional RK estimates that do not use bias-correction.

Estimates of interest. The fuzzy RK estimator $\tilde{\tau}$ in (2) is equal to the estimated change in the slope of the conditional mean of the outcome at the kink point (the reduced form) divided by the estimated change in the slope of the conditional mean of benefits at the kink point (the first stage). I report estimates for the fuzzy RK parameter $\hat{\beta}_1^+ - \hat{\beta}_1^-$ and the first

25. For example, Pei et al. (2022) find in Monte Carlo simulations using data on Austrian UI recipients that a local linear specification has a smaller asymptotic mean squared error than a local quadratic specification for sample sizes up to 86 million observations. My analysis sample is considerably smaller than this.

26. I prefer a uniform kernel over the boundary-optimal triangular kernel because the asymptotic bias and variance of the RK estimator (2) with a uniform kernel are not affected by imposing continuity (Card et al. 2012).

27. I follow Gelber et al. (2023) and omit the regularization term of the Calonico et al. (2014a) MSE-optimal bandwidth selector since Card et al. (2015, 2017) argue it tends to pick too small bandwidths in RK settings.

28. While covariate adjustment is not necessary for RK estimates to be consistent, Ando (2017) argues that including covariates can improve efficiency when the relationship between the running variable and the outcome variable is non-linear.

stage $\hat{\kappa}_1^+ - \hat{\kappa}_1^-$. The first stage estimate tells how average daily benefits change in response to a 1 SEK increase in the daily wage. The fuzzy RK estimate $\hat{\tau}$ tells how the outcome changes on average in response to a 1 SEK increase in daily benefits B^*. When reporting fuzzy RK estimates for outcomes related to costs, I scale the estimates so that they tell how much how much costs change per a 1 SEK increase in benefits.[29]

I also report the estimated elasticity of the outcome Y with respect to unemployment benefits B^* at the kink point k,

$$\hat{\varepsilon}_{Y,B} = \hat{\tau} \times \frac{\overline{B^*}}{\overline{Y}}, \tag{3}$$

where $\overline{Y}$ and $\overline{B^*}$ denote the means of the outcome Y and observed benefits B^* around the kink (using observations with a daily wage within 10 SEK of the kink). The estimated elasticity gives the percent change in the outcome per a 1 percent increase in daily benefits. For elasticities, I compute standard errors via a non-parametric bootstrap with 100 replicates where I sample unemployment spells with replacement.

5 Results

5.1 First Stage & Disemployment Effects

I first verify that unemployment benefit payments closely align with the policy rule, that is, there is a strong first stage. Panels A and B of Figure 1 plot the average replacement rate (left column) and average daily benefit (right column) as a function of the daily wage, using a bandwidth of 250 SEK and 5 SEK bins. Red lines in each plot illustrate the relationship between the variables predicted by the policy rules. Figure 1 clearly shows that observed average replacement rates and daily benefits closely follow those predicted by the policy rule, indicating that non-compliance and measurement error in daily benefits or the daily wage are not an issue.

Next, I verify that the level of unemployment benefits affect the duration of unemployment and benefit payment spells. These "disemployment effects" have been the focus of the literature on the behavioral effects of UI (see Cohen and Ganong 2025). Appendix Figure 1 shows binned scatterplots of spell durations against the daily wage; Appendix Table 3 presents the corresponding estimates. An increase in the level of benefits increases spell

29. The unscaled fuzzy RK estimates tell how much costs of healthcare use, measured over the first 40 weeks since the start of the unemployment spell, change in response to a 1 SEK increase in daily unemployment benefits. In practice, I scale these estimates by dividing them by $40 \times 5 = 200$ because costs are measured over 40 weeks and unemployment benefits are paid 5 days per week.

duration: a 1 percent increase in benefits increases unemployment spell duration by 0.19 percent (SE = 0.21) and unemployment benefit spell duration by 0.83 percent (SE = 0.24). Both elasticities are roughly in line with the existing literature.[30]

5.2 Effects on Healthcare Use

Total costs of healthcare use. I now turn to the main results. Figure 2A shows how the total costs of the UI recipient's healthcare use evolve around the kink point where individuals reach the cap in daily benefits. The outcome of interest is the sum of the total costs of inpatient and outpatient care visits and drug purchases, measured over the first 40 weeks since the start of the unemployment spell.

Figure 2A does not indicate discontinuous changes in the slope of total healthcare costs as a function of the daily wage, either when measuring costs directly (left panel) or when focusing on the extensive margin, i.e. whether the benefit recipient had any healthcare use (right panel). Columns 1–2 of Table 2 and Appendix Table 5 confirm these findings by showing that an increase in unemployment benefits has no statistically significant effect on total costs of healthcare use.

The estimates in Table 2 are precise enough to be informative. Specifically, the 95 percent pointwise confidence intervals in columns 1–2 can rule changes in total costs of healthcare use greater than 0.08 SEK per a 1 SEK increase in unemployment benefits. In terms of elasticities, my estimates can rule out changes in costs greater than 1.3 percent per a 1 percent increase in benefits.

Inpatient and outpatient care use. The lack of an effect on overall healthcare use could be the result of opposing effects on inpatient and outpatient care visits and drug purchases that cancel out. However, Figure 2B and Appendix Figure 2 do not indicate kinks in the relationship between the daily wage and inpatient and outpatient care use, either. The corresponding estimates, shown in columns 3–8 of Table 2 and Appendix Tables 4–5, corroborate the graphical evidence: an increase in the level of unemployment benefits does not have a

30. In their meta-analysis of studies estimating disemployment effects of UI, Cohen and Ganong (2025) find that, after correcting for publication bias, 90 percent of elasticities exploiting variation in the replacement rate fall between -0.22 and 0.65 (see Cohen and Ganong 2025, Table 2, Row 1, Column 7). My estimates are smaller than those reported in the Swedish context by Kolsrud et al. (2018, Table 2, Panel I, Column 1), who estimate an elasticity of unemployment spell duration on daily unemployment benefits of 1.53 (SE = 0.13). My estimates may differ from those in Kolsrud et al. (2018) for several reasons, such as differences in study periods (they focus on a different kink point applicable for the period 1999–2007) and differences in when the unemployment spell is defined to end.

statistically significant effect inpatient or outpatient care use.

Again, the estimates are precise enough to be informative. Starting with costs, the 95 percent pointwise column intervals in Table 2 can rule changes costs of inpatient and outpatient care visits greater than 0.18 SEK (columns 3–4), changes in costs of inpatient care visits greater than 0.10 SEK (columns 5–6), and changes in costs of outpatient care visits greater than 0.03 SEK (columns 7–8) per a 1 SEK increase in benefits.[31]

I do not find effects when measuring inpatient and outpatient care use by the number of visits or at the extensive margin (having any visits), either. For example, the estimates in columns 1–2 of Appendix Table 4 allow ruling out changes in the number of visits greater than 0.23 visits per a 100 SEK increase in daily benefits, relative to a mean of 1.0 visits over the first 40 weeks since the start of the unemployment spell among individuals with a daily wage close to the kink point.

Overall, these findings are consistent with Ahammer and Packham (2023, cf. Table 4), who find that increasing the potential benefit duration does not have statistically significant effects on inpatient or outpatient care use in their sample of older Austrian unemployed individuals.

Drug purchases. Figure 2C shows how drug purchases, measured in terms of costs and at the extensive margin (having any purchases) evolve around the kink point. As noted in Section 3, costs of drug purchases include both out-of-pocket costs as well as costs covered by prescription drug insurance. Although the relationship between drug purchase costs and the daily wage is non-linear, Figure 2C does not suggest clear discontinuous slope changes at the kink point where individuals reach the cap in daily benefits.[32]

The corresponding estimates shown in columns 9–10 of Table 2 and Appendix Table 5 confirm the graphical evidence, showing that an increase in benefits does not have a statistically significant effect on drug purchases, whether measured by total costs or at the extensive margin. For total costs, the 95 percent pointwise confidence intervals in columns 9–10 of Table 2 can rule out changes greater than 0.02 SEK per a 1 SEK increase in benefits.

31. Note that since I choose the MSE-optimal bandwidth separately for each outcome, the upper bounds on the effects on inpatient care (columns 5–6) and outpatient care (columns 7–8) use should not be expected to sum up to the upper bound on the effect on inpatient *and* outpatient care use (columns 3–4), even though the latter outcome is the sum of the former two outcomes.

32. Appendix Figure 6(c) shows that the relationship between costs and the daily wage is also non-linear when looking at drug purchases *before* the start of the unemployment spell.

5.3 Effect Heterogeneity

The main takeaway from the previous section is that the UI generosity matters little for recipients' healthcare use, whether measured by costs, number of visits, or at the extensive margin. Next, I examine whether this lack of an effect for the whole estimation sample masks variation when looking at different groups of benefit recipients or different types of healthcare use. Figure 3 summarizes the findings from these analyses.

Studying such effect heterogeneity is important for at least three reasons. First, existing evidence suggests that there is heterogeneity in the adverse health effects of unemployment, with effects being smaller for women and larger for long-term unemployed and for measures of mental health (see Picchio and Ubaldi 2023). Second, some studies find that behavioral responses to UI are heterogeneous (e.g., Ahammer and Packham 2023; Ferey et al. 2024), and that unemployment, job loss, and UI can generate spillover effects on partners (e.g., Cullen and Gruber 2000; Marcus 2013; Hendren 2017; Gathmann et al. 2025). And third, meaningful effect heterogeneity could also provide a rationale for policies where UI generosity varies by recipient characteristics such as age (Akerlof 1978; Spinnewijn 2020).

Effects by gender and age group. Figure 3A shows point estimates and their 95 percent confidence intervals of the effect of unemployment benefits on total costs of healthcare use, separately by gender and age quartile. The youngest age quartile includes those aged 20–29 while the oldest includes those aged 49–64. Although the estimates for some gender and age groups are somewhat noisy (in particular for the youngest age quartile) and for men and women aged 30–48 the estimates are statistically significant (which could happen by chance given the number of coefficients), the main takeaway is that I do not find that more generous UI would systematically increase or decrease the costs of in-/outpatient care visits or drug purchases in any of the groups.

Effects for singles vs. couples. The bottom part of Figure 3A shows point estimates and confidence intervals separately for individuals who had ("Couples", 47.8 percent of the spells) and did not have ("Singles") a marital or cohabiting partner in the calendar year before the start of the unemployment spell. For couples, I estimate effects on total healthcare costs of the recipient and the partner, both together and separately. I do not find statistically significant effects on healthcare use in any of these cases. For example, the 95 percent pointwise confidence intervals can rule out changes greater than 0.06 SEK for singles and greater than 0.31 SEK for couples, where the latter sums up both the recipient's and partner's healthcare costs.

Effects by type of healthcare use. Figures 3B and 3C show point estimates and confidence intervals for different categories of healthcare visits and drug purchases, respectively. Appendix Table 10 describes how I define these categories, using main diagnosis codes for healthcare visits and ATC codes of purchased drugs.

Starting with inpatient and outpatient visits, I follow Kuhn et al. (2009) and focus on the broad categories of visits related to cancers, heart disease (e.g., hypertension, stroke, heart attack, unstable angina), mental health conditions (e.g., anxiety, depression), respiratory conditions (e.g., asthma, pneumonia, chronic obstructive pulmonary disease), cerebrovascular conditions (e.g., intracranial hemorrhage), and external conditions (e.g., accidents and self-harm). For example, Kuhn et al. (2009), Eliason and Storrie (2009b), and Gathmann et al. (2025) find that job loss increases hospital visits related to mental health conditions and external causes. However, I estimate very null precise null effects for each of these categories.

Turning to drug purchases, I again follow Kuhn et al. (2009) and first focus on costs related to purchases of *psychotropic* and *psychosomatic* drugs. The former drugs are meant to treat psychological distress and include drugs such as sedatives, benzodiazepins, and antidepressants; the latter are meant to treat physical ailments linked to prolonged stress and include drugs such as migraine therapeutics and anti-inflammatory drugs. As in Ahammer and Packham (2023), I also look specifically at purchases of antidepressants and drugs related to treating chronic pain (opioids and non-opioid painkillers). As above, I estimate very precise null effects for all of these categories.

Effects over time. The lack of an effect of UI generosity on healthcare use might still mask interesting dynamics over time, for example if any effects on healthcare use reflect a gradual deterioration of physical or mental health. To investigate such dynamics, Figure 4 presents estimates for the effects of UI generosity on costs of healthcare use at the *weekly* level, ranging from 52 calendar weeks *before* the start of the unemployment spell and going up to 40 weeks *after* the start of the spell. Weeks before the start of the spell serve as placebo tests to rule out sorting around the kink point e.g. due to health shocks that occur just before the start of the unemployment spell (i.e. an Ashenfelter dip). Figure 4 shows no evidence of dynamic effects: the estimates for the effects of benefits on total healthcare costs (Panel A), total costs of inpatient and outpatient care visits (Panel B), and total costs of drug purchases (Panel C) remain stable and close to zero, both before and after the start of the unemployment spell.

5.4 Validity Tests and Sensitivity Analyses

Here I provide support for the validity of the RK design by summarizing results from a series of robustness checks and sensitivity analyses. I also probe the sensitivity of my estimates to various specification choices.

Manipulation of running variable. Figure 1B(ii) shows the density function of the daily wage. The graph does not suggest a discontinuous jump (bunching) or kink (slope change) in the density. I formally test for the presence of discontinuities in two ways and report test statistics and associated *p*-values from the tests on the graph. Both a McCrary (2008) test for a discontinuous jump and a test similar to Landais (2015) and Card et al. (2015) for a kink in the density function indicate that I cannot reject the null hypothesis of a lack of discontinuous jump or slope change at the kink point (see notes to Figure 1 for details).

Smoothness of pre-determined covariates and placebo outcomes around kink. Three pieces of evidence support the assumption that other determinants correlated with healthcare use evolve smoothly around the kink point.

First, Appendix Figure 4 shows binned scatterplots of *predicted* costs healthcare use around the kink point. I form predictions using fitted values from a linear regression of the outcome against the set of covariates measured in the calendar year before the start of the unemployment spell (see Section 3).[33] Although some of the predicted outcomes evolve non-linearly around the kink point, the corresponding coefficients shown in Appendix Table 7 do not indicate the presence of kinks.

Second, Appendix Figure 5 presents binned scatterplots of the conditional means of selected covariates against the daily wage. Although these conditional means evolve non-linearly as a function of the daily wage, the coefficients in Appendix Table 8 do not indicate the presence of kinks.

Third, Appendix Figure 6 shows binned scatterplots of the costs of healthcare use in the last 12 months *before* the start of the unemployment spell against the daily wage. The plots for these placebo outcomes show no evidence of discontinuities at the kink points, which the corresponding point estimates reported in Appendix Table 9 confirm.

Sensitivity to bandwidth choice. Appendix Figure 7 shows point estimates and their 95 percent pointwise confidence intervals for the effect of unemployment benefits on costs of

33. Appendix Table 6 reports results from the regressions used to create these covariate indices.

healthcare use for varying bandwidths. In each panel, the dashed vertical line indicates the MSE-optimal bandwidth that I use for the main results. For the total costs of healthcare and costs of in-/outpatient care visits, the coefficients and confidence intervals remain stable and closely centered around zero for bandwidths much shorter and wider than the MSE-optimal bandwidth. In contrast, for the total costs of drug purchases, estimates based on bandwidths greater than 400 SEK would indicate that more generous unemployment benefits decrease drug expenditures. However, these coefficients are not economically meaningful. For example, using a bandwidth of 500 SEK, the point estimate implies that a 1 SEK increase in benefits decreases costs of drug purchases by 0.003 SEK (SE = 0.001).

Alternative specifications. My main estimates are based on a specification with a local linear estimator, a uniform kernel, quadratic bias-correction, and robust standard errors. Appendix Figure 8 compares these estimates to estimates from alternative specifications where I vary the polynomial order (linear vs. quadratic), the kernel function (uniform vs. triangular kernel), and whether I control for pre-determined covariates or not. In each panel, the left set of estimates show conventional RK estimates without bias-correction, while the right set of estimates use bias-correction. The first two bias-corrected estimates from the left correspond to my baseline estimates.

The main takeaway from Appendix Figure 8 is that none of the alternative specifications indicate a statistically significant effect of unemployment benefits on costs of healthcare use. Although the estimates without bias-correction have considerably tighter confidence intervals, Card et al. (2017) show that confidence intervals based on the conventional RK estimator can have too low coverage rates. I therefore prefer the confidence intervals based on the bias-corrected estimator.

Appendix Figure 3 probes the sensitivity of the main estimates to the level above which healthcare costs are winsorized. Relative to the baseline estimates (shown with red square markers), which are based on outcomes winsorized above the 99th percentile, estimates based on outcomes that are not winsorized at all or at a higher level are less precise, as indicated by the wider confidence intervals. However, point estimates tend to stabilize and confidence intervals are of similar lengths when winsorizing roughly above the 99th-to-99.5th percentile. Nevertheless, the confidence intervals for estimates using unwinsorized outcomes are noticeable wider than the intervals for each of the winsorized outcomes. This is consistent with healthcare costs being heavily right-skewed.

6 Conclusion

I use Swedish administrative data on around 340,000 unemployment spells and a regression kink design to study how the generosity of unemployment insurance affects the healthcare use of recipients and their partners. My measure of healthcare use covers inpatient and outpatient care visits and drug purchases and measures total costs to the healthcare system, not just out-of-pocket costs.

I find little evidence that more generous unemployment benefits affect healthcare use, a conclusion that is robust across specification choices and applies to men and women, older and younger individuals, benefit recipients and their partners, and different types of healthcare use. My findings therefore suggest that, in the Swedish context, the adverse health effects of unemployment mainly reflect factors that matter independently of income, such as social stigma or loss of social contacts and identity (e.g., Jahoda 1982), rather than income loss.

My analysis has some limitations. First, the measure of healthcare use is incomplete because I do not observe primary care visits. Second, the estimates do not consider potential spillover effects on children.[34] Third, due to data limitations, the cost measure for healthcare visits is based on a coarse categorization of visits to 29 groups. Fourth, my research design only allows estimating the effects of a small increase in the generosity of unemployment benefits for the subgroup of individuals located close to the kink point.

I close by highlighting two directions for future work. First, although more generous unemployment benefits do not appear to affect healthcare use in the Swedish setting, such effects could show up in settings where the out-of-pocket costs of healthcare are high and consumption smoothing is costly (cf., Chetty and Looney 2006, 2007).

Second, it is important to study whether benefit generosity affects recipients' healthcare use in the context of other social insurance programs, such as disability insurance (see e.g., Gelber et al. 2023). Since public healthcare systems in developed economies are typically heavily subsidized, the fiscal externalities created by such effects on healthcare use could be sizable and matter for the optimal design of social insurance programs. To detect such fiscal externalities, it is important to use a comprehensive measure of the costs of healthcare use, similar to the one used in this paper.

34. Recent work by Barr et al. (2022) and Bailey et al. (2024), among others, highlights the relevance of intergenerational spillover effects on children in the context of social insurance and transfer programs.

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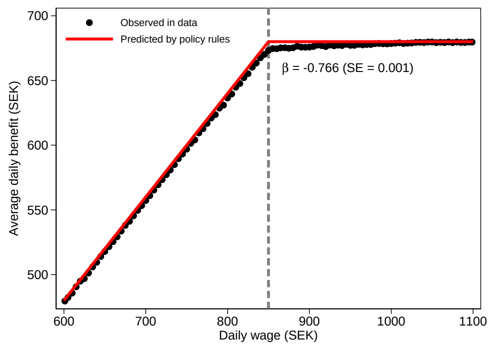
Figures and Tables

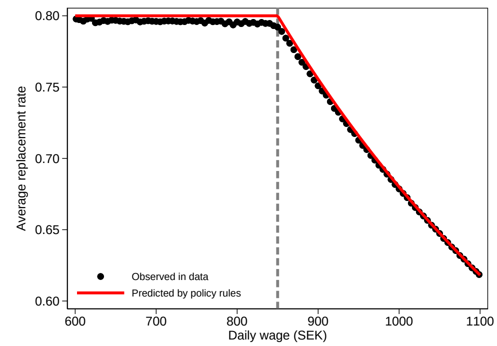
Figure 1: Illustrating the Regression Kink Design

Panel A: First Stage

(i) Average replacement rate

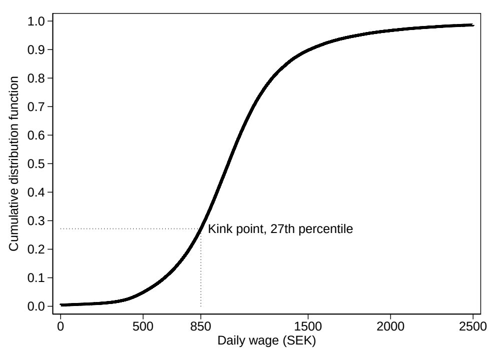


(ii) Average daily benefit amount

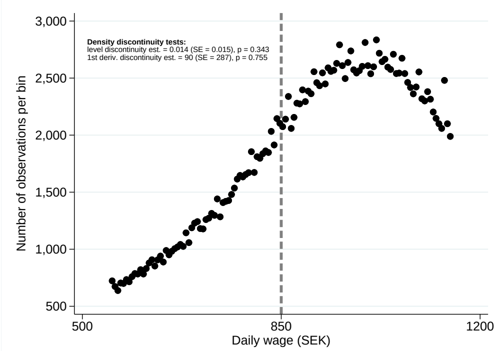


Panel B: Kink Point Location and Density of the Daily Wage

(i) Kink point location



(ii) Density of the daily wage



Notes. This figure illustrates the regression kink design using the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). In each panel, the unit of observation is an unemployment spell. Panel A illustrates the first stage relationship by showing binned scatterplots of the average replacement rate (left column) and average daily benefit (right column) against the daily wage (running variable) using a bandwidth of 250 SEK and 5 SEK bins. Red kinked lines show the relationships between the daily wage, the replacement rate, and the daily benefit predicted by the policy rules. The left column also reports the estimated first stage coefficient and its standard error, using a conventional local linear estimator, a uniform kernel, and a 250 SEK bandwidth. The left column of Panel B shows the empirical cumulative distribution function of the daily wage and marks location of the kink point (850 SEK), indicating that the kink point is at the 27th percentile of the distribution. The right column of Panel B shows the density function of the daily wage using a bandwidth of 300 SEK and 5 SEK bins of the running variable. Black dots show the number of observations in each bin. The top-left corner in each panel reports point estimates and standard errors from two tests for discontinuities in the density of the running variable. The top estimate and standard error are from a McCrary (2008) test for a discontinuous jump in the logarithm of the density of the running variable at the kink point. The bottom estimate is from a test for a discontinuity in the slope (first derivative) of the density of the running variable at the kink point similar to Card et al. (2015) and Landais (2015). The latter test is implemented by estimating

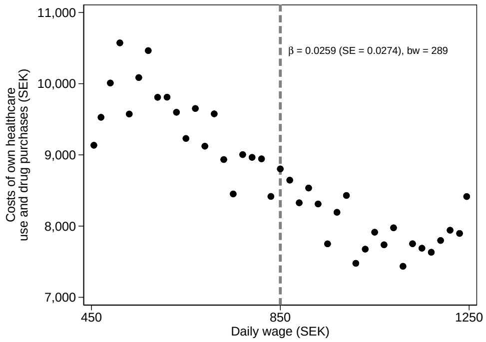
$$\text{obs}_b = \alpha_0 + \sum_{p=1}^{P} \left[\alpha_p \ w_b^p + \beta_p \ w_b^p \times (w_b \geq 0)\right] + \varepsilon_b,$$

where obs_b is the number of observations in bin $b \in \{1,\ldots,120\}$, w_b is the mean of the running variable in bin b, $P = 5$ is the polynomial order, and ε_b is an error term. The figure reports the OLS estimate of the parameter β_1 and its heteroskedasticity-robust standard error.

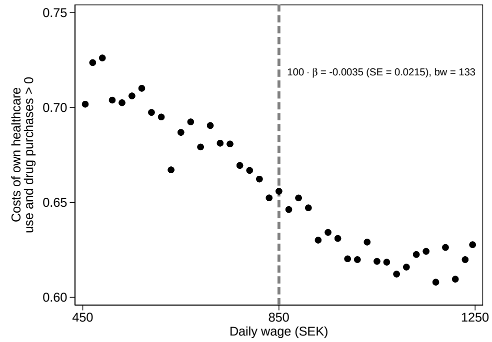
Figure 2: Healthcare Use Around the Kink Point

Panel A: Total Healthcare Use

(i) Total costs

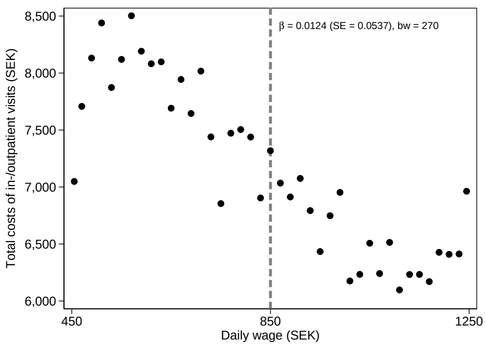


(ii) Any healthcare use

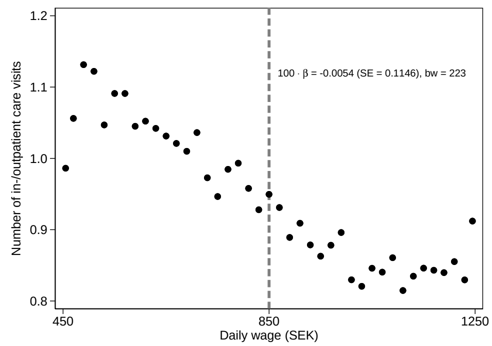


Panel B: Inpatient and Outpatient Care Visits

(i) Total costs

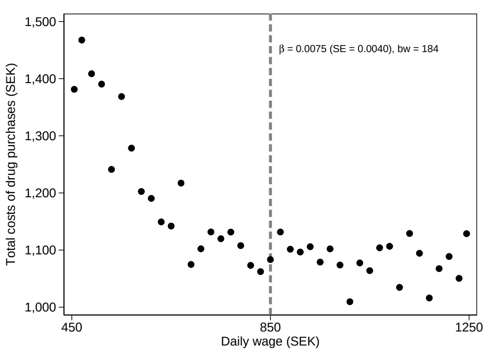


(ii) Number of visits

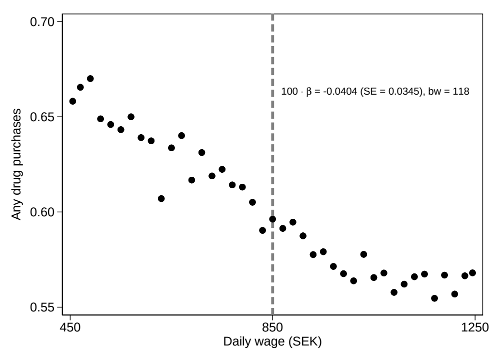


Panel C: Prescription Drug Purchases

(i) Total costs

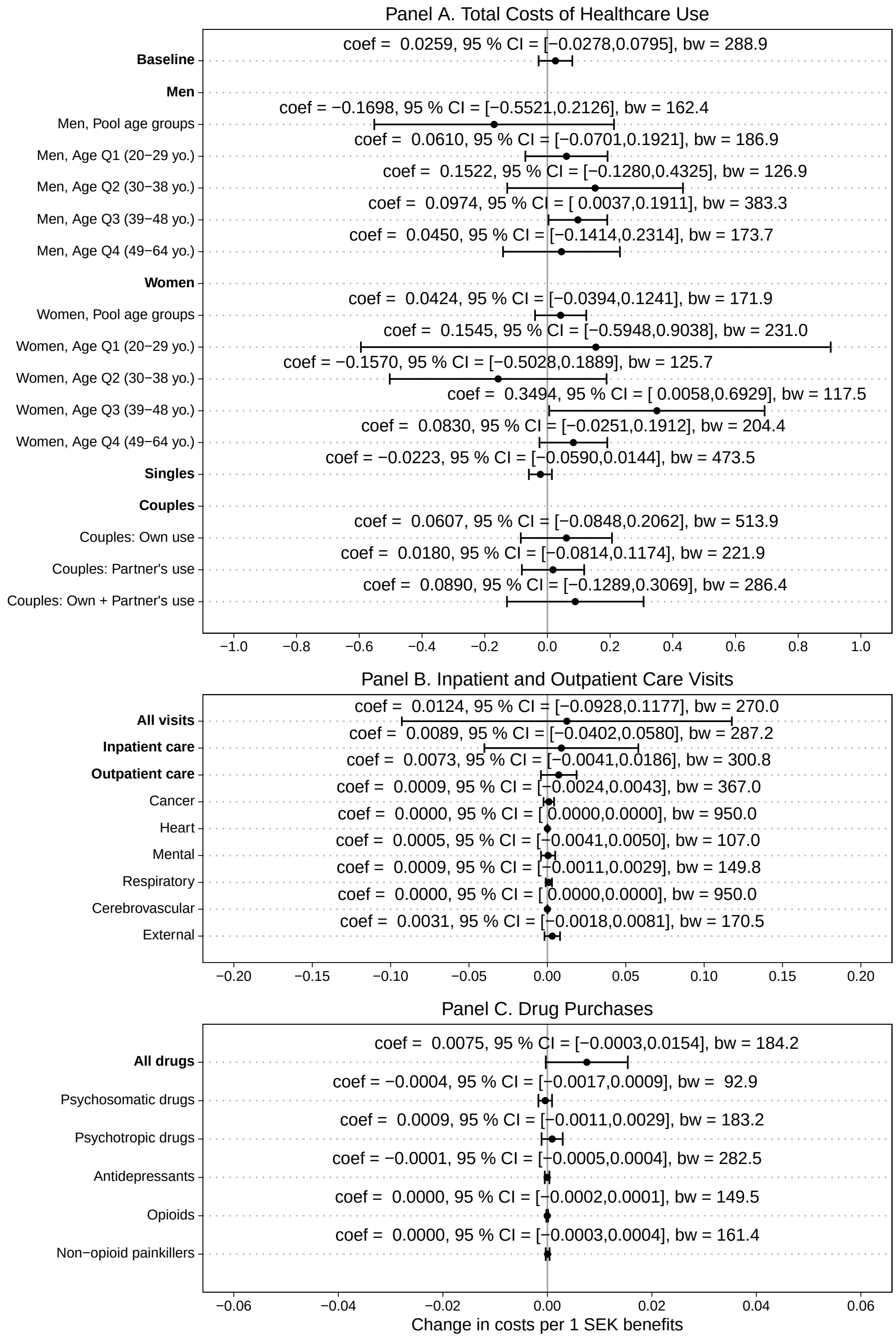


(ii) Any purchases



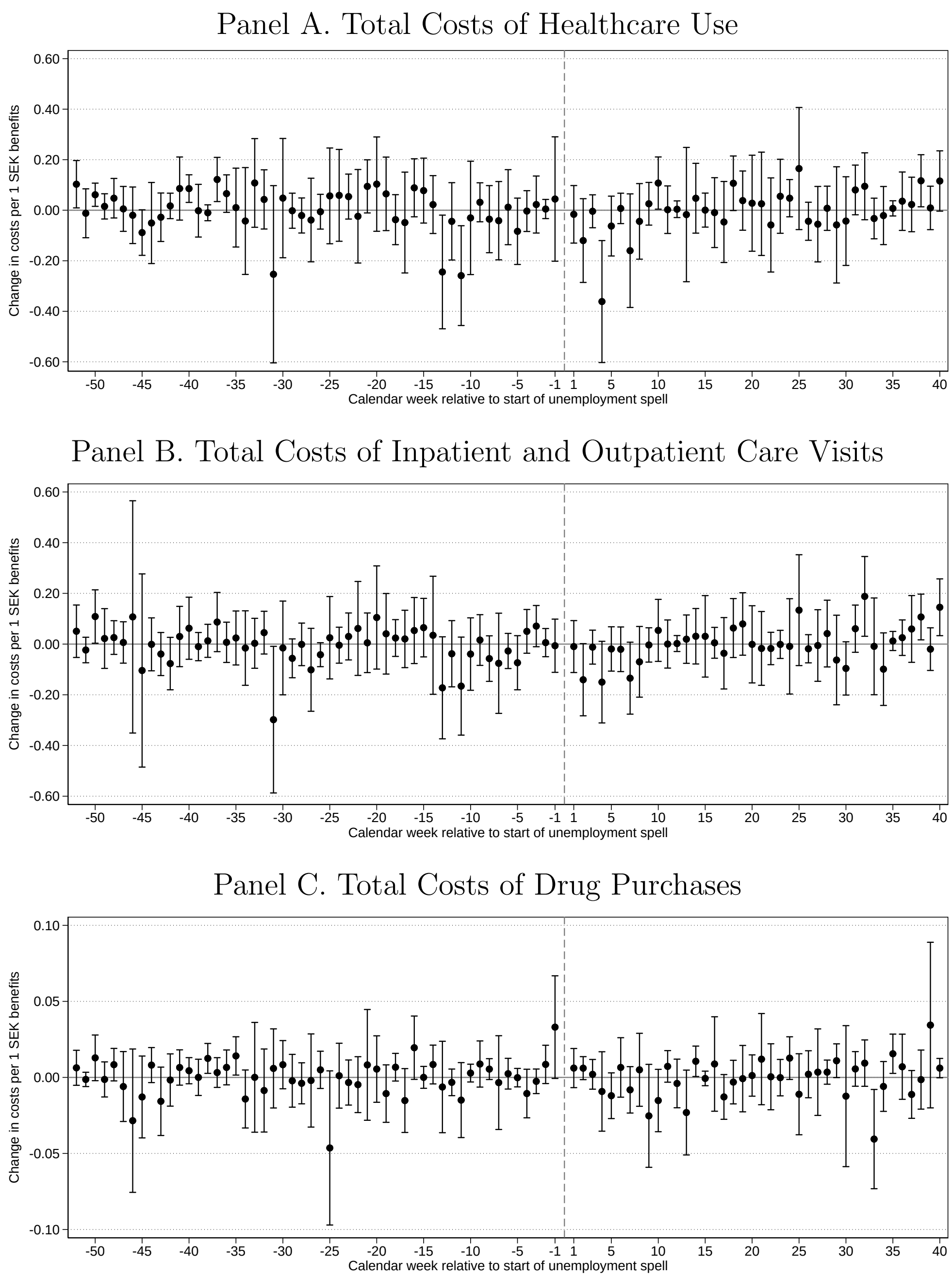
Notes. This figure shows binned scatterplots of different measures of healthcare use as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (Panel A, left column), an indicator for having any healthcare use, (total costs greater than zero; Panel B, right column), total costs of inpatient and outpatient care visits (Panel B, left column), an indicator for having any inpatient or outpatient care visits (Panel B, right column), total costs of drug purchases (Panel C, left column), and an indicator for having any drug purchases (Panel C, right column). Total costs of drug purchases refer to the sum of out-of-pocket costs and costs covered by prescription drug insurance. Each plot also reports the estimated effect of unemployment benefits on the outcome of interest, its standard error, and the bandwidth used for estimation. Estimates are based on a local linear specification with a uniform kernel, MSE-optimal bandwidth, quadratic bias correction, and robust standard errors (Calonico et al. 2014b), controlling for pre-determined covariates. Standard errors clustered at the individual level.

Figure 3: Heterogeneity in the Effects on Healthcare Use



Notes. This figure presents coefficients of the effect of UI benefits on the costs of healthcare use, along with their 95 percent pointwise confidence intervals. Panel A presents estimates separately for subgroups based on gender, age quartile, and relationship status. Panels B and C present estimates separately for different categories of in-/outpatient care visits and drug purchases, respectively. See Appendix Table 10 for how these categories are defined. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). Outcome are the total costs of inpatient and outpatient care visits and drug purchases (Panel A), the total costs of inpatient and outpatient care visits (Panel B), and the total costs of drug purchases (Panel C). Estimates are based on a local linear specification with a uniform kernel, MSE-optimal bandwidths, quadratic bias correction and robust pointwise 95 percent confidence intervals (Calonico et al. 2014b), controlling for pre-determined covariates. Confidence intervals are based on standard errors clustered at the individual level. Point estimates, confidence intervals, and bandwidths used in estimation are shown above the markers for each point estimate. For estimates labeled ”Singles”, the estimates are based on individuals in the analysis sample who did not have a marital or cohabiting partner in the calendar year before the start of the unemployment spell. For the estimates labeled ”Couples”, the estimates are based on individuals in the analysis sample who had a marital or cohabiting partner in the calendar year before the start of the unemployment spell.

Figure 4: Effects on Healthcare Use Before and Over the Unemployment Spell



Notes. This figure presents coefficients of the effect of UI benefits on the costs of healthcare use separately by calendar week relative to the start of the unemployment spell, along with their 95 percent pointwise confidence intervals. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). The figure presents estimates from 52 weeks before the start of the spell up to 40 weeks after the start of the spell. Estimates are based on a local linear specification with a uniform kernel, quadratic bias correction, MSE-optimal bandwidths, and robust standard errors (Calonico et al. 2014b), controlling for pre-determined covariates. Confidence intervals are based on standard errors clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (Panel A), total costs of inpatient and outpatient care visits (Panel B), and total costs of drug purchases (Panel C).

Table 1: Descriptive statistics

	Analysis sample					Population 20–64 yo.				
A. Socioeconomic status, previous calendar year	Mean	Std. Dev.	P5	P50	P95	Mean	Std. Dev.	P5	P50	P95
Age	39.07	11.93	22.00	38.00	60.00	41.87	12.91	22.00	42.00	62.00
Share female	0.45	0.50	0.00	0.00	1.00	0.49	0.50	0.00	0.00	1.00
Share married or cohabiting	0.35	0.48	0.00	0.00	1.00	0.41	0.49	0.00	0.00	1.00
Share with children under age 18	0.38	0.49	0.00	0.00	1.00	0.37	0.48	0.00	0.00	1.00
Share with higher education	0.28	0.45	0.00	0.00	1.00	0.37	0.48	0.00	0.00	1.00
Share in manufacturing sector	0.23	0.42	0.00	0.00	1.00	0.11	0.31	0.00	0.00	1.00
Gross earnings (kSEK)	257.00	127.27	2.18	272.53	446.17	247.43	248.32	0.00	251.64	599.10
B. Unemployment spell										
Spell duration (weeks)	41.56	20.68	6.00	52.57	60.00					
Avg. replacement rate	0.67	0.13	0.45	0.67	0.80					
C. Health-related outcomes, previous 12 months										
Total costs of healthcare use (SEK)	14124.21	68144.08	0.00	671.49	62733.14	18735.18	122126.54	0.00	722.12	75671.18
Inpatient and outpatient care										
Total costs (SEK)										
In-/outpatient care	12070.02	65016.16	0.00	0.00	54968.06	16136.83	118521.85	0.00	0.00	63464.38
Inpatient care	8141.18	61731.43	0.00	0.00	38489.71	12059.01	115542.91	0.00	0.00	52929.83
Outpatient care	3928.84	9374.83	0.00	0.00	19270.27	4077.82	12180.13	0.00	0.00	19746.09
Number of visits										
In-/outpatient care	1.38	4.54	0.00	0.00	6.00	1.60	8.58	0.00	0.00	7.00
Inpatient care	0.43	3.35	0.00	0.00	2.00	0.59	7.67	0.00	0.00	3.00
Outpatient care	0.96	2.30	0.00	0.00	5.00	1.00	2.84	0.00	0.00	5.00
Drug purchases (SEK)										
Total costs	2054.19	15864.70	0.00	228.13	7654.33	2598.35	22356.70	0.00	251.28	10059.63
Benefit costs	1367.84	14924.50	0.00	0.00	5442.91	1870.02	21532.45	0.00	0.00	7788.72
Out-of-pocket costs	686.63	4704.52	0.00	215.73	2164.10	728.65	5046.66	0.00	236.38	2252.09
Observations			340,955					44,059,580		
Individuals			320,592					6,745,753		

Notes. This table provides descriptive statistics of selected variables for the analysis sample and the Swedish population aged 20–64 for the years 2007–2014. For the analysis sample, the unit of observation is an unemployment spell. For the population, the unit of observation is a person-year. Panel A shows statistics for selected socioeconomic covariates, measured in the previous (analysis sample) or the same calendar year (population). Gross earnings refer to the sum of salary and self-employment income. Panel B shows statistics related to the unemployment spell, only for the analysis sample. Duration of the unemployment spell is capped at 60 weeks. The average replacement rate refers to the overall replacement rate over the first 40 weeks of the unemployment spell. Panel C shows statistics for healthcare use (inpatient and outpatient care visits and drug purchases) over the last 365 days before the start of the unemployment spell (analysis sample) or over the previous calendar year (population). Total costs of healthcare use refer to the sum of the total costs of inpatient and outpatient care visits and drug purchases. Total costs of drug purchases refer to the sum of out-of-pocket costs and costs covered by prescription drug insurance. Earnings and costs are deflated using the overall CPI with 2020 as the reference year.

Table 2: Effect of Unemployment Benefits on Costs of Healthcare Use

	Total Healthcare Use		In- & Outpatient Visits		Inpatient Visits		Outpatient Visits		Drug Purchases	
First stage estimates										
Change in daily benefits per 1 SEK daily wage	-0.7614	-0.7416	-0.7180	-0.7306	-0.7323	-0.7379	-0.7309	-0.7445	-0.7385	-0.7367
	(0.0019)	(0.0032)	(0.0111)	(0.0073)	(0.0060)	(0.0037)	(0.0060)	(0.0028)	(0.0038)	(0.0041)
	[-0.7651,-0.7576]	[-0.7478,-0.7355]	[-0.7397,-0.6963]	[-0.7450,-0.7163]	[-0.7440,-0.7206]	[-0.7452,-0.7305]	[-0.7426,-0.7192]	[-0.7499,-0.7391]	[-0.7460,-0.7309]	[-0.7448,-0.7286]
Fuzzy RK estimates										
Change in costs per 1 SEK benefits	0.0004	0.0259	0.0223	0.0124	0.0207	0.0089	0.0064	0.0073	0.0038	0.0075
	(0.0172)	(0.0274)	(0.0826)	(0.0537)	(0.0389)	(0.0251)	(0.0116)	(0.0058)	(0.0038)	(0.0040)
	[-0.0334,0.0341]	[-0.0278,0.0795]	[-0.1396,0.1842]	[-0.0928,0.1177]	[-0.0555,0.0968]	[-0.0402,0.0580]	[-0.0164,0.0291]	[-0.0041,0.0186]	[-0.0038,0.0113]	[-0.0003,0.0154]
Implied elasticity										
% Change in costs per 1% change in benefits	0.0054	0.3943	0.4091	0.2276	0.6345	0.2737	0.2994	0.3410	0.4673	0.9348
	(0.2844)	(0.4470)	(1.5893)	(1.0478)	(1.2649)	(0.8257)	(0.5396)	(0.3294)	(0.4951)	(0.5084)
	[-0.5520,0.5628]	[-0.4818,1.2704]	[-2.7058,3.5240]	[-1.8261,2.2813]	[-1.8447,3.1137]	[-1.3447,1.8921]	[-0.7582,1.3569]	[-0.3046,0.9866]	[-0.5031,1.4377]	[-0.0617,1.9313]
Covariates		✓		✓		✓		✓		✓
Mean costs around kink point (SEK)	8818.9	8818.9	7333.1	7333.1	4383.8	4383.8	2861.3	2861.3	1085.5	1085.5
Bandwidth (SEK)	295.8	288.9	474.4	270.0	275.6	287.2	197.2	300.8	209.1	184.2
Number of observations	219,564	215,744	291,315	204,787	207,954	214,835	156,999	222,274	165,208	147,479

Notes. This table presents coefficients and standard errors of the effect of UI benefits on the costs of healthcare use. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification with a uniform kernel, MSE-optimal bandwidth, quadratic bias correction, and robust standard errors (Calonico et al. 2014b), with and without controlling for pre-determined covariates. Standard errors are clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (columns 2–3), total costs of inpatient and outpatient care visits (columns 4–5), total costs of inpatient care visits (columns 6-7), total costs of outpatient care visits (columns 8–9), and total costs of drug purchases (10–11). For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, rows 5–6 show the implied elasticity, row 7 indicates whether covariates are included, row 8 shows the outcome sample mean around the kink (using observations within 10 SEK of the kink), row 9 shows the MSE-optimal bandwidth, and row 10 shows the number of observations within the bandwidth. For elasticities, standard errors and confidence intervals are obtained via a non-parametric bootstrap with 100 replicates that samples unemployment spells with replacement.

Appendix

A Measuring Inpatient and Outpatient Care Use

A.1 Total Costs of Inpatient and Outpatient Care Visits

I calculate the total costs of a person's inpatient and outpatient care visits in two steps. First, I determine the average per-day costs of inpatient and outpatient care visits for each Major Diagnostic Category (MDC). Second, I calculate the total costs of inpatient and outpatient care visits by multiplying the average per-day costs by the length of the visit for each visit and summing over all visits.

1. Determining the average per-day costs of a visit. I calculate the average per-day costs of an inpatient or outpatient visit with a given MDC by using data on the total number of visits, average length of the visit, and average weights for all diagnosis-related groups (DRG) that fall under the MDC, and combining these with data on the cost per DRG point.

DRG codes are divided into codes only used in inpatient care and codes only used in outpatient care, but a given MDC code can contain both DRG codes used in inpatient care and outpatient care. Therefore, for each MDC I calculate average per-day costs separately for inpatient care and outpatient care visits.[35]

Denote the set of inpatient care DRG codes that belong to MDC m by $D(m,1)$, outpatient care DRG codes that belong to MDC m by $D(m,0)$, and fix a reference year t. I calculate the average per-day cost $c_{m,1}$ of an inpatient care visit with MDC code m as

$$c_{m,1} = \sum_{j \in D(m,1)} \underbrace{\left(\frac{N_j}{N_{m,1}}\right)}_{\substack{\text{DRG } j\text{'s share of all} \\ \text{inpatient visits with MDC } m}} \times \underbrace{\left(w_j \times \frac{c}{d_j}\right)}_{\substack{\text{Average per-day} \\ \text{costs of DRG } j}},$$

where N_j is the total number of inpatient care visits with DRG code j, $N_{m,1}$ is the total number of inpatient care visits with MDC code m, w_j is the weight for DRG j, d_j is the average duration (in days) of visits with DRG code j, and c is the cost per DRG point, all measured in the reference year. I define the average per-day cost $c_{m,0}$ of an outpatient care

35. DRG codes used in outpatient care are further divided into codes used in primary care and codes used in specialized outpatient care. Since the Patient Register data does not include primary care visits, I only consider DRG codes used in specialized outpatient care.

visit with MDC code m analogously, assuming that $d_j = 1$ for each DRG code $j \in D(m,0)$.

Appendix Table 2 shows the resulting average per-day costs of inpatient and outpatient care visits for all 29 MDC codes used in Sweden during my study period. For example, for MDC code 05 ("Diseases of the circulatory system"), the average per-day cost was 17,828 SEK for inpatient care visits and 5,181 SEK for outpatient care visits. For all MDC codes except for 0 ("Pre-MDC") and 25 ("HIV infection and HIV-related diseases"), I measure costs using 2020 as the reference year. MDC codes 0 and 25 were only used until 2011 and 2005, respectively, so I use these years as the reference years for these two codes.

Even though data on average costs, number of visits, and average visit lengths are published annually for each DRG code, I measure average costs using a fixed reference year for two reasons. First, I only have access to data on average costs of each DRG code for both inpatient and outpatient care from the year 2020 onwards. Second, using a fixed reference year ensures that any dynamic effects of UI generosity on costs of healthcare use in Section 5 reflect changes in the intensity and type of healthcare use, rather than changes over time in the costs of providing care in the healthcare system. The latter reason is analogous to e.g. the common practice of deflating measures of consumption expenditures using the consumer price index.

2. Determining total costs of all visits. Consider a healthcare visit j that appears in the Patient Register data. In the data, I observe whether the visit is an inpatient care visit ($I(j) = 1$) or outpatient care visit ($I(j) = 0$), the visit's MDC code $m(j)$, its admission date D_j^{start}, and for inpatient care visits its discharge date D_j^{end}. For outpatient visits, I assume admission and discharge dates coincide, that is, $D_j^{start} = D_j^{end}$.

Fix some interval of dates $D = [D^{min}, D^{max}]$ for $D^{min} < D^{max}$ (say, the first and last day of a calendar week). For a visit j that overlaps with period D (i.e., $[D^{min}, D^{max}] \cap [D_j^{start}, D_j^{end}] \neq \emptyset$), I calculate the total costs C_j^D of visit j during the period D by multiplying the per-day costs of visit j by the number days of visit j that fall within period D, that is,

$$C_j^D = \left[1 + \min(D^{max}, D_j^{end}) - \max(D^{min}, D_j^{start})\right] \times c_{m(j),i},$$

where $i = 1$ if visit j is an inpatient care visit and $i = 0$ if it is an outpatient care visit. Denote the set of visits that overlap with period D by J^D. I calculate the total costs of all visits during the period D as $C^D = \sum_{j \in J^D} C_j^D$.

For individuals in the analysis sample (see Section 3), I cannot assign costs for 0.6 percent of inpatient care visits and 1.1 percent of outpatient care visits. For the population aged 20–64 in Table 1, the corresponding shares are 0.6 and 3.2 percent, respectively. In virtually all cases the reason for not being able to assign costs is that the MDC code for the visit is missing, since I can assign costs for more than 99.99 percent of all visits with a non-missing MDC code. I assign zero costs for all visits for which I cannot assign costs, so my measure of the total costs of inpatient and outpatient care visits can be seen as a lower bound.

A.2 Number of Inpatient Care and Outpatient Care Visits

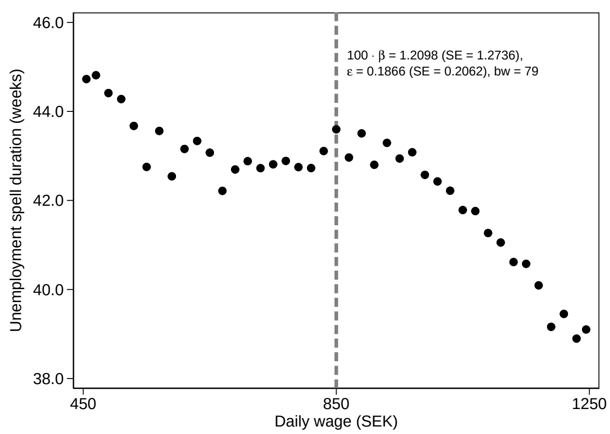
Fix some interval of dates $D = [D^{min}, D^{max}]$ and denote the set of visits that overlap with period D by J^D. I define the total number N^D of in-/outpatient care visits during period D as the number of visits with an admission date during period D, that is $N^D = \sum_{j \in J^D} 1\left\{D_j^{start} \in D\right\}$. I note that N^D also includes visits for which I cannot measure costs.

B Supplementary Figures and Tables

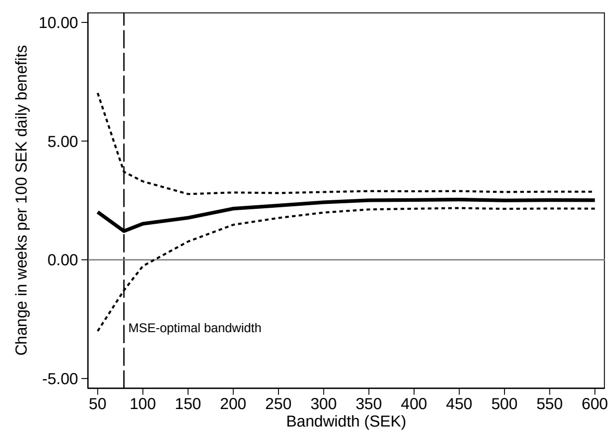
Appendix Figure 1: Spell Duration Around the Kink Point

Panel A: Unemployment Spell Duration

(i) Binned scatterplot

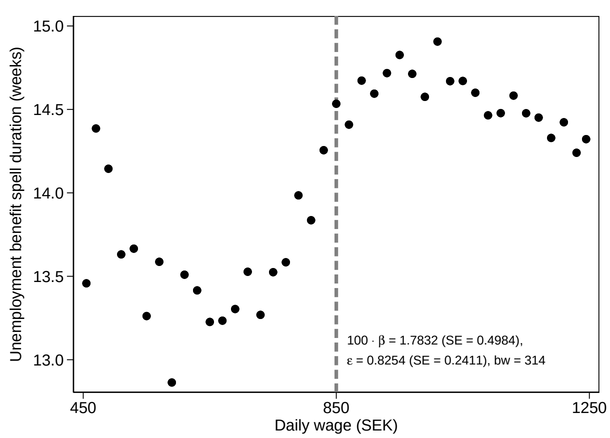


(ii) Sensitivity to bandwidth

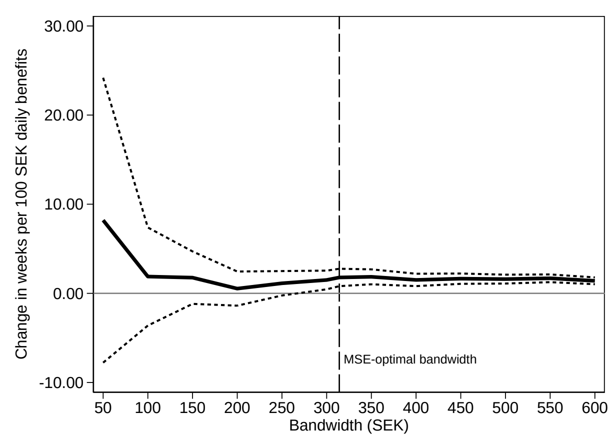


Panel B: Unemployment Benefit Spell Duration

(i) Binned scatterplot



(ii) Sensitivity to bandwidth

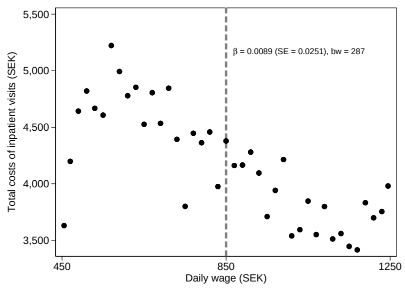


Notes. This figure shows binned scatterplots and coefficients of the effect of unemployment benefits on the duration of the unemployment spell (Panel A) and the duration of the unemployment benefit spell (Panel B). The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). The outcomes are the duration of the unemployment spell (Panel A) and the duration of the unemployment benefit payment spell (Panel B), both measured in weeks. Unemployment spell duration is censored at 60 weeks. In each panel, the left column shows a binned scatterplot of the outcome against the daily wage using a bandwidth of 400 SEK and 20 SEK bins. Binned scatterplots also report the estimated effect of unemployment benefits on spell duration and its standard error, the implied elasticity and its standard error, and the bandwidth used for estimation. The right column shows coefficients of the effect of unemployment benefits on spell duration for varying bandwidth choices along with their 95 percent pointwise confidence intervals. Estimates are based on a local linear specification with a uniform kernel, quadratic bias correction, and robust standard errors (Calonico et al. 2014b), controlling for pre-determined covariates. Confidence intervals are based on standard errors clustered at the individual level. For elasticities, standard errors are obtained via a non-parametric bootstrap with 100 replicates that samples unemployment spells with replacement. The dashed vertical lines indicate the MSE-optimal bandwidth (Calonico et al. 2014b), which is used for the main estimates.

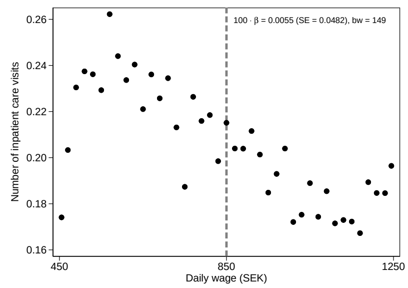
Appendix Figure 2: Inpatient and Outpatient Care Use Around the Kink Point

Panel A: Inpatient Care Visits

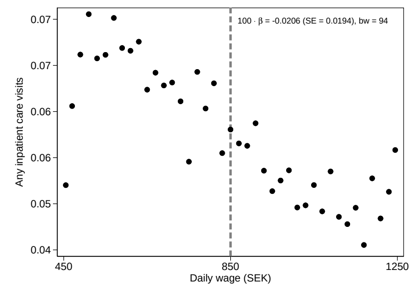
(i) Total costs



(ii) Number of visits

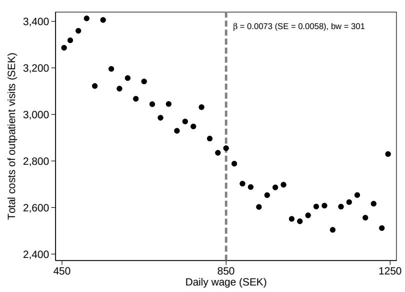


(iii) Any visits

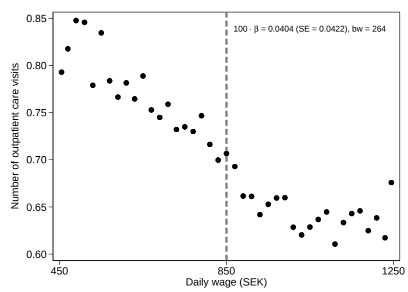


Panel A: Outpatient Care Visits

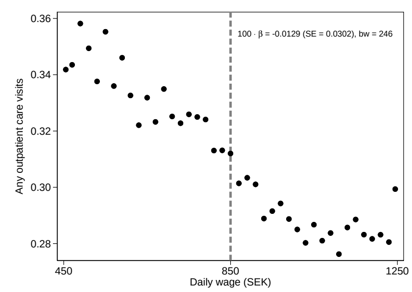
(i) Total costs



(ii) Number of visits



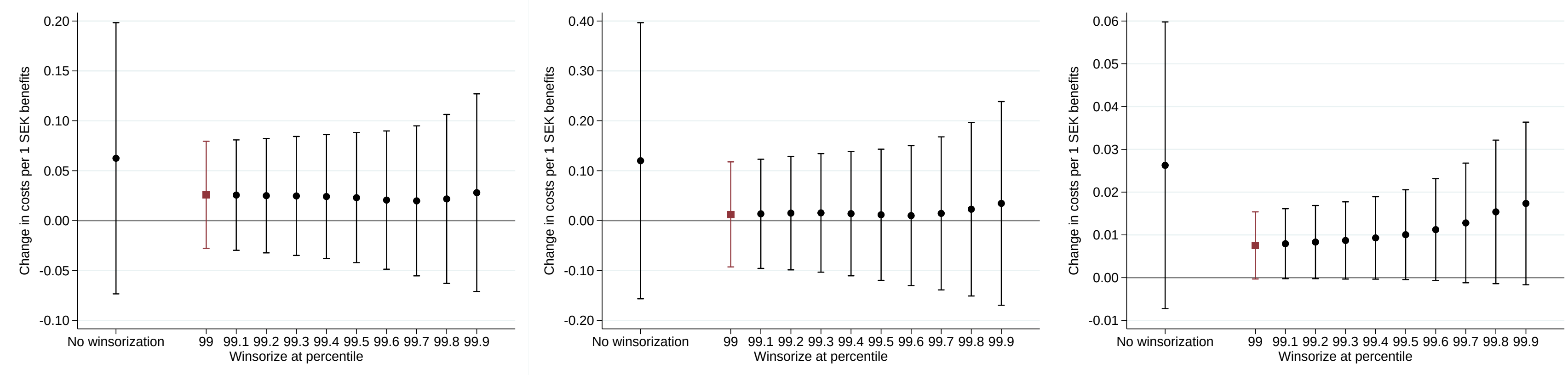
(iii) Any visits



Notes. This figure shows binned scatterplots of inpatient and outpatient care use as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). Panel A focuses on inpatient care visits, while Panel B focuses on outpatient care visits. In each panel, the outcomes are the total costs of visits (left column), the total number of visits (middle column), and an indicator for having any visits (right column). In each column, the unit of observation is an unemployment spell. Each plot also reports the estimated effect of unemployment benefits on the outcome of interest, its standard error, and the bandwidth used for estimation. Estimates are based on a local linear specification with a uniform kernel, MSE-optimal bandwidth, quadratic bias correction, and robust standard errors (Calonico et al. 2014b), controlling for pre-determined covariates. Standard errors clustered at the individual level.

Appendix Figure 3: Comparing Estimated Effects on Healthcare Use With vs. Without Winsorization

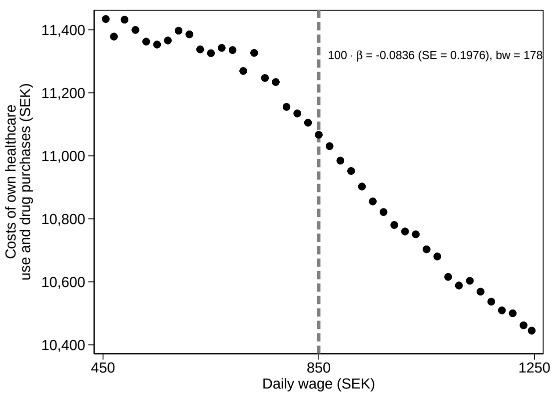
(a) Total Healthcare Use (b) Inpatient and Outpatient Care Visits (c) Drug Purchases



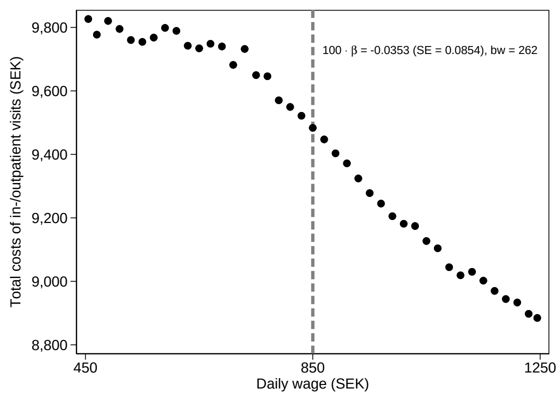
Notes. This figure presents the estimated coefficients of the effect of unemployment benefits on the costs of healthcare use when healthcare costs are vs. are not winsorized. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). The figure reports local linear estimates with a uniform kernel, quadratic bias correction, and robust pointwise 95 percent confidence intervals (Calonico et al. 2014a), controlling for pre-determined covariates. Confidence intervals are based on standard errors clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (Panel A), total costs of inpatient and outpatient care visits (Panel B), and total costs of drug purchases (Panel C). In each panel, the horizontal axis indicates the level at which the outcome variable is winsorized, ranging from no winsorization to winsorizing costs above the p^{th} percentile, where p varies from the 99th to 99.9th percentile. The estimates shown with red square markers indicate the baseline estimates shown in Table 2, which winsorize costs above the 99th percentile. To aid comparison with the baseline estimates, each point estimate uses the same bandwidth as for the baseline estimates (see Table 2). This ensures that estimates and their confidence intervals only differ because of the level of winsorization, rather than differences in the data-driven choice for the bandwidth.

Appendix Figure 4: Predicted Healthcare Use Around the Kink Point

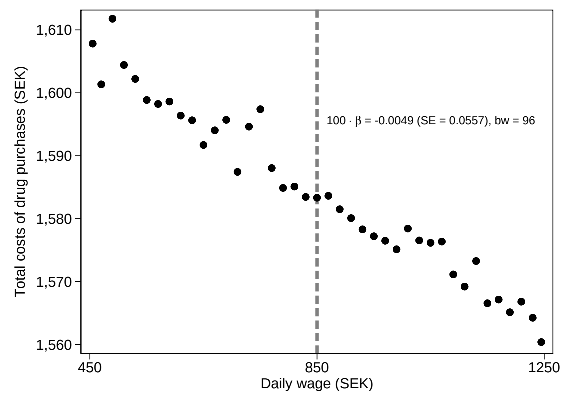
(a) Total Healthcare Use



(b) Inpatient and Outpatient Care Visits



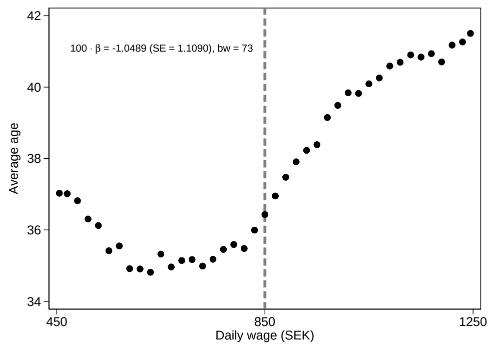
(c) Drug Purchases



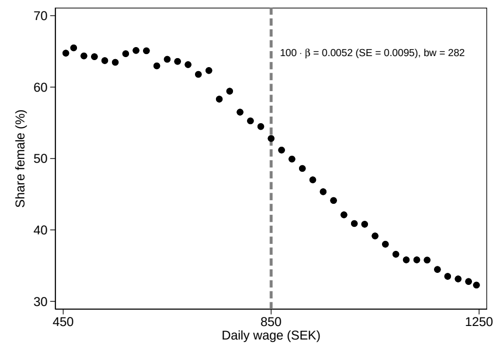
Notes. This figure shows binned scatterplots of predicted costs of healthcare use as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). The outcomes are the predicted total costs of inpatient and outpatient care visits and drug purchases (left column), the predicted total costs of inpatient and outpatient care visits (middle column), and the predicted total costs of drug purchases (right column). In each column, the unit of observation is an unemployment spell. Predicted outcomes are fitted values obtained after regressing each outcome against indicators for being married or cohabiting, female, having higher education, and having children under age 18 at home, indicators for age, indicators for the region of residence, and indicators for the industry of the highest-paying employer (incl. missing industry as a separate category). Appendix Table 6 presents the estimation results from these regressions. A person is defined as having higher education if s/he has completed at least one semester of post-secondary education. Control variables are measured in the calendar year before the start of the unemployment spell. Each plot also reports the estimated effect of unemployment benefits on the outcome of interest, its standard error, and the bandwidth used for estimation. Estimates are based on a local linear specification with a uniform kernel, MSE-optimal bandwidth, quadratic bias correction, and robust standard errors (Calonico et al. 2014b), without controlling for pre-determined covariates. Standard errors clustered at the individual level.

Appendix Figure 5: Pre-Determined Covariates Around Daily Wage Kinks

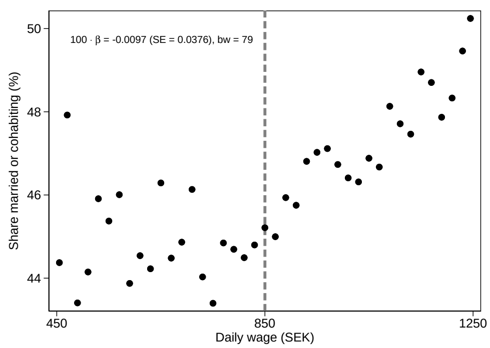
(a) Average age



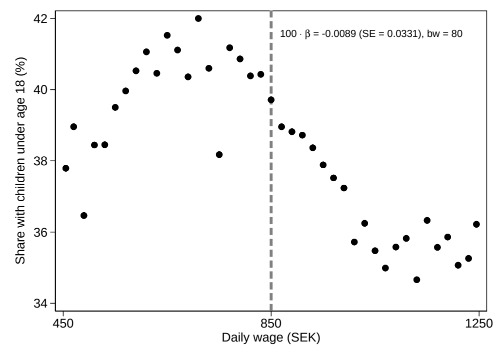
(b) Share female



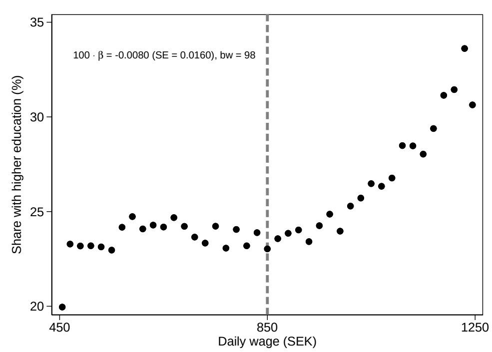
(c) Share married or cohabiting



(d) Share with children under age 18



(e) Share with higher education



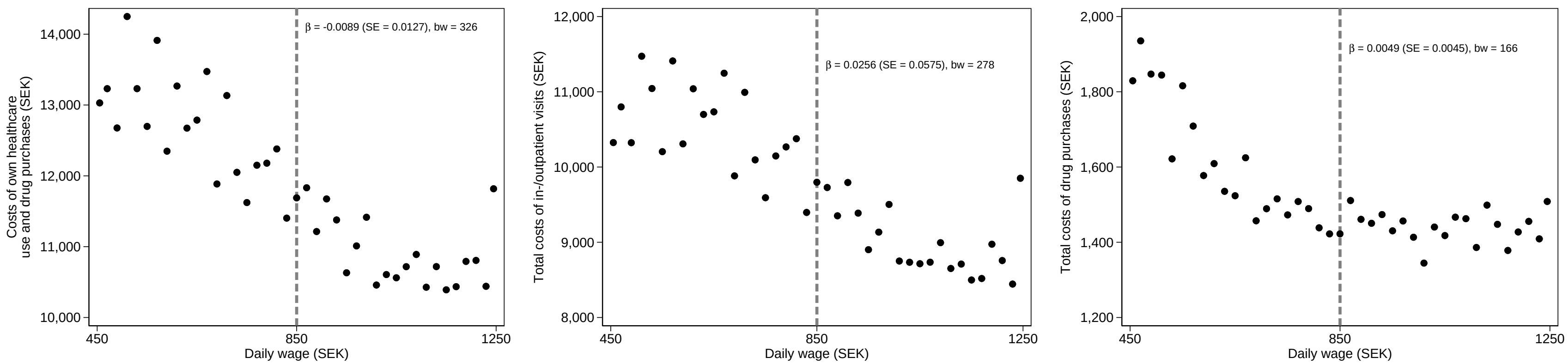
Notes. This figure shows binned scatterplots of selected pre-determined covariates as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). Each covariate is measured in the calendar year before the start of the unemployment spell. A person is defined as having higher education if s/he has completed at least one semester of post-secondary education. Each plot also reports the estimated effect of unemployment benefits on the covariate of interest, its standard error, and the bandwidth used for estimation. Estimates are based on a local linear specification with a uniform kernel, MSE-optimal bandwidth, quadratic bias correction, and robust standard errors (Calonico et al. 2014b). Standard errors clustered at the individual level.

Appendix Figure 6: Pre-Unemployment Healthcare Use Around Daily Wage Kinks

(a) Total Healthcare Use

(b) Inpatient and Outpatient Care Visits

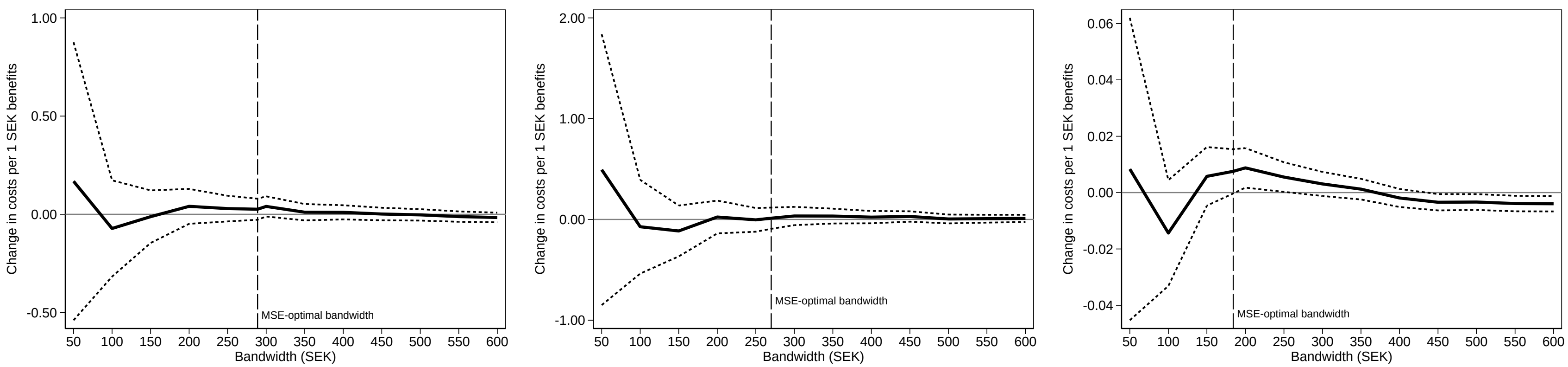
(c) Drug Purchases



Notes. This figure shows binned scatterplots of outcomes measured before the start of the unemployment spell as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells starting between March 5, 2007 and July 14, 2014 (see Section 3). Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (left column), total costs of inpatient and outpatient care visits (middle panel), and total costs of drug purchases (right panel). For each outcome, costs are measured over the last 52 calendar weeks prior to the start of the unemployment spell and deflated using the overall CPI with 2020 as the reference year. Each plot also reports the estimated effect of unemployment benefits on the outcome of interest, its standard error, and the bandwidth used for estimation. Estimates are based on a local linear specification with a uniform kernel, MSE-optimal bandwidth, quadratic bias correction, and robust standard errors (Calonico et al. 2014b), controlling for pre-determined covariates. Standard errors clustered at the individual level.

Appendix Figure 7: Effects on Total Costs of Healthcare Use for Varying Bandwidths

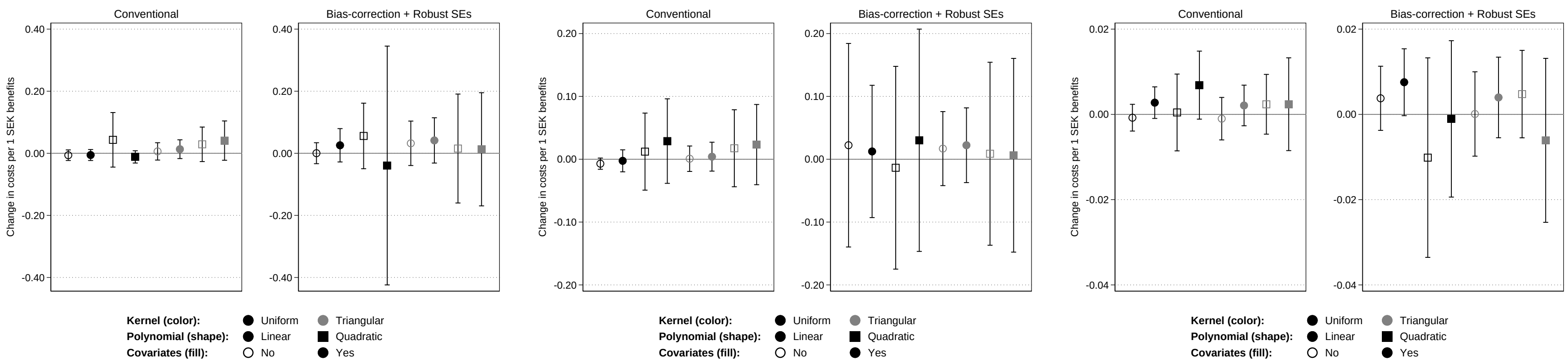
(a) Total Healthcare Use (b) Inpatient and Outpatient Care Visits (c) Drug Purchases



Notes. This figure presents coefficients of the effect of unemployment benefits on the costs of healthcare use for varying bandwidth choices along with their 95 percent pointwise confidence intervals. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). The figure reports local linear estimates with a uniform kernel, quadratic bias correction, and robust pointwise 95 percent confidence intervals (Calonico et al. 2014a), controlling for pre-determined covariates. Confidence intervals are based on standard errors clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (left panel), total costs of inpatient and outpatient care visits (middle panel), and total costs of drug purchases (right panel). The dashed vertical lines indicate the MSE-optimal bandwidths (Calonico et al. 2014b), which are used for the main estimates.

Appendix Figure 8: Effects on Healthcare Use for Alternative Specifications

(a) Total healthcare use (b) Inpatient and outpatient care visits (c) Drug purchases



Notes. This figure presents coefficients of the effect of unemployment benefits on the costs of healthcare use for alternative specifications, along with their 95 percent pointwise confidence intervals. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). Specifications vary by (i) whether they use a uniform (black markers) or triangular (gray markers) kernel, (ii) whether they use a local linear (circle markers) or local quadratic (square markers) estimator, and (iii) whether they include (filled markers) or exclude (hollow markers) pre-determined covariates as controls. Each plot also presents conventional estimates that do not use bias-correction ("Conventional") and bias-corrected estimates with robust standard errors ("Bias-correction + Robust SEs"). Each specification uses an MSE-optimal bandwidth following (Calonico et al. 2014a). Confidence intervals are based on standard errors clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (left panel), total costs of inpatient and outpatient care visits (middle panel), and total costs of drug purchases (right panel). In each panel, the first two bias-corrected estimates from the left correspond to the baseline estimates shown in Table 2.

Appendix Table 1: Mapping Job Seeker and Deregistration Codes in the PES Data

(i) Job seeker codes

Code	Name	Employed	Unemployed	PES Program	Other registered	Other deregistered
0	Unknown	–	–	–	X	–
11	Openly unemployed	–	X	–	–	–
12	Unemployed, guidance service	–	X	–	–	–
13	Unemployed, waiting for decided action	–	X	–	–	–
14	Jobseeker with obstacles	–	–	–	X	–
15	Municipal effort	–	–	–	X	–
20	Establishment job	X	–	–	–	–
21	Part-time unemployed	X	–	–	–	–
22	Hourly employee	X	–	–	–	–
23	Professional fisherman	–	–	–	X	–
24	Protected work Samhall (temporary employees)	X	–	–	–	–
28	The establishment program, mapping	–	–	X	–	–
30	Introduction job	X	–	–	–	–
31	Temporary work	X	–	–	–	–
33	New start job	X	–	–	–	–
34	Outgoing EU/EEA job seeker	–	–	–	X	–
35	Change-seeking Samhall	X	–	–	–	–
38	Wage subsidy for development in employment	X	–	–	–	–
39	Wage subsidy for security in employment	X	–	–	–	–
40	Professional introduction	X	–	–	–	–
41	Change-seeking	X	–	–	–	–
42	Wage subsidy for employment	X	–	–	–	–
43	Publicly protected work	X	–	–	–	–
44	Graduate job	–	–	X	–	–
46	Support for starting a business	–	–	X	–	–
47	General employment support	X	–	–	–	–
48	Enhanced employment support (2-year enrollment)	X	–	–	–	–
49	Special employment support	X	–	–	–	–
50	Modern preparedness jobs	X	–	–	–	–
51	Extra services	X	–	–	–	–
52	Working life development	–	–	X	–	–
53	Temporary education	–	–	X	–	–
54	Work practice	–	–	X	–	–
55	Workplace introduction	–	–	X	–	–
57	Project work (unemployment benefit)	–	–	X	–	–
58	Wage subsidy for development in work at Samhall	X	–	–	–	–
60	Interpraktik	–	–	X	–	–
61	Youth practice	–	–	X	–	–
62	Academic internship	–	–	X	–	–
63	Youth introduction with education grant	–	–	X	–	–
64	Computer tech	–	–	X	–	–
65	Municipal youth program	–	–	X	–	–
66	Youth guarantee	–	–	X	–	–
68	The establishment program	–	–	X	–	–
69	Job guarantee for youth	–	–	X	–	–
70	Job and development guarantee	–	–	X	–	–
74	Mediation efforts	–	–	X	–	–
75	Project with labor market policy orientation	–	–	X	–	–
80	Preparatory measures	–	–	X	–	–
81	Labor market training	–	–	X	–	–
82	IT investment	–	–	X	–	–
83	Preparatory education	–	–	X	–	–
84	Deficiency training for employees	–	–	X	–	–
86	Validation	–	–	X	–	–
89	Off-year	–	–	–	X	–
91	Special category not included in statistics	–	–	–	X	–
95	Unemployed, revocation of decision	–	X	–	–	–
96	Unemployed, incorrect registration of decision	–	X	–	–	–
97	Unemployed, interruption/revocation of decision	–	X	–	–	–
98	Unemployed, completed decision period	–	X	–	–	–
99	Kalmarmodellen	–	–	–	X	–

(ii) Deregistration codes

Code	Name	Employed	Unemployed	PES Program	Other registered	Other deregistered
1	Got permanent employment	X	–	–	–	–
2	Got temporary employment	X	–	–	–	–
3	Got continued employment with the same employer	X	–	–	–	–
4	Got employment within Samhall	X	–	–	–	–
5	Contact terminated, other known cause	–	–	–	–	X
6	Contact terminated, unknown reason	–	–	–	–	X
7	Education other than labor market education	–	–	–	–	X

Notes. This table shows how the job seeker categories and deregistration codes in the Public Employment Service data (AF 2024b, 2024c) are mapped to employment, unemployment, participation in labor market programs, others registered at PES, and those deregistered from the PES.

Appendix Table 2: Total Costs per Day of Care in 2020, Separately by Major Diagnostic Category (MDC)

		Total costs per day of care (SEK)	
MDC	Name	Inpatient care	Outpatient care
00	Pre-MDC	23,326	—
01	Diseases of the nervous system	16,858	5,355
02	Diseases of the eye and adnexa	22,678	3,315
03	Diseases of the ear, nose, mouth, and throat	22,250	4,411
04	Diseases of the respiratory system	12,947	5,769
05	Diseases of the circulatory system	17,828	5,181
06	Diseases of the digestive system	17,473	5,480
07	Diseases of the liver, biliary tract, and pancreas	17,176	6,816
08	Diseases of the musculoskeletal system and connective tissue	26,465	4,937
09	Diseases of the skin and subcutaneous tissue	17,549	3,653
10	Endocrine, nutritional and metabolic diseases	21,054	4,194
11	Diseases of the genitourinary system	15,693	5,227
12	Diseases of the male reproductive system	36,186	5,105
13	Diseases of the female reproductive system	35,067	4,088
14	Pregnancy, childbirth and the puerperium	19,245	2,774
15	Newborns and certain perinatal conditions	16,090	3,672
16	Blood diseases and immune disorders	13,076	5,763
17	Myeloproliferative diseases and unspecified tumors	15,553	6,384
18	Infectious and parasitic diseases including HIV	12,544	4,680
19	Mental disorders, behavioral disorders and alcohol- or drug-related disorders	19,751	3,628
21	Injuries, poisonings and toxic effects	21,182	4,359
22	Burns	21,118	4,186
23	Other and unspecified health problems	14,969	3,592
24	Multiple trauma excluding superficial injuries and wounds	20,627	5,956
30	Diseases of the breast	77,630	8,881
40	MDC-wide problems in outpatient care	—	4,887
50	Provider-dependent groups in outpatient care	—	4,378
99	Unspecified or erroneous information	11,945	3,053

Notes. This table shows a list of the 28 Major Diagnostic Categories (MDC) used in Sweden during my study period. Note that MDC code 00 was used until 2011. For each MDC, I also report the average per-day total care costs, separately for inpatient and outpatient care. For all MDC codes except for 0 and 25, I measure average costs in 2020. For MDC codes 00 and 25, I measure average costs in the last year the MDC code was used. Costs are deflated using the overall CPI with 2020 as the reference year. Appendix A describes in detail how I calculate the average per-day costs.

Appendix Table 3: Effect of Unemployment Benefits on Unemployment Spell Duration

	Unemployment benefit spell duration		Unemployment spell duration	
First stage estimates				
Change in daily benefits per 1 SEK daily wage	-0.739***	-0.728***	-0.724***	-0.723***
	(0.004)	(0.008)	(0.010)	(0.011)
	[-0.746,-0.732]	[-0.744,-0.712]	[-0.743,-0.704]	[-0.744,-0.702]
Fuzzy RK estimates				
Change in spell length (weeks) per 100 SEK daily benefits	1.040***	1.783***	0.701	1.210
	(0.255)	(0.498)	(1.286)	(1.274)
	[0.541,1.539]	[0.806,2.760]	[-1.821,3.222]	[-1.286,3.706]
Implied elasticity				
% Change in spell length per 1% increase in daily benefits	0.481***	0.825***	0.108	0.187
	(0.109)	(0.241)	(0.204)	(0.206)
	[0.268,0.695]	[0.353,1.298]	[-0.292,0.508]	[-0.217,0.591]
Covariates		✓		✓
Mean spell length around kink point (weeks)	14.5	14.5	43.6	43.6
Bandwidth (SEK)	221.1	314.1	98.1	79.0
Number of observations	173,611	229,551	80,973	65,540

Notes. This table presents coefficients and standard errors of the effect of unemployment benefits on the duration of the unemployment benefit spell and the unemployment spell. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). Estimates are based on a local linear specification with a uniform kernel, quadratic bias-correction, MSE-optimal bandwidth, and robust standard errors (Calonico et al. 2014b), with and without controlling for pre-determined covariates. Standard errors are clustered at the individual level. The outcomes are the duration of the unemployment benefit payment spell (columns 1–2) and the duration of the unemployment spell (columns 3–4), both measured in weeks. Unemployment spell duration is censored at 60 weeks. For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, and rows 5–6 show the implied elasticity. For elasticities, standard errors are obtained via a non-parametric bootstrap with 100 replicates that samples unemployment spells with replacement. Row 7 indicates whether covariates are included, row 8 shows the mean spell length around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth.

Appendix Table 4: Effect of Unemployment Benefits on Number of Inpatient and Outpatient Care Visits

	In- & Outpatient visits		Inpatient visits		Outpatient visits	
First stage estimates						
Change in daily benefits per 1 SEK daily wage	-0.7407	-0.7297	-0.7308	-0.7276	-0.7325	-0.7372
	(0.0035)	(0.0085)	(0.0062)	(0.0078)	(0.0064)	(0.0042)
	[-0.7476,-0.7337]	[-0.7463,-0.7131]	[-0.7429,-0.7186]	[-0.7429,-0.7123]	[-0.7451,-0.7200]	[-0.7455,-0.7290]
Fuzzy RK estimates						
Change in number of visits per 100 SEK daily benefits	0.0434	-0.0054	0.0245	0.0055	0.0336	0.0404
	(0.0502)	(0.1146)	(0.0375)	(0.0482)	(0.0616)	(0.0422)
	[-0.0550,0.1418]	[-0.2301,0.2193]	[-0.0491,0.0980]	[-0.0890,0.0999]	[-0.0871,0.1543]	[-0.0423,0.1232]
Implied elasticity						
% Change in number of visits per 1% change in daily benefits	0.3062	-0.0379	0.7631	0.1701	0.3190	0.3838
	(0.3785)	(0.8333)	(1.2235)	(1.6340)	(0.5673)	(0.3994)
	[-0.4357,1.0481]	[-1.6712,1.5953]	[-1.6349,3.1610]	[-3.0325,3.3727]	[-0.7930,1.4309]	[-0.3991,1.1666]
Covariates		✓		✓		✓
Mean number of visits around kink	1.0	1.0	0.2	0.2	0.7	0.7
Bandwidth (SEK)	163.8	223.1	219.6	148.7	200.6	264.1
Number of observations	132,086	175,018	172,661	120,615	159,285	201,338

Notes. This table presents coefficients and standard errors of the effect of unemployment benefits on the number of inpatient and outpatient care visits. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification with a uniform kernel, quadratic bias-correction, MSE-optimal bandwidth, and robust standard errors (Calonico et al. 2014b), with and without controlling for pre-determined covariates. Standard errors are clustered at the individual level. The outcomes are the number of inpatient and outpatient care visits (columns 1–2), the number of inpatient care visits (columns 3–4), and the number of outpatient care visits (columns 5–6). For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, and rows 5–6 show the implied elasticity. For elasticities, standard errors are obtained via a non-parametric bootstrap with 100 replicates that samples unemployment spells with replacement. Row 7 indicates whether covariates are included, row 8 shows the outcome mean around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth.

Appendix Table 5: Effect of Unemployment Benefits on Healthcare Use at the Extensive Margin

	Total healthcare use		In- & Outpatient visits		Inpatient visits		Outpatient visits		Drug purchases	
First stage estimates										
Change in daily benefits per 1 SEK daily wage	-0.7349	-0.7331	-0.7363	-0.7147	-0.7206	-0.7186	-0.7363	-0.7243	-0.7340	-0.7230
	(0.0056)	(0.0071)	(0.0049)	(0.0097)	(0.0120)	(0.0130)	(0.0048)	(0.0101)	(0.0061)	(0.0115)
	[-0.7458,-0.7239]	[-0.7469,-0.7193]	[-0.7459,-0.7266]	[-0.7338,-0.6957]	[-0.7442,-0.6970]	[-0.7441,-0.6931]	[-0.7457,-0.7269]	[-0.7441,-0.7045]	[-0.7460,-0.7219]	[-0.7455,-0.7006]
Fuzzy RK estimates										
Percentage point change per 100 SEK daily benefits	-0.6293	-0.3540	0.6572	-0.8710	-0.4992	-2.0594	1.0551	-1.2925	-1.5558	-4.0380
	(1.7831)	(2.1550)	(1.5769)	(3.0809)	(1.7405)	(1.9408)	(1.5215)	(3.0235)	(1.9869)	(3.4496)
	[-4.1242,2.8655]	[-4.5777,3.8697]	[-2.4335,3.7479]	[-6.9094,5.1674]	[-3.9105,2.9121]	[-5.8633,1.7445]	[-1.9269,4.0371]	[-7.2184,4.6334]	[-5.4502,2.3385]	[-10.7991,2.7231]
Implied elasticity										
Percentage change per 1% change in daily benefits	-0.0645	-0.0363	0.1359	-0.1801	-0.5765	-2.3785	0.2271	-0.2782	-0.1754	-0.4552
	(0.1747)	(0.2156)	(0.3362)	(0.6628)	(2.2080)	(2.5880)	(0.3280)	(0.6664)	(0.2228)	(0.3770)
	[-0.4069,0.2779]	[-0.4588,0.3862]	[-0.5231,0.7949]	[-1.4792,1.1190]	[-4.9042,3.7511]	[-7.4508,2.6939]	[-0.4157,0.8699]	[-1.5844,1.0280]	[-0.6121,0.2613]	[-1.1941,0.2838]
Covariates		✓		✓		✓		✓		✓
Mean share around kink (%)	65.6	65.6	32.5	32.5	5.8	5.8	31.2	31.2	59.6	59.6
Bandwidth (SEK)	151.9	132.9	182.7	516.3	134.8	94.2	193.4	245.8	127.5	117.8
Number of observations	123,183	108,540	146,363	300,961	110,059	77,789	154,063	189,897	104,423	96,778

Notes. This table presents coefficients and standard errors of the effect of unemployment benefits on using inpatient and outpatient care at the extensive margin. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification with a uniform kernel, quadratic bias-correction, MSE-optimal bandwidth, and robust standard errors (Calonico et al. 2014b), with and without controlling for pre-determined covariates. Standard errors are clustered at the individual level. The outcomes are an indicator for having any inpatient and outpatient care visits (columns 1–2), an indicator for having any inpatient care visits (columns 3–4), and an indicator for having any outpatient care visits (columns 5–6). For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, and rows 5–6 show the implied elasticity. For elasticities, standard errors are obtained via a non-parametric bootstrap with 100 replicates that samples unemployment spells with replacement. Row 7 indicates whether covariates are included, row 8 shows the outcome mean around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth.

Appendix Table 6: Estimates from Regressions Used to Construct Covariate Indices

	Total Healthcare Use	Inpatient and Outpatient Care	Drug Purchases
Constant	10760.07	9076.69	1683.38
	(201.37)	(194.57)	(40.07)
Any higher education	-545.97	-523.97	-22.00
	(256.21)	(243.94)	(62.15)
Married or cohabiting	-1289.55	-1207.58	-81.97
	(272.34)	(263.84)	(49.09)
Female	2681.78	2541.71	140.06
	(216.85)	(205.50)	(58.08)
Any children under age 18	-1008.85	-676.77	-332.07
	(290.85)	(281.48)	(56.27)
Region FEs	✓	✓	✓
Age FEs	✓	✓	✓
Industry FEs	✓	✓	✓
Observations	340,772	340,772	340,772

Notes. This table presents estimated coefficients and their standard errors, clustered at the individual level, from regressions of outcomes related to healthcare use against a set of pre-determined covariates. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (column 1), the total costs of inpatient and outpatient care visits (column 2), and the total costs of drug purchases (column 3). Outcomes are measured over the first 40 weeks since the start of the unemployment spell. All covariates are measured in the calendar year before the start of the unemployment spell. I construct covariate indices for each outcome as the fitted values from each regression and use these indices in Appendix Figure 4. The row "Region FEs" refers to indicators for the region (*kommun*) of residence. The row "Industry FEs" refers to indicators for the industry of the highest-paying employer, also including a separate indicator for missing industry code. A person is defined as having any higher education if s/he has completed at least one semester of post-secondary education.

Appendix Table 7: Effect of Unemployment Benefits on Predicted Healthcare Use

	Total Healthcare Use	Inpatient and Outpatient Care	Drug purchases
First stage estimates			
Change in daily benefits per 1 SEK daily wage	-0.7397	-0.8093	-0.7245
	(0.00379)	(0.00201)	(0.00917)
Fuzzy RK estimates			
Change in predicted costs per 100 SEK benefits	-0.0836	-0.0353	-0.0049
	(0.19755)	(0.08539)	(0.05570)
Mean predicted costs around kink	11066.6	9483.3	1583.3
Bandwidth	177.6	262.2	96.1
Number of observations	142,617	200,093	79,346

Notes. This table presents coefficients and standard errors of the effect of unemployment benefits on predicted costs of healthcare use. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification with a uniform kernel, quadratic bias-correction, MSE-optimal bandwidth, and robust standard errors (Calonico et al. 2014b), without controlling for pre-determined covariates. Standard errors are clustered at the individual level. Outcomes are the predicted total costs of inpatient and outpatient care visits and drug purchases (column 2), predicted total costs of inpatient and outpatient care visits (column 3), and predicted total costs of drug purchases (column 4). For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, row 5 shows the outcome mean around the kink (using observations within 10 SEK of the kink), row 6 shows the MSE-optimal bandwidth, and row 7 shows the number of observations within the bandwidth. The predicted outcomes are fitted values obtained after regressing each outcome against a set of pre-determined covariates. Appendix Table 6 presents the estimation results from these regressions.

Appendix Table 8: Effect of Unemployment Benefits on Pre-Determined Covariates

	Average age	Share female	Share with higher education	Share with partner	Share with children
First stage estimates					
Change in daily benefits per 1 SEK daily wage	-0.7142	-0.7458	-0.7324	-0.7204	-0.7236
	(0.01575)	(0.00268)	(0.00601)	(0.01226)	(0.01098)
Fuzzy RK estimates					
Change in outcome per 100 SEK daily benefits	-1.0489	0.0052	-0.0080	-0.0097	-0.0089
	(1.10899)	(0.00947)	(0.01597)	(0.03765)	(0.03313)
Covariate mean around kink	36.438	0.528	0.230	0.452	0.397
Bandwidth (SEK)	73.3	281.5	97.6	79.5	80.3
Number of observations	60,737	211,334	80,602	65,855	66,532

Notes. This table presents coefficients and standard errors of the effect of unemployment benefits on selected pre-determined covariates, all measured in the calendar year before the start of the unemployment spell. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification with a uniform kernel, quadratic bias-correction, MSE-optimal bandwidth, and robust standard errors (Calonico et al. 2014b). Standard errors are clustered at the individual level. Outcomes are age (column 2) and indicators for being female (column 3), having higher education (column 4), being married or cohabiting (column 5), and having children under the age of 18 at home (column 6). For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, row 5 shows the sample mean of the covariate around the kink (using observations within 10 SEK of the kink), row 6 shows the MSE-optimal bandwidth, and row 7 shows the number of observations within the bandwidth.

Appendix Table 9: Effect of Unemployment Benefits on Pre-Unemployment Healthcare Use

	Total healthcare use	Inpatient and outpatient care	Drug purchases
First stage estimates			
Change in daily benefits per 1 SEK daily wage	-0.8633	-0.7307	-0.7382
	(0.00192)	(0.00779)	(0.00447)
Fuzzy RK estimates			
Change in costs per 1 SEK benefits	-2.3049	6.6501	1.2644
	(3.29404)	(14.95972)	(1.16143)
Outcome mean around kink	11724.4	9829.8	1426.0
Bandwidth	326.0	278.1	166.1
Number of observations	235,478	209,367	133,782

Notes. This table presents coefficients and standard errors of the effect of unemployment benefits on healthcare use measured in the calendar year before the start of the unemployment spell. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification with a uniform kernel, quadratic bias-correction, MSE-optimal bandwidth, and robust standard errors (Calonico et al. 2014b), controlling for pre-determined covariates. Standard errors are clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (column 2), total costs of inpatient and outpatient care visits (column 3), and total costs of drug purchases (column 4). For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, row 5 shows the outcome sample mean around the kink point (using observations within 10 SEK of the kink), row 6 shows the MSE-optimal bandwidth, and row 7 shows the number of observations within the bandwidth.

Appendix Table 10: Definitions for Healthcare Use Categories

Panel A. Inpatient and Outpatient Visits

Category	ICD-10 codes	Notes
Cancer	C00-D48	Adapted from Kuhn et al. (2009, Table A.1)
Heart	I00–I52	Adapted from Kuhn et al. (2009, Table A.1)
Mental	F00–F99, Z03.2, Z04.6, Z13.3	Adapted from Kuhn et al. (2009, Table A.1)
Respiratory	J00–J99	Adapted from Kuhn et al. (2009, Table A.1)
Cerebrovascular	I60–I69	Adapted from Kuhn et al. (2009, Table A.1)
External	V01–Y98	Codes of ICD-10 Chapter XX (”External causes of morbidity and mortality”)

Panel B. Drug Purchases

Category	ATC codes	Notes
Psychosomatic drugs	A03, M01A, M03BX, N02B, N02C	Adapted from Kuhn et al. (2009, Table A.1)
Psychotropic drugs	N05, N06, N07	Adapted from Kuhn et al. (2009, Table A.1)
Antidepressants	N06A	Ahammer and Packham (2023, p.3)
Opioids	N01AH, N02A	Ahammer and Packham (2023, p.3)
Non-opioid painkillers	N02B	Ahammer and Packham (2023, p.3)

Notes. This table provides lists of the ICD-10 diagnosis codes (Panel A) and ATC codes (Panel B) used to create the different categories of healthcare use used in Figure 3. In both panels, the first column gives the name of the category, the second panel gives the list of code(s) used to map visits/purchases to the category, and the third category provides additional information.