UNEMPLOYMENT INSURANCE GENEROSITY AND HEALTHCARE USE: EVIDENCE FROM SWEDEN*

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Abstract

Unemployment can worsen health and increase healthcare use, creating fiscal externalities in publicly financed systems. Using Swedish register data on inpatient and outpatient care visits and drug purchases, measured as total costs rather than out-of-pocket payments, this paper estimates the effect of unemployment insurance (UI) generosity on healthcare use. Exploiting benefit caps in a regression kink design, I find no evidence that more generous UI affects healthcare use. Estimates rule out cost effects greater than 0.08 SEK per 1 SEK increase in benefits during the first 40 weeks after spell start. In Sweden's generous welfare state, modest benefit increases do not to meaningfully alter the public healthcare costs of unemployment.

Keywords: administrative data, healthcare, regression kink design, social insurance

JEL codes: H51, I18, I38, J65

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1 Introduction

Job loss and unemployment are stressful events detrimental to mental and physical health (e.g., Wanberg 2012; Brand 2015; Picchio and Ubaldi 2023). These adverse health consequences are costly not only for the affected individuals, but also for the public budget through increased healthcare spending. The main rationale for social insurance programs, such as unemployment insurance (UI), is to financially support individuals facing adverse shocks such as unemployment (Chetty and Finkelstein 2013).

Although financial resources are strongly associated with health (e.g., Cutler et al. 2012; Lleras-Muney et al. 2025), evidence on the causal effect of UI generosity on health and healthcare use is scarce. Estimating how UI affects healthcare use is informative about whether the health consequences of unemployment mainly reflect income losses or instead reflect factors that operate independently of income, such as social stigma or the loss of social contacts and identity (see e.g., Jahoda 1982). Since healthcare is largely publicly financed, any UI-induced changes in healthcare use affect the public budget and should be considered when determining the optimal level of UI (Chetty 2006).[1]

In this paper, I study how UI generosity affects healthcare use using Swedish administrative data. I begin with a stylized framework that shows how UI-induced changes in healthcare use generate fiscal externalities in publicly financed systems and enter the welfare analysis of UI. I then estimate the causal effect of unemployment benefits on healthcare costs by exploiting variation in benefit levels due to statutory benefit caps in a regression kink design. Because observed benefit payments closely follow the policy rule, the design isolates exogenous variation in UI generosity and alleviates concerns such as reverse causality from health to pre-unemployment earnings. I focus on the first 40 weeks after spell start and measure healthcare costs using register data on inpatient and outpatient care visits and drug purchases, capturing total costs (including the publicly financed share) rather than out-of-pocket payments alone.

I first develop a simple conceptual framework linking UI generosity to healthcare use and costs. The effect of more generous UI on healthcare use is theoretically ambiguous: higher benefits may reduce psychological stress and improve health, potentially lowering

1. 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 2019a, Figure 2).

the need for care, while at the same time relaxing budget constraints and increasing demand for care when out-of-pocket costs are salient. I then augment a simple static model of UI and job search (Chetty and Finkelstein 2013) with health production in which subsidized healthcare is an input, and show that any UI-induced changes in healthcare costs translate into fiscal externalities that affect the optimal level of UI.

My empirical analysis uses individual-level register data on unemployment spells, benefit payments, and healthcare use. The estimation sample includes around 340,000 spells starting between March 2005 and July 2014 during which individuals received earnings-related benefits. For each spell, I match information on weekly benefit payments and detailed information on inpatient and outpatient care visits and drug purchases over the first 40 weeks since spell start.[2] A key limitation is that primary and dental care visits (typically less costly than specialized care) are not recorded in the registers.

My primary outcome measure is total healthcare spending, covering inpatient and outpatient visits and drug purchases. For drug purchases, I observe out-of-pocket payments and total spending, including the amount covered by prescription drug insurance. For inpatient and outpatient visits (including inpatient and specialized outpatient care provided by public and private providers), I measure visit-level resource costs by combining length of stay and Major Diagnostic Category (MDC) with external data on national average per-day costs by MDC. This measure is intended to capture the full resource costs of each visit, including procedures, medications, and materials, as well as underlying costs such as those related to personnel and administration.

To obtain exogenous variation in UI generosity, I use a regression kink (RK) design that exploits caps in the daily benefit amount. This non-linear policy rule creates a kink in daily benefits as a function of 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 (about 53–65 percent of the median monthly wage), so that roughly one in four in my estimation sample has a daily wage below the kink point. Related work uses this research design to identify economically meaningful effects on unemployment durations (Kolsrud et al. 2018) and consumption (Landais and Spinnewijn 2021), suggesting that the benefit cap creates salient financial incentives.

If individuals on either side of the kink point are similar in terms of other determinants

2. I focus on the first 40 weeks because the kink point shifts afterwards as the replacement rate falls while the benefit cap remains unchanged (see Section 4).

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. I provide evidence in favor of this assumption by showing that pre-determined covariates, healthcare use predicted by pre-determined covariates alone (fitted values), pre-unemployment healthcare use, and the density of the daily wage evolve smoothly around the kink point.

My main finding is that UI generosity has little effect on healthcare use. The estimates are precise: over the first 40 weeks after spell start, the 95 percent confidence interval for my preferred specification rules out changes (increases or decreases) in total healthcare costs greater than 0.08 SEK per 1 SEK increase in benefits.[3] Under conservative assumptions about the publicly-financed share of costs, my estimates rule out healthcare-related fiscal externalities greater than 0.13 SEK per 1 SEK increase in benefits, much smaller than the traditional fiscal externalities due to spell duration effects, which vary from 0.84 to 0.96 SEK in my setting.

The register data allow me to examine effects along several margins. Estimates remain precise enough to rule out economically meaningful effects: the 95 percent confidence intervals rule out changes in inpatient and outpatient visit costs greater than 0.18 SEK and changes in drug-purchase costs greater than 0.02 SEK per 1 SEK increase in benefits.[4] I also find no effects when measuring healthcare use by the number of visits or at the extensive margin (having any visits or drug purchases).

The data also allow me to study heterogeneity across different benefit recipients and types of healthcare use. This is relevant given evidence that unemployment harms mental health in particular and affects men more strongly (Picchio and Ubaldi 2023), that behavioral responses to UI can vary across individuals (e.g., Ahammer and Packham 2023), and that the effects of job loss and UI can spill over to partners (e.g., Gathmann et al. 2025; Hendren 2017). However, I do not find clear effect heterogeneity by gender, age, partnership status (including own vs. partner's healthcare use among couples), type of in-/outpatient visit or drug purchase, or when estimating effects week-by-week after spell start.

Overall, I find that in Sweden's generous social insurance and public healthcare system, marginal increases in unemployment benefits do not affect beneficiaries' healthcare

3. Roughly, 1 SEK $\approx$ 0.10 EUR and 0.10 USD (average exchange rates in 2020, Riksbanken 2025).

4. These bounds do not sum to the bound for total costs because I choose bandwidths separately by outcome, as recommended by Cattaneo and Vazquez-Bare (2017).

costs and therefore do not reduce the public healthcare costs associated with unemployment. This suggests a low income elasticity of healthcare spending on this policy margin. As a result, healthcare-related fiscal externalities are unlikely to be a first-order consideration when setting the level of UI in this context.

My findings contrast with existing evidence of sizable utilization responses to insurance coverage expansions and cost-sharing reforms (e.g., Card et al. 2009; Brot-Goldberg et al. 2017), highlighting that institutional context and the policy margin matter for whether income-replacement programs affect healthcare spending. More broadly, the findings suggest that the responsiveness of healthcare spending to earnings-related social insurance likely depends on institutional features, particularly healthcare financing. While this channel appears muted in the Swedish UI system, it may be economically relevant in settings with less generous public coverage and higher out-of-pocket costs, or where access and prices are mediated by private insurance and plan design.

Related literature. This paper contributes to a growing literature on how social insurance and cash transfer programs affect the health and healthcare use of recipients and their family members (see, e.g. Levy and Meltzer 2008; Ziebarth 2018, for reviews). Previous studies examine the health and healthcare spending effects of sickness and disability insurance (e.g., Gelber et al. 2023; Wikström 2024), health insurance (e.g., Card et al. 2009; Brot-Goldberg et al. 2017), pensions (e.g., Cheng et al. 2018; Miglino et al. 2023), and social assistance (e.g., Snyder and Evans 2006; Aizer et al. 2016; Hoynes et al. 2016).

Evidence on how UI affects health and healthcare use is more limited.[5] Closest to my paper, Kuka (2020) uses between-state policy variation in the U.S. to show that more generous UI increases health insurance coverage and healthcare spending, while Ahammer and Packham (2023) show that a nine-week extension of potential benefit duration in Austria reduces opioid and antidepressant use among women but not men. Relative to these studies, I use a more comprehensive measure of healthcare use that captures the full costs of inpatient and outpatient visits and drug purchases, including costs covered by the healthcare system.

My finding of limited healthcare responses differs from the utilization and prescription

5. A related set of studies examines how active labor market programs affect the health of participants (see e.g., Caliendo et al. 2023; Baekgaard et al. 2024).

responses in Kuka (2020) and Ahammer and Packham (2023). A natural interpretation is that these differences reflect both the policy margin studied (benefit levels vs. potential duration) and institutional context, since healthcare responses tend to be larger when policy variation affects insurance coverage, cost sharing, or entails large discrete benefit changes.

More broadly, I contribute to the large literature on how job loss affects health and public health costs (e.g., Sullivan and Von Wachter 2009; Kuhn et al. 2009). Even in Nordic countries with universal healthcare systems, job loss adversely affects health (e.g., Eliason and Storrie 2009; Black et al. 2015; Browning and Heinesen 2012; Gathmann et al. 2025). An important yet less studied question is whether UI mitigates these health consequences.[6] My findings suggest that, in the Swedish context where public healthcare is highly subsidized, marginal increases in UI benefits have little effect on recipients' healthcare use.

Outline. The paper proceeds as follows. Section 2 discusses mechanisms through which UI could affect healthcare use and why such effects affect the optimal level of UI. Section 3 describes the Swedish UI and healthcare systems. Section 4 describes the data and analysis sample. Section 5 discusses the research design. Section 7 discusses the findings, and Section 8 concludes. The Online Appendix contains proofs, derivations, and additional discussion and results.

2 Conceptual Framework

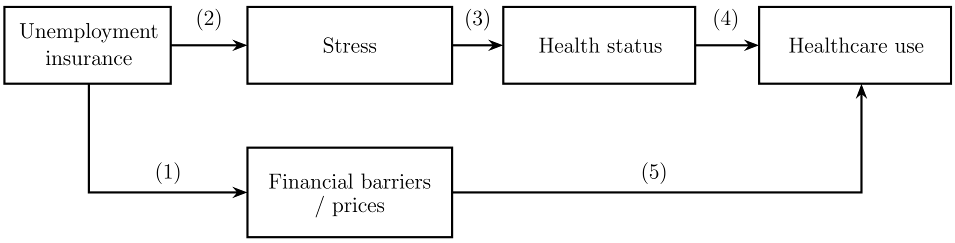
In this section, I first discuss mechanisms through which UI generosity could affect healthcare use, even under universal public coverage, and why the sign is ambiguous *ex ante*. I then use a stylized model to show that any UI-induced changes in healthcare use create healthcare-related fiscal externalities that enter the welfare analysis of UI.

2.1 Mechanisms Linking Unemployment Insurance to Healthcare Use

More generous unemployment benefits could affect healthcare use through (at least) two channels, summarized in Figure 1. The sign and magnitude are ambiguous *ex ante*,

6. An exception is Amorim et al. (2024), who use a tenure-based regression discontinuity design and find that UI access partly offsets the mortality and hospitalization effects of job loss in Brazil.

Figure 1: Mechanisms Via Which Unemployment Insurance Could Affect Healthcare Use



Notes. This figure illustrates two potential channels through which more generous unemployment insurance (UI) could affect healthcare use. The first operates through financial barriers (arrow (1)): more generous UI increases disposable resources and may reduce sensitivity to co-payments and other out-of-pocket costs, which can affect healthcare use (arrow (5)). The second operates through psychological stress (arrow (2)) and health (arrow (3)): by reducing stress associated with unemployment, more generous UI may improve physical and mental health, which in turn can affect healthcare use (arrow (4)). See Section 2.1 for a detailed discussion.

especially under universal and publicly financed healthcare.

Direct effect via financial barriers. The first channel, which I label the *direct effect* (arrows (1) and (5) in Figure 1), operates through financial barriers. Even under universal coverage, patients face cost sharing that may bind for those with limited resources. By raising disposable income, more generous benefits may reduce sensitivity to co-payments and other out-of-pocket costs (arrow (1)), thereby increasing healthcare demand (arrow (5)) to the extent that healthcare is a normal good (Folland et al. 2016, Chapter 9).

Consistent with this mechanism, evidence from Sweden and other Nordic countries shows that even modest cost sharing can meaningfully affect healthcare demand (Haaga et al. 2024a, 2024b; Johansson et al. 2019; Johansson et al. 2023; Landsem and Magnussen 2018; Nilsson and Paul 2018; Olsen and Melberg 2018). Related evidence exploiting variation in the timing of benefit payments and co-payment size shows that prescription drug purchases are sensitive to short-run liquidity constraints among low-income individuals (Lyngse 2020; Gross et al. 2022; Wikström 2023). That these effects arise even with modest co-payments suggests that financial barriers can matter for healthcare use.

Indirect effect via psychological stress. The second channel, which I label the *indirect effect* (arrows (2)–(4) in Figure 1), operates through psychological stress and health. By partially insuring against income loss and financial uncertainty, more generous UI may alleviate psychological stress associated with unemployment (arrow (2)), improving physical and mental health (arrow (3)) and, in turn, affecting healthcare use (arrow (4)). In contrast to the direct effect, the indirect effect would likely *decrease* healthcare demand.

Consistent with this mechanism, job loss is widely associated with increased psychological distress and poorer mental health (Picchio and Ubaldi 2023; Wanberg 2012). Medical and epidemiological research further shows that psychological stress, particularly when prolonged, is an important risk factor for a range of physical and mental health conditions, including depression, anxiety, cardiovascular disease, and metabolic disorders.[7]

Evidence from other social insurance and cash transfer programs also suggests that income support can affect mental health and healthcare use. For example, the Finnish basic income experiment reduced psychotropic drug use and mental-health-related outpatient visits (Hämäläinen et al. 2025). Related work on UI finds that extensions of potential benefit duration reduce antidepressant use among recipients (Ahammer and Packham 2023). Together, these findings support the plausibility of a stress-mediated channel linking income support to healthcare use.

Discussion. Taken together, an increase in UI generosity could plausibly affect healthcare use both directly, by reducing sensitivity to financial barriers, and indirectly, by reducing stress associated with unemployment, even in settings with a high degree of public healthcare financing. Because the two channels predict effects in opposite directions, the sign and magnitude of the net effect are ultimately an empirical question.

At first glance, the two channels may appear at odds with evidence from Swedish lottery studies showing limited effects of large wealth shocks on health and healthcare use (Cesarini et al. 2016; Lindqvist et al. 2020). However, these studies primarily identify average long-run effects of substantial, permanent wealth gains among samples of lottery players, a population that is arguably more representative of the general Swedish population, and thus less selected, than UI recipients.

Section 2.2 formalizes these channels with a stylized model and shows that, in a

7. Medical and epidemiological research suggests that psychological stress affects health through sustained activation of the hypothalamic–pituitary–adrenal (HPA) axis and the sympathetic nervous system, leading to dysregulation of cortisol and other stress hormones (Chrousos and Gold 1992; Schneiderman et al. 2005). Prolonged exposure to these physiological responses is associated with systemic inflammation, immune dysregulation, endothelial dysfunction, and adverse metabolic changes such as impaired glucose regulation and lipid metabolism (Brotman et al. 2007; Chourpiliadis et al. 2024). These biological mechanisms have been linked to increased risk of cardiovascular disease and stroke (Brotman et al. 2007; Kivimäki and Steptoe 2018; Reddin et al. 2022), as well as to depression and anxiety disorders (Schneiderman et al. 2005). Stress may also influence health by affecting behavior via e.g., sleep disruption, reduced physical activity, smoking, and alcohol use (Schneiderman et al. 2005).

publicly financed healthcare system, any UI-induced changes in healthcare use generate fiscal externalities that affect the optimal level of UI.

2.2 Stylized Model

Intuition. I extend the static job-search and UI model in Chetty and Finkelstein (2013, Section 3.1) by incorporating health production in the spirit of Grossman (1972) and healthcare consumption with a wedge between private (out-of-pocket) and social costs due to subsidization. This wedge generates an additional source of fiscal externalities. Although I cast the model in the context of UI, its lessons apply more broadly to social insurance programs. Details, derivations, and proofs are in Appendix A.

Setup. There are two states $s \in \{E, U\}$ (employment and unemployment). The individual ("she") chooses search effort $e \in [0,1]$ at disutility cost $\psi(e)$, which determines the probability of being employed. When employed she earns wage w and pays a tax $\tau(b)$ that finances unemployment benefits; when unemployed she receives benefits b.

In each state s, the individual chooses consumption c_s and healthcare use m_s. Given exogenous baseline health h_0, health is produced according to $h_s = H_s(m_s; h_0)$, and total healthcare costs are $\kappa(m_s, h_s)$.[8] Healthcare is subsidized: the government pays a share $\theta \in [0,1]$ of total costs while the individual pays $1-\theta$.[9]As in Chetty and Finkelstein (2013), the individual takes $(\tau(b), b)$ as given when making choices.

Government's problem and optimal UI. The government chooses the benefit level b to maximize the individual's expected utility subject to a balanced budget constraint,

$$e\,\tau(b) = (1-e)\,b + \theta\Big[e\,\kappa(m_E, h_E) + (1-e)\,\kappa(m_U, h_U)\Big],$$

accounting for the individual's endogenous choices.

8. The dependence of $\kappa(\cdot)$ on m captures the direct resource cost of providing care, while the dependence on health h reflects that treatment costs vary with underlying health: at a given level of utilization, individuals in worse health may require more intensive or more expensive treatment. This formulation distinguishes between healthcare use and healthcare costs, similarly to Finkelstein et al. (2019).

9. In practice, out-of-pocket costs are typically non-linear in total healthcare spending due to, e.g., deductibles and caps (see also Section 3). The constant θ can be interpreted as the marginal public financing share evaluated at the relevant spending level. Allowing for non-linearities would complicate analysis without changing the basic insight that UI-induced changes in healthcare use generate a fiscal externality.

The following proposition shows that UI-induced changes in healthcare costs enter the optimal UI formula through an additional fiscal externality term.

Proposition 1. *Consider the static model of job search and unemployment insurance described above (see Appendix A for details). Let* $\varepsilon_{1-e,b} \equiv \frac{d(1-e)}{db} \frac{b}{1-e}$ *denote the elasticity of unemployment with respect to benefits* b*. Define the healthcare fiscal externality as*

$$FE^{health}(b) \equiv \theta \left[\frac{d\kappa(m_U, h_U)}{db} + \frac{e}{1-e} \frac{d\kappa(m_E, h_E)}{db} \right]. \tag{1}$$

At an interior optimum, the optimal benefit level b^* *satisfies*

$$\frac{u_c(c_U, h_U)}{v_c(c_E, h_E)} = 1 + \left(1 + \theta \frac{\kappa(m_U, h_U)}{b} \right) \varepsilon_{1-e,b} + FE^{health}(b), \tag{2}$$

where the effect of benefits on total healthcare costs can be decomposed as

$$\frac{d\kappa(m_s, h_s)}{db} = \underbrace{\kappa_m(m_s, h_s) \frac{dm_s}{db}}_{\text{(1) direct effect}} + \underbrace{\kappa_h(m_s, h_s) \frac{dh_s}{db}}_{\text{(2) indirect effect}}, \qquad s \in \{E, U\}. \tag{3}$$

Proof. See Appendix A. □

Economic intuition. Equation (2) is a modified version of the Baily-Chetty formula (Chetty and Finkelstein 2013) augmented with an additional term, $FE^{\text{health}}(b)$, capturing fiscal externalities operating through healthcare spending. The left-hand side, $u_c(c_U, h_U)/v_c(c_E, h_E)$, captures the standard insurance value of UI. The first term on the right-hand side captures the mechanical fiscal cost of benefits and the standard duration externality through unemployment spell duration responses, $\varepsilon_{1-e,b}$. The final term, $FE^{\text{health}}(b)$, captures the additional per-unemployed fiscal cost (or saving) arising from UI-induced changes in total healthcare costs.

This healthcare fiscal externality arises only because healthcare is publicly financed. If individuals bore the full resource cost of healthcare ($\theta = 0$), then $FE^{\text{health}}(b) = 0$ and (2) reduces to the standard Baily-Chetty formula. Equation (3) links the fiscal externality to the mechanisms in Section 2.1: benefits may affect healthcare costs by changing healthcare use (direct effect) and/or by changing underlying health and thereby the cost of treatment at a given level of use (indirect effect).

Scope and extensions. The model is intentionally stylized to isolate fiscal externalities arising from healthcare spending. In particular, it abstracts from the possibility that UI-induced changes in health also affect the insurance value of benefits, e.g., through longevity (Gelber et al. 2023) or state-dependent marginal utility of consumption (e.g., Finkelstein et al. 2013; Kanninen et al. 2025), as well as from endogenous take-up, interactions with other taxes and transfers, and general equilibrium effects. Incorporating these features would complicate the analysis but would not alter the central insight: whenever healthcare is publicly financed, benefit-induced changes in healthcare use create fiscal externalities that enter the optimal UI formula.

3 Institutional Background

3.1 Unemployment Insurance

Eligibility criteria. To qualify for unemployment insurance (UI), individuals must be registered at the Public Employment Service, satisfy a work history requirement, actively seek work, and be prepared to accept suitable job offers or participate in labor market programs. Individuals can receive benefits for up to 300 days (5 payment days per week for 60 weeks; 450 days for parents of children under 18), after which continued income support typically requires participation in active labor market programs (though participation may also occur earlier during the spell).

Program structure and benefit schedule. During my study period, statutory UI was administered by 27 UI funds ("a-kassa"), typically affiliated with trade unions. Although membership is voluntary, enrollment was high, ranging from 70–83 percent of the labor force aged 16–64 (IAF 2024b).[10]

Statutory UI consists of basic and earnings-related benefits. Basic benefits pay a flat amount (320 SEK per day during my study period, equivalent to 25–31 percent of the median wage; cf. Statistics Sweden 2024a) and accounted for 18 percent of unemployment spells. I focus on earnings-related benefits, which covered the remaining 82 percent of

10. Enrollment fell by more than 10 percentage points following reforms in 2007–2008 that increased premiums and introduced an "unemployment fee" that partly linked premiums to the unemployment rate of fund members. Since 2007, enrollment has remained around 70 percent, even after the fee was repealed in 2014 (Kolsrud 2018; Landais et al. 2021).

spells and are available to individuals aged 20–64 who have contributed continuously to a UI fund in the 12 months before unemployment. Earnings-related benefits replace 80 percent of the pre-unemployment daily wage up to a cap of 680 SEK per day, implying a kink in the benefit schedule at a daily wage of 850 SEK. The benefit cap was binding at relatively low earnings levels, corresponding to roughly 53–65 percent of the median monthly wage (Statistics Sweden 2024a). This kink forms the basis of my empirical strategy (Section 5).[11]

Interaction with other social insurance programs. Unemployment insurance can interact with other social insurance programs, potentially inducing substitution (e.g., Inderbitzin et al. 2016; Leung and O'Leary 2020). In Sweden, substitution to sickness benefits is a potential concern (Larsson 2006; Hall and Hartman 2010), but it is unlikely to be quantitatively important in my setting. Transitions to sickness insurance are most pronounced near benefit exhaustion (Larsson 2006; Hall and Hartman 2010), whereas my analysis focuses on the first 40 weeks after spell start (Section 4). Moreover, during my study period sickness benefits for the unemployed had the same replacement rate (80%) but a substantially lower cap (486 SEK per day) than UI (Försäkringskassan 2025, p. 65), limiting incentives to substitute early in the spell.

3.2 Healthcare System

Financing. Sweden has a tax-financed healthcare system with universal coverage that heavily subsidizes healthcare visits and prescription drugs (e.g., Anell et al. 2012; Björvang et al. 2023). Total health spending was 11% of GDP in 2017, and 84% of spending was publicly funded (OECD 2019b). Out-of-pocket payments accounted for 1% of inpatient spending, 14% of outpatient spending, and 28% of prescription drug spending in 2016 (OECD 2019a, Figure 2).

Healthcare provision. Healthcare is delivered by a mix of public and private providers. Inpatient and specialized outpatient care remain largely publicly provided (OECD 2019b), while private provision is more common in primary care following the 2010 primary

11. Some unions offer non-statutory top-ups; see, e.g., Kjellberg (2019, Table 3) and Lindquist and Wadensjö (2011, p. 17). Since I do not observe union membership or top-up receipt in my data, my analysis focuses on statutory UI.

care choice reform (Burström et al. 2017; Pontén et al. 2017). Both public and private providers operate within the publicly financed system, face the same regulations on subsidies and patient fees, and are reimbursed by the 21 healthcare regions (OECD 2019b).[12]

Access to care. Despite substantial subsidization, access to care varies across regions, including waiting times (OECD 2019b; European Commission 2019). Regional differences in access likely reflect variation in provider availability, patient fees, reimbursement schemes, and resource constraints (OECD 2019b).[13]

Out-of-pocket costs. Patient fees are set by healthcare regions and are relatively low: in 2017, fees were at most 100 SEK for inpatient visits and 350 SEK for specialized outpatient visits, and typically 100–250 SEK for primary care visits (Pontén et al. 2017). All residents are covered by a uniform prescription drug insurance scheme in which the out-of-pocket share declines from 100 to 0 percent as annual drug spending increases (see e.g., Wikström 2023).[14]

Both patient fees and prescription drug expenditures are subject to annual out-of-pocket ceilings that reset 12 months after the first visit or purchase. In 2017, the ceiling for prescription drugs was 2200 SEK and the ceiling for patient fees ranged from 900–1100 SEK across regions (Pontén et al. 2017).[15]

12. Within legal constraints set at the national level, regions have discretion over provider reimbursement schemes, patient fees, organization of primary and specialized outpatient care, and the degree of patient choice. Reimbursement schemes differ across regions and provider types. In primary care, regions use a combination of per-patient, per-visit, and performance-based payment schemes; for hospital care, regions typically use a combination of per-patient payments and price or volume ceilings (Anell et al. 2012, Section 3.6.1).

13. The increasing popularity of private health insurance may also contribute to disparities in access, although uptake is still relatively low (around 15% in the population aged 18–64 in 2016–17) and mostly employment-based (Anell et al. 2012; Kullberg et al. 2019).

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

15. Exact national figures for the share of individuals reaching the out-of-pocket ceiling are not readily available. But government simulations using administrative data for 2012 suggest that most individuals with positive drug expenditures were below the annual out-of-pocket ceiling for prescription drugs (Socialdepartementet 2011).

4 Data

This section describes the administrative data, analysis sample, and main variables. Appendices B and C provide additional details on data sources and variable construction.

4.1 Administrative Data

Unemployment spells. I use administrative data on registered unemployment spells from the Swedish Public Employment Service (PES) (AF 2024a, 2024b, 2024c). For each spell, I observe the registration and deregistration dates and the reason for deregistration (e.g., finds employment, exits from labor force or to another social insurance program). I define spell start as the registration date and spell end as the deregistration date.

Unemployment benefit payments. To each spell, I match data on weekly unemployment benefit payments from the Swedish Unemployment Insurance Inspectorate (IAF 2024a). For each payment week, I observe the number of payment days, the daily benefit amount, the pre-unemployment daily wage used to calculate benefits, and whether payments are under the basic or earnings-related scheme. For each spell, I calculate the average daily wage, average daily benefit, and average replacement rate over the first 200 payment days (40 payment weeks).[16]

Socioeconomic background. For each spell, I use data from Statistics Sweden (2022, 2023a, 2023b) to match information on demographic and socioeconomic characteristics (age, gender, education, marital/cohabitation status, any children under 18 at home, county of residence, and industry of the highest-paying employer when available). Covariates are measured at the end of the calendar year preceding spell start and are used for testing the validity of the research design (Section 5) and heterogeneity analyses.

Healthcare use. I measure healthcare use using register data on inpatient care, specialized outpatient care, and drug purchases from the National Board of Health and Welfare (Socialstyrelsen 2022b, 2022c, 2022d). The patient registers cover care provided by public

16. In some cases, the daily wage is recalculated during the spell without affecting past payments (e.g., due to updated normal working hours or new collective agreements) (IAF 2016). Such adjustments are rare and small in practice.

and private providers.[17] Primary care and dental care are not included. For each visit, I observe admission and discharge dates (discharge for inpatient only), the main diagnosis, and the Major Diagnostic Category (MDC). For each drug purchase, I observe the purchase date, active ingredient, and detailed costs.

4.2 Sample Definition

My analysis sample covers the universe of registered unemployment spells starting between March 5, 2007 and July 14, 2014.[18] I restrict to spells for individuals aged 20–64 (measured in the calendar year before spell start), exclude spells with only basic benefits, and keep spells with pre-unemployment daily wages between 150 and 1,800 SEK. I exclude spells for which I cannot match information on pre-unemployment socioeconomic characteristics, except for employer industry which I allow to be missing since this information is missing in a non-negligible number of cases.

I measure outcomes over the first 40 calendar weeks after spell start.[19] Over this window, earnings-related benefits replace 80% of the pre-unemployment daily wage up to a cap of 680 SEK per day, implying a kink in the benefit schedule at a daily wage of 850 SEK (the 27$^{\text{th}}$ percentile of the daily wage distribution; see Figure 2, Panel B).[20]

I observe daily wages and daily benefit amounts directly in the register data. The daily wage is computed by the PES from pre-unemployment earnings histories, and the daily benefit amount follows the statutory schedule given the daily wage and accumulated payment days. I measure both variables in nominal terms because benefits are not indexed.

4.3 Outcome Variables

I construct the main outcomes from the administrative registers. I deflate all cost variables using the consumer price index (Statistics Sweden 2024b), with 2020 as the reference year.

17. In Sweden, most private specialized care is delivered within the publicly financed system and follows the same patient-fee rules as public care. Providers are required to report to the register. When coverage issues arise, they primarily reflect non-response among some private providers (Ludvigsson et al. 2011; Grönvik 2015; Socialstyrelsen 2023b).

18. I focus on this period because it is the longest span during which the earnings-related UI rules remained unchanged and the main outcomes can be measured.

19. This ensures that the replacement rate and benefit cap are constant over the outcome window.

20. After 200 payment days (40 payment weeks), the replacement rate falls to 70% while the cap remains unchanged, so the kink point shifts.

Inpatient and outpatient care use. I measure inpatient and specialized outpatient care use over the first 40 calendar weeks after spell start as (i) the number of visits and (ii) the total costs of visits. Outcomes are measured over a fixed 40-week window regardless of whether the individual exits registered unemployment before week 40.

I assign costs to each visit using its Major Diagnostic Category (MDC) and external data on the national average per-day costs by MDC, separately for inpatient and outpatient care, from the Swedish Association of Local Authorities and Regions (SKR 2023) and the National Board of Health and Welfare (Socialstyrelsen 2023a).[21] I use 2020 as the reference year so that variation over time reflects changes in utilization and case mix rather than changes in unit costs. Appendix C provides details and Appendix Table 2 reports the resulting costs by MDC.

This cost measure is intended to capture the full resource costs of each visit, not just patient payments. National guidelines stress that regions should attribute costs as closely as possible to a unique patient and visit. Relevant costs include both direct treatment inputs (e.g., procedures, diagnostics, medications, and materials) and underlying costs such as personnel, administration, and facilities (SKR 2020).[22]

Drug purchases. I measure drug purchase costs over the first 40 calendar weeks after spell start, distinguishing total costs, out-of-pocket payments, and costs covered by prescription drug insurance.

Total costs of healthcare use. I measure total healthcare costs as the sum of inpatient and outpatient visit costs and drug-purchase costs over the first 40 calendar weeks after spell start. Because healthcare expenditures are highly right-skewed (e.g., Karlsson et al. 2024), I winsorize each cost measure at the 99th percentile and assess sensitivity to alternative winsorization thresholds in Section 6.5.

21. Major Diagnostic Categories are groupings of Diagnosis Related Groups (DRG), which in turn group healthcare visits 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 used to monitor the cost-effectiveness and resource allocation of healthcare systems in many countries, including Sweden and the United States. See Socialstyrelsen (2022a).

22. A drawback 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 into 28 categories. Unfortunately, I do not observe DRG codes for visits in the patient registers.

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.

4.4 Summary Statistics

Table 1 reports summary statistics for the analysis sample and 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 sample are younger (mean age

39 vs. 42), less likely to be partnered, and less likely to have higher education (28 vs. 37 percent), while prior-year earnings are similar. Pre-spell mean healthcare and drug spending is slightly lower in the sample (2054 SEK) than in the population (2598 SEK).

5 Research Design

This section describes the regression kink (RK) design I use to identify the causal effect of unemployment benefit generosity on healthcare use and costs.

5.1 Identification

Under the earnings-related UI scheme, the daily benefit amount is a kinked function of the pre-unemployment daily wage: benefits replace a constant fraction ρ of earnings up to a cap $\bar{B}$. Let W denote the (observed) pre-unemployment daily wage (the running variable) and B the (observed) daily benefit amount (the treatment). The benefit schedule implies a kink at $\bar{w} = \bar{B}/\rho$ (850 SEK in my setting), generating a discontinuity in the slope of $\mathrm{E}[B \mid W = w]$ at $w = \bar{w}$.

I use a *fuzzy* RK design because realized benefit payments do not perfectly follow the statutory schedule (e.g., due to occasional sanctions).[23] My parameter of interest is the fuzzy RK estimand, given by the ratio of the reduced-form kink in the outcome to the first-stage kink in benefits:

$$\tau = \frac{\lim_{w \downarrow \bar{w}} \frac{d}{dw} \mathrm{E}[Y \mid W = w] - \lim_{w \uparrow \bar{w}} \frac{d}{dw} \mathrm{E}[Y \mid W = w]}{\lim_{w \downarrow \bar{w}} \frac{d}{dw} \mathrm{E}[B \mid W = w] - \lim_{w \uparrow \bar{w}} \frac{d}{dw} \mathrm{E}[B \mid W = w]}. \tag{4}$$

Card et al. (2015) discuss conditions sufficient for (4) to identify a weighted average of marginal effects of B on Y at $\bar{w}$, with larger weights on groups more likely to comply with the benefit formula. Under standard regularity conditions, the key identifying assumption is that the density of the running variable is continuously differentiable around $\bar{w}$ (so there is no sorting at the kink). I assess this assumption by testing for smoothness of the daily wage distribution and pre-determined covariates around $\bar{w}$ and by testing for kinks in outcomes measured before spell start. As discussed in Section 6.5, these tests do not indicate violations of the assumption.

23. Sanctions and other deviations are rare in the analysis sample; see Sections 6.1 and 6.5 for evidence on the first stage and robustness to alternative specifications.

5.2 Estimation and Inference

Following the standard in the literature, I estimate (4) using local polynomial estimation. The fuzzy RK estimator of τ in (4) is

$$\hat{\tau} = \frac{\hat{\beta}_1^+ - \hat{\beta}_1^-}{\hat{\kappa}_1^+ - \hat{\kappa}_1^-}, \tag{5}$$

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 observations to the left and $s = +$ to the right of the kink, p is polynomial order, K is the kernel function, and h is the bandwidth.

My baseline specification uses a local linear estimator[24] ($p = 1$), a uniform kernel ($K(c) = \frac{1}{2}1\{|c| < 1\}$), and mean-squared-error (MSE) optimal bandwidth selection, with bias-corrected point estimates and robust confidence intervals following Calonico et al. (2014b).[25] I cluster standard errors at the individual level to account for multiple spells per person. I choose the optimal bandwidth separately for each outcome, following Cattaneo and Vazquez-Bare (2017, p. 143). The results are robust to alternative bandwidths, polynomial orders, and kernels (Section 6.5). I also compare estimates with and without covariate adjustment following Calonico et al. (2019). Covariate adjustment is not required for consistency but can improve precision (Ando 2017).

5.3 Interpretation and Scaling of Estimates

For cost outcomes measured over the first 40 weeks after spell start, the fuzzy RK estimator $\hat{\tau}$ in (5) gives the estimated change in costs over this window per 1 SEK increase in the daily benefit amount at the kink. For ease of interpretation, I report effects per 1

24. For the main outcomes, the data-driven polynomial order selection procedure by Pei et al. (2022) selects a local linear estimator as the preferred specification.

25. 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.

SEK increase in total UI benefits over the first 40 weeks by scaling the raw daily-benefit effects by $1/(40 \times 5) = 1/200$ (since benefits are paid five days per week).[26] I also report elasticities (i.e., percent changes in costs per 1 percent increase in benefits) evaluated at the kink,

$$\hat{\varepsilon}_{Y,B} = \hat{\tau} \times \frac{\tilde{B}}{\tilde{Y}}, \tag{6}$$

where $\tilde{Y}$ and $\tilde{B}$ denote mean outcomes and benefits computed within a narrow window around the kink. For elasticities, I compute standard errors via a non-parametric bootstrap with 100 replicates where I sample unemployment spells with replacement.

6 Results

6.1 First Stage & Disemployment Effects

I begin by verifying a strong first stage. Figure 2 shows binned scatterplots of the average replacement rate (Panel A) and average daily benefit (Panel B) against the daily wage, with red lines indicating the statutory schedule. The figure shows that both average replacement rates and daily benefits closely follow the policy rule.

As a benchmark, I also estimate the effect of unemployment benefits on UI benefit spell duration. These ”disemployment effects” have been the focus of the literature on the behavioral effects of UI (see Cohen and Ganong 2025) and help illustrate that the benefit cap creates salient financial incentives. Appendix Figure 1 and Appendix Table 3 show that a 1% increase in benefits raises UI benefit spell duration by 0.83% (SE = 0.24), in line with prior evidence.[27]

6.2 Effects on Healthcare Use

Total costs of healthcare use. I now turn to the main results. Figure 3A shows a binned scatterplot of total healthcare costs against the daily wage around the kink where indi-

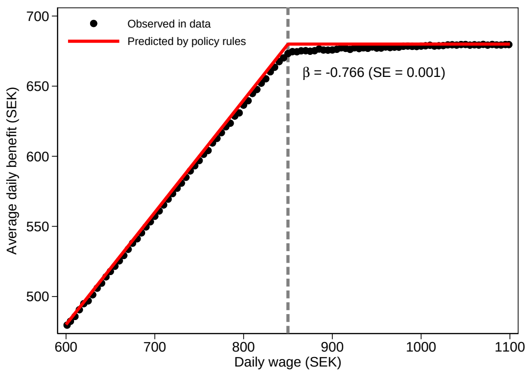
26. An alternative normalization would divide by the estimated effect of a 1 SEK increase in daily benefits on total benefit payments over the 40-week window, which would incorporate endogenous changes in spell duration. I instead use the mechanical conversion from a 1 SEK increase in the daily benefit amount to benefits paid over 40 weeks for ease of interpretation.

27. In their meta-analysis, Cohen and Ganong (2025) find that, after correcting for publication bias, 90 percent of estimated duration elasticities exploiting variation in the replacement rate fall between -0.22 and 0.65 (Table 2, Row 1, Column 7).

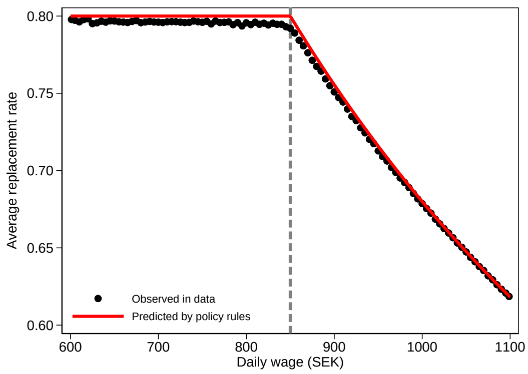
Figure 2: Illustrating the Regression Kink Design

Panel A: First Stage

(i) Average replacement rate

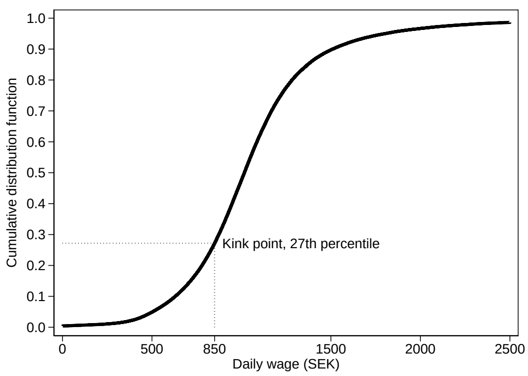


(ii) Average daily benefit amount

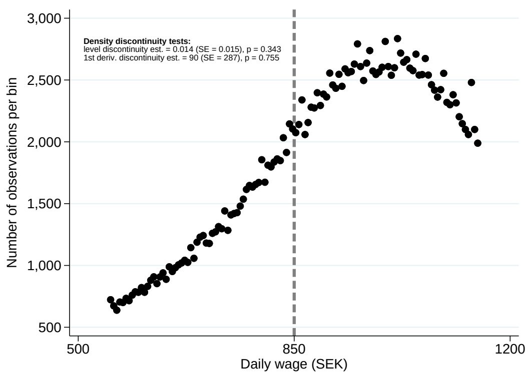


Panel B: Kink Point Location and Density of the Daily Wage

(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 4). 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.

viduals reach the daily benefit cap. The outcome is the sum of inpatient and outpatient visit and drug purchase costs, measured over the first 40 weeks since spell start.

Consistent with well-documented socioeconomic gradients in health and healthcare (e.g., Cutler et al. 2012; Lleras-Muney et al. 2025), total healthcare costs decline with the daily wage. However, Figure 3A shows no evidence of a discontinuous slope change at the kink, either for costs (left panel) or at the extensive margin (having any visits or drug purchases; right panel). The corresponding estimates in columns 1–2 of Table 2 and Appendix Table 5 confirm that more generous benefits have no statistically significant effect on total healthcare costs.

The estimates in Table 2 are precise enough to be informative. The 95% pointwise confidence intervals in columns 1–2 rule out changes in total healthcare costs larger than 0.08 SEK per 1 SEK increase in benefits. In terms of elasticities, the estimates rule out cost changes larger than 1.3% per 1% increase in benefits.

Inpatient and outpatient care use. The absence of an effect on total healthcare costs could in principle reflect offsetting responses across inpatient care, outpatient care, and drug purchases. However, Figure 3B and Appendix Figure 2 show no evidence of discontinuous slope changes in inpatient or outpatient care use at the benefit-cap kink. The corresponding estimates in columns 3–8 of Table 2 and Appendix Tables 4–5 corroborate the graphical evidence: increases in unemployment benefits have no statistically significant effects on inpatient or outpatient care use.

The estimates are again precise enough to be informative. The 95% pointwise confidence intervals in Table 2 rule out changes in costs of inpatient and outpatient care visits larger than 0.18 SEK (columns 3–4), inpatient costs larger than 0.10 SEK (columns 5–6), and outpatient costs larger than 0.03 SEK (columns 7–8) per 1 SEK increase in benefits.[28]

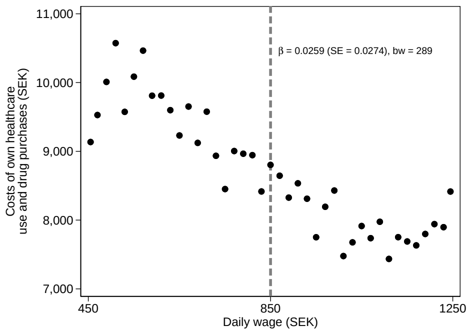
I also find no effects when measuring utilization by the number of visits or at the extensive margin (any visits). For example, columns 1–2 of Appendix Table 4 rule out changes larger than 0.23 visits per 100 SEK increase in daily benefits, relative to a mean of 1.0 visits over the first 40 weeks among individuals with daily wages close to the kink.

28. Because I select the MSE-optimal bandwidth separately for each outcome, the bounds for inpatient costs (columns 5–6) and outpatient costs (columns 7–8) need not sum to the bound for combined inpatient and outpatient costs (columns 3–4), even though the latter outcome is the sum of the former two.

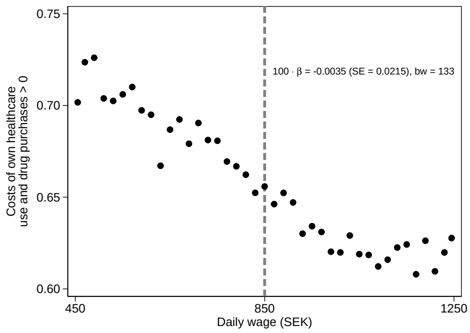
Figure 3: Healthcare Use Around the Kink Point

Panel A: Total Healthcare Use

(i) Total costs

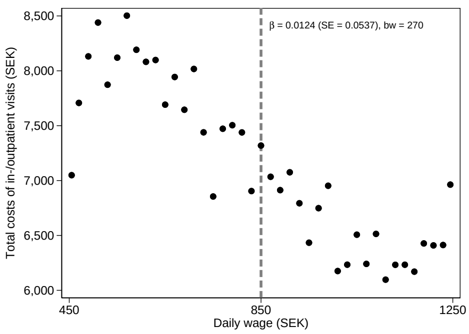


(ii) Any healthcare use

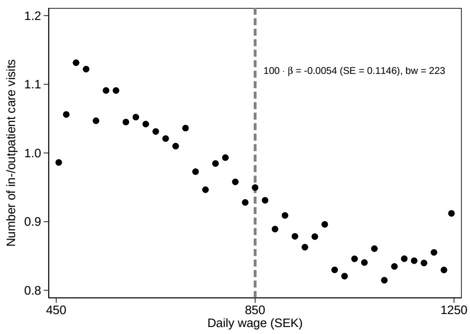


Panel B: Inpatient and Outpatient Care Visits

(i) Total costs

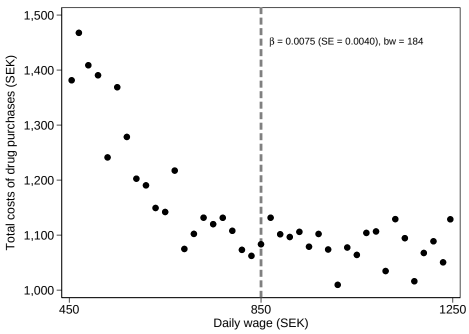


(ii) Number of visits

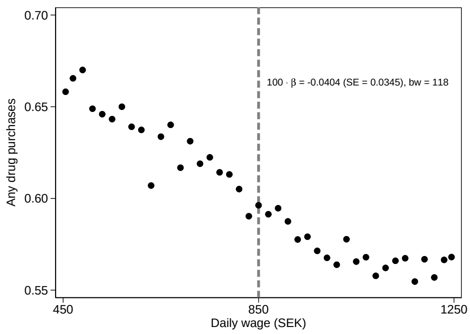


Panel C: Prescription Drug Purchases

(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 4). 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 A, 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 are clustered at the individual level.

Overall, these findings are consistent with Ahammer and Packham (2023, Table 4), who find no statistically significant effects of longer potential benefit duration on inpatient or outpatient care use among older unemployed individuals in Austria.

Drug purchases. Figure 3C shows binned scatterplots of drug purchases against the daily wage around the kink, measured both in total costs and at the extensive margin (any purchases). As noted in Section 4, drug-purchase costs include both out-of-pocket payments and costs covered by prescription drug insurance. Although the relationship between drug costs and the daily wage is non-linear, Figure 3C shows no clear discontinuous slope change at the kink where individuals reach the daily benefit cap.[29]

The corresponding estimates in columns 9–10 of Table 2 and Appendix Table 5 confirm the graphical evidence: higher benefits have no statistically significant effect on drug purchases, whether measured by total costs or at the extensive margin. The 95% pointwise confidence intervals in columns 9–10 rule out changes in total drug costs larger than 0.02 SEK per 1 SEK increase in benefits. Appendix Table 6 shows similarly precise (and statistically insignificant) estimates when decomposing drug costs into out-of-pocket payments and publicly reimbursed "benefit costs".

6.3 Implications for Fiscal Externalities

I use the estimates in Section 6.2 to bound the magnitude of the healthcare-related fiscal externality, i.e. the term $FE^{\text{health}}(b)$ in Proposition 1, in the Swedish context. A measurement issue is that my inpatient and outpatient outcomes capture *total resource costs*, which include both patient fees and publicly financed costs, whereas only the publicly financed component enters the government budget. In contrast, for prescription drugs I observe both out-of-pocket payments and reimbursed costs ("Benefit Costs" in Appendix Table 6), allowing a direct estimate of the fiscal component on this margin.

I therefore bound $FE^{\text{health}}(b)$ under three assumptions about the public share of inpatient and outpatient costs, while using the observed benefit-cost component for drugs. First, under the conservative assumption that inpatient and outpatient costs are borne entirely by patients (so that only drug reimbursements enter the public budget), the implied 95% confidence interval for $FE^{\text{health}}(b)$ ranges from -0.001 to 0.011 SEK per 1

29. Appendix Figure 6(c) shows a similar non-linearity in drug purchases *before* the start of the unemployment spell.

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 4). 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.

SEK increase in benefits.[30] Second, at the other extreme, assuming that inpatient and outpatient costs are fully publicly financed (i.e., patient fees are zero), the implied 95% confidence interval ranges from -0.094 to 0.129 SEK.[31] Third, using Sweden's average public financing shares for inpatient and outpatient care reported in Section 3, the implied 95% confidence interval ranges from -0.044 to 0.085 SEK.[32]

Taken together, these bounds indicate that even under generous assumptions about public financing, marginal increases in UI generosity do not generate large healthcare-related fiscal externalities in the Swedish context. For comparison, the classical fiscal externality from behavioral responses in UI spell duration—the term $\left(1+\theta\frac{\kappa(m_U,h_U)}{b}\right)\varepsilon_{1-e,b}$ in Proposition 1—lies between 0.84 and 0.96 SEK per 1 SEK increase in benefits in my data.[33]

6.4 Effect Heterogeneity

The main takeaway from Sections 6.2–6.3 is that UI generosity has little effect on recipients' healthcare use, whether measured by costs, the number of visits, or at the extensive margin. I next examine whether these average effects mask heterogeneity across recipient groups or across types of care. Figure 4 summarizes the results.

Studying such effect heterogeneity is of interest for three reasons. First, existing evidence suggests that the adverse health effects of unemployment may be heterogeneous;

30. This bound uses the 95% confidence interval for the effect of UI benefits on drug benefit costs from Appendix Table 6 and assumes that none of the estimated change in inpatient or outpatient costs accrues to the public budget.

31. This bound sums the lower and upper endpoints of the 95% confidence intervals for (i) drug benefit costs (Appendix Table 6) and (ii) total inpatient and outpatient visit costs (Table 2).

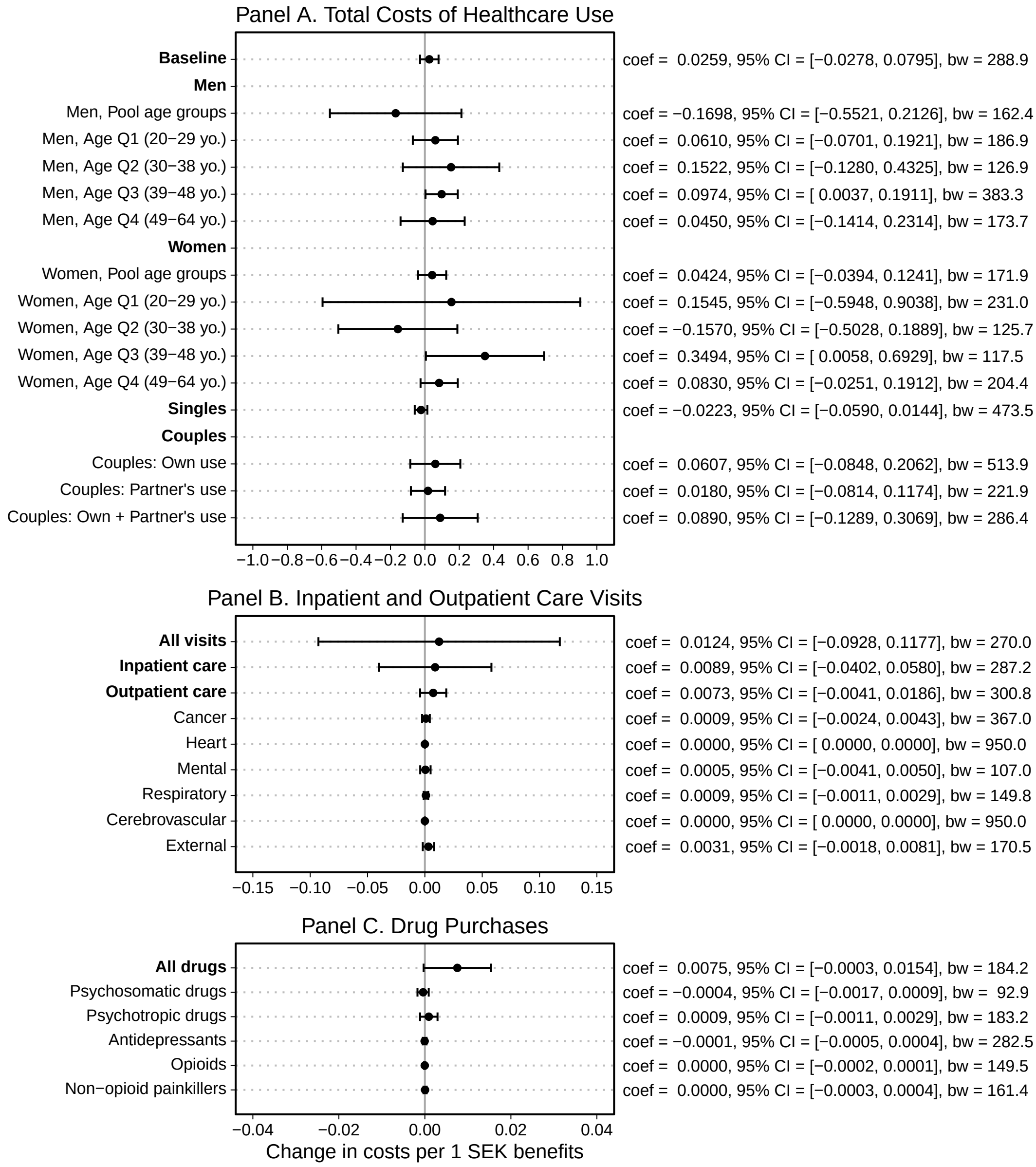
32. Let s^{in} and s^{out} denote the public shares of inpatient and outpatient spending, respectively. Using the average out-of-pocket shares in Section 3, $s^{in}=0.99$ and $s^{out}=0.86$. I combine (i) the estimated effects of benefits on total inpatient and outpatient visit costs, ΔC^{in} and ΔC^{out} (Table 2), and (ii) the estimated effect on drug benefit costs, ΔC^{drug} (Appendix Table 6), as

$$FE^{\text{health}}(b)=s^{in}\cdot\Delta C^{in}\;+\;s^{out}\cdot\Delta C^{out}\;+\;\Delta C^{drug}.$$

I apply these weights to the endpoints of the corresponding 95% confidence intervals and sum the lower endpoints and the upper endpoints.

33. I proxy $\varepsilon_{1-e,b}$ by the elasticity of UI benefit spell duration with respect to the benefit level, and I proxy b by mean total UI benefits received over the first 40 weeks since spell start. I proxy $\theta\kappa(m_U,h_U)$ by mean healthcare costs over the same window and consider bounds for the publicly financed component. In Section 6.1, I estimate $\hat{\varepsilon}_{1-e,b}=0.83$. Furthermore, individuals in the analysis sample on average received $b=64,198$ SEK in UI benefits, incurred 9,235 SEK in inpatient and outpatient costs, and generated 1,046 SEK in publicly financed drug costs (benefit costs). This implies $\theta\kappa(m_U,h_U)/b\in[0.02,0.16]$, where the lower bound assumes inpatient and outpatient costs are fully borne by individuals (so only drug reimbursements enter the public budget) and the upper bound assumes they are fully publicly financed. Therefore, $\left(1+\theta\frac{\kappa(m_U,h_U)}{b}\right)\varepsilon_{1-e,b}\in[0.84,0.96]$.

Figure 4: 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 11 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 4). Outcomes 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 to the right of 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.

for example, smaller for women and larger for the long-term unemployed and for mental health outcomes (see Picchio and Ubaldi 2023). Second, behavioral responses to UI can differ across groups (e.g., Ahammer and Packham 2023; Ferey et al. 2024), and unemployment and UI may generate spillovers to partners (e.g., Cullen and Gruber 2000; Marcus 2013; Hendren 2017; Gathmann et al. 2025). Third, meaningful effect heterogeneity could provide a rationale for differentiated UI policies where benefit generosity varies with recipient characteristics, such as age (Akerlof 1978; Spinnewijn 2020).

Effects by gender and age group. Figure 4A reports estimates (along with 95% confidence intervals) for the effect of unemployment benefits on total healthcare costs by gender and age quartile. The youngest quartile includes ages 20–29 and the oldest ages 49–64. While estimates are noisier for some subgroups (especially the youngest quartile) and for men and women aged 30–48 statistically significant (which could happen by chance given the multiple comparisons), there is no systematic pattern suggesting that more generous UI increases or decreases in-/outpatient or drug costs for any group.

Effects for singles vs. couples. The bottom panel of Figure 4A splits the sample by marital/cohabitation status in the calendar year before spell start ("Couples", 47.8% of spells, vs. "Singles"). For couples, I estimate effects on total healthcare costs for the recipient and partner, both separately and combined. I find no statistically significant effects in any case. The 95% pointwise confidence intervals rule out changes larger than 0.06 SEK per 1 SEK increase in benefits for singles and 0.31 SEK for couples, where the latter corresponds to the sum of recipient and partner costs.

Effects by type of healthcare use. Figures 4B and 4C report estimates separately for categories of healthcare visits and drug purchases. Appendix Table 11 summarizes how I define these categories.

For inpatient and outpatient visits, I follow Kuhn et al. (2009) and consider broad categories related to cancers, cardiovascular conditions, mental health conditions, respiratory conditions, cerebrovascular conditions, and external causes (accidents and self-harm). Prior work finds that job loss can increase hospitalizations related to mental health and external causes (Kuhn et al. 2009; Eliason and Storrie 2009; Gathmann et al. 2025). In my setting, the estimates are consistently close to zero, with confidence intervals tight enough to rule out meaningful effects across categories.

For drug purchases, I again follow Kuhn et al. (2009) and examine psychotropic drugs intended to treat psychological distress (e.g., sedatives, benzodiazepines, antidepressants) and psychosomatic drugs intended to treat physical ailments linked to prolonged stress (e.g., migraine therapeutics and anti-inflammatory drugs). As in Ahammer and Packham (2023), I also consider antidepressants specifically and drugs related to chronic pain (opioids and non-opioid painkillers). The estimates are again centered near zero with tight confidence intervals across all categories, including those most plausibly linked to stress-related mechanisms.

Effects over time. The null average effect could still mask dynamics, for example if any impacts emerge gradually as health deteriorates during unemployment. To examine this, Figure 5 plots weekly estimates of the effect of UI generosity on healthcare costs from 52 calendar weeks *before* spell start through 40 weeks *after*. The pre-spell weeks serve as placebo tests to rule out sorting around the kink point, such as from health shocks just before unemployment (an "Ashenfelter dip"). Figure 5 shows no evidence of dynamic effects: estimates for total healthcare costs (Panel A), in-/outpatient visit costs (Panel B), and drug purchase costs (Panel C) are stable and close to zero both before and after spell start.

6.5 Validity Tests and Sensitivity Analyses

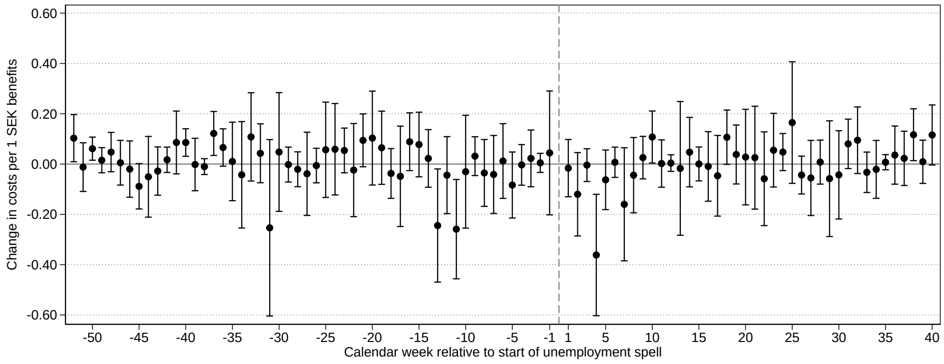
This section summarizes evidence supporting the validity of the RK design and the robustness of the main findings.

Manipulation of running variable. Figure 2B(ii) shows no visible discontinuity or kink in the density of the daily wage at the benefit-cap kink. Consistent with this, both a McCrary (2008) test for a discontinuous jump and a test in the spirit of Landais (2015) and Card et al. (2015) for a kink in the density function fail to reject smoothness at the kink (see notes to Figure 2 for details).

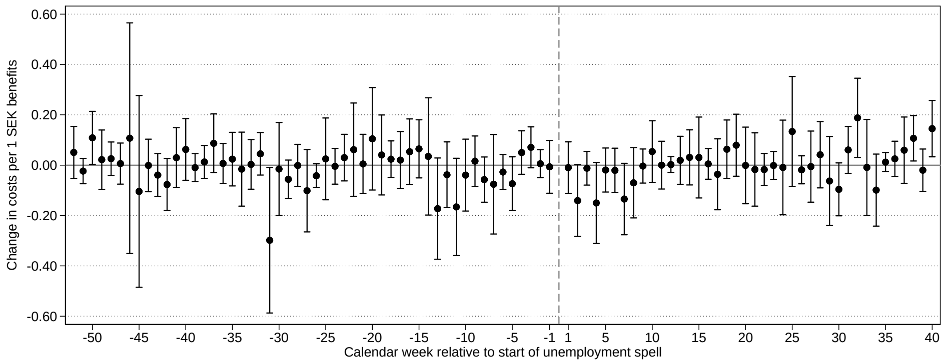
Smoothness of pre-determined covariates and placebo outcomes around kink. Three complementary exercises support the assumption that other determinants of healthcare use evolve smoothly around the kink point. First, Appendix Figure 4 shows that predicted

Figure 5: Effects on Healthcare Use Before and Over the Unemployment Spell

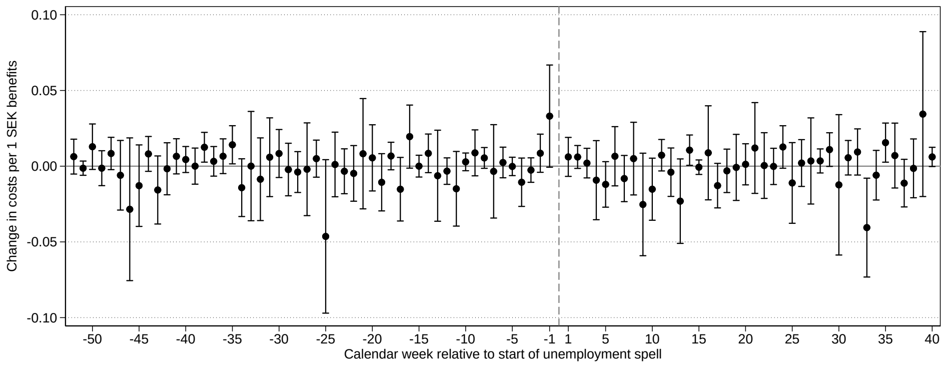
Panel A. Total Costs of Healthcare Use



Panel B. Total Costs of Inpatient and Outpatient Care Visits



Panel C. Total Costs of Drug Purchases



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 4). 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).

healthcare costs based on pre-determined covariates do not exhibit a kink; Appendix Table 8 reports the corresponding estimates.[34] Second, Appendix Figure 5 and Appendix Table 9 show that key pre-determined covariates vary smoothly around the kink. Third, Appendix Figure 6 shows no kink in healthcare costs measured in the 12 months *before* spell start; Appendix Table 10 confirms the placebo test results.

Sensitivity to bandwidth choice. Appendix Figure 7 reports estimates and 95 percent confidence intervals across a range of bandwidths. For total healthcare costs and in-/outpatient costs, estimates are stable and centered near zero for bandwidths both below and above the MSE-optimal choice. For drug purchases, estimates using wider bandwidths become negative and statistically distinguishable from zero, but the implied magnitudes are economically negligible.[35]

Alternative specifications. Appendix Figure 8 shows that the results are robust to alternative polynomial orders, kernels, and covariate adjustment, and to reporting conventional versus bias-corrected estimates. I emphasize bias-corrected inference because conventional RK confidence intervals can have too low coverage rates (Card et al. 2017). Finally, Appendix Figure 3 shows that varying the winsorization threshold for healthcare costs primarily affects precision: point estimates remain close to zero, while confidence intervals widen noticeably without winsorization, consistent with the right-skewness of healthcare expenditures.

7 Discussion

Section 6 shows that marginal increases in UI generosity generate at most modest changes in healthcare use and costs. This section discusses why limited responses could arise given the institutional context and the conceptual framework in Section 2.

Margin of policy variation. First, my estimates reflect *incremental* changes in benefit generosity among individuals already eligible for earnings-related UI. Many studies finding larger effects leverage more extensive variation, such as changes in program eligibility

34. Appendix Table 7 reports the regressions used to construct these covariate indices.

35. For example, with a 500 SEK bandwidth, the point estimate implies that a 1 SEK increase in benefits reduces drug purchase costs by 0.003 SEK (SE = 0.001).

(e.g., Card et al. 2009), benefit duration (e.g., Ahammer and Packham 2023), insurance coverage (e.g., Kuka 2020), or discrete policy reforms (e.g., Brot-Goldberg et al. 2017). When the baseline UI system already enables substantial consumption smoothing, small benefit changes may induce limited healthcare responses relative to policies that alter coverage or generate larger income shocks.[36]

Institutional context. Second, the Swedish setting (Section 3) implies that UI generosity does not affect healthcare use through health insurance coverage. Healthcare is universally available at low out-of-pocket prices, private supplementary insurance is uncommon, and eligibility for publicly funded care does not depend on employment status. As a result, any UI-induced changes in healthcare use are more likely to operate through income, stress, or behavioral channels than through coverage changes. This contrasts with settings where insurance is employment-linked or privately financed, and unemployment or UI reforms can directly affect insurance status.

Generosity of UI and healthcare systems. Third, unemployment benefits in Sweden are relatively generous by international standards (OECD 2026). Combined with universal coverage and low out-of-pocket prices, this suggests that the income elasticity of healthcare spending may be low on this margin in this context. Importantly, the benefit cap still generates salient incentives: I find effects on UI benefit spell duration (Appendix Figure 1), and related work shows that the same kink point affects consumption (Landais and Spinnewijn 2021). Rather, if liquidity constraints are limited and individuals can smooth consumption through savings or other household resources, marginal increases in UI benefits may have limited effects on health investments and healthcare use.

Offsetting behavioral responses. Finally, as discussed in Section 2, UI generosity may affect healthcare use through multiple channels that can operate in opposite directions. Higher benefits may reduce psychological stress, improving health and lowering healthcare needs. At the same time, higher benefits may relax budget constraints and reduce

36. In the model in Section 2.2 and Appendix A, a marginal increase in the benefit level b affects healthcare use through its effect on consumption, which enters the marginal rate of substitution between health and consumption that pins down optimal healthcare use (Appendix A, Equations 8–9). When baseline benefits and other resources already smooth consumption in unemployment so that c_U is close to c_E, marginal utility of consumption is similar across states, implying that a marginal change in b induces only small changes in optimal healthcare use.

sensitivity to out-of-pocket prices when these are binding, increasing demand. If these forces offset, the net effect on utilization may be small.

Summary. Taken together, the limited healthcare responses are consistent with a context characterized by universal coverage, relatively generous UI, and marginal changes in benefit generosity. This does not imply that income support or social insurance policies are generally irrelevant for health or healthcare use. Rather, my findings indicate that, in a high-income welfare state, incremental increases in UI generosity among existing recipients generate at most modest changes in healthcare use.

8 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 benefits affect healthcare use. The null effects are robust across specifications and hold across subgroups (gender and age), for partners, and across multiple measures and categories of healthcare use. In the Swedish setting, these results suggest that any adverse health effects of unemployment may operate largely through channels that matter independently of income, such as social stigma or loss of social contacts and identity (e.g., Jahoda 1982), rather than through income loss itself.

Despite the richness of the register data, my analysis has some limitations. First, I do not observe primary care or dental care visits. Second, I do not consider potential spillovers to children.[37] Third, due to data limitations, my cost measure for healthcare visits is based on a coarse categorization of visits to 28 groups. Fourth, my research design identifies the effect of marginal benefit changes for individuals near the benefit-cap kink.

I close by highlighting two directions for future work. First, while UI generosity does not appear to affect healthcare use in Sweden, effects may be larger in settings where out-of-pocket costs are higher and consumption smoothing is more difficult (Chetty and

37. Recent work highlights intergenerational spillovers in the context of social insurance and transfer programs (see e.g., Barr et al. 2022; Bailey et al. 2024).

Looney 2006). Second, it is important to study whether benefit generosity affects healthcare use in the context of other social insurance programs, such as disability insurance (see e.g., Gelber et al. 2023). Because healthcare is heavily publicly financed in many developed economies, even modest policy-induced changes in healthcare use could generate fiscal externalities that matter for optimal program design. Detecting such effects requires comprehensive measures of healthcare costs, similar to the one used in this paper.

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Online Appendix

UNEMPLOYMENT INSURANCE GENEROSITY AND HEALTHCARE USE: EVIDENCE FROM SWEDEN*

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A Proofs and Derivations

This appendix provides derivations for Section 2.2 and a proof of Proposition 1.

Setup. There are two states $s \in \{E,U\}$, where E denotes employment and U unemployment. The individual ("she") has initial assets A determined exogenously outside the model. She chooses search effort e at disutility cost $\psi(e)$, where $\psi^{'}(e) > 0$ and $\psi^{''}(e) > 0$, and effort determines the employment probability $e \in [0,1]$. She earns a wage w when employed; when unemployed, she receives unemployment benefits b from the government, financed by an actuarially fair tax $\tau(b)$ levied when employed. Assume an interior solution and standard regularity conditions throughout.

The individual derives utility from consumption c and health h, with expected utility

$$U = e\, v(c_E, h_E) + (1-e)\, u(c_U, h_U) - \psi(e),$$

where $v(c,h)$ and $u(c,h)$ capture (possibly) state-dependent preferences over consumption and health when employed and unemployed, respectively. I assume $v(\cdot)$ and $u(\cdot)$ are increasing and concave in both c and h.

Health h_s in state $s \in \{E,U\}$ depends on exogenously determined baseline health h_0 and healthcare use m_s,

$$h_s = H_s(m_s; h_0), \quad s \in \{E,U\}, \tag{1}$$

with $H_s^{'}(m) > 0$ and $H_s^{''}(m) < 0$. Total resource costs of healthcare use are given by

$$\kappa(m_s, h_s), \tag{2}$$

where $\kappa_m(m,h) > 0$ and $\kappa_h(m,h) \leq 0$. Out of total costs $\kappa(m,h)$, the government pays $\theta\kappa(m,h)$ while the individual pays $(1-\theta)\kappa(m,h)$, where $\theta \in [0,1]$ is the public financing share (so $1-\theta$ is the out-of-pocket share).

Individual's problem. The individual chooses consumption and healthcare use in each state, (c_s, m_s) for $s \in \{E,U\}$, and search effort e to maximize expected utility

$$\max_{c_E, c_U, m_E, m_U, e} V(c_E, c_U, m_E, m_U, e) = e\, v(c_E, h_E) + (1-e)\, u(c_U, h_U) - \psi(e),$$

subject to the health production technologies (1) and the budget constraints

$$c_E + (1-\theta)\kappa(m_E, h_E) \leq A + w - \tau(b),$$
$$c_U + (1-\theta)\kappa(m_U, h_U) \leq A + b.$$

Importantly, as in Chetty and Finkelstein (2013), I assume that the individual takes the tax and benefit levels $(\tau(b), b)$ as given when making her choices.

Denoting the Lagrange multipliers on the budget constraints in states E and U by λ_E and λ_U, respectively, the Lagrangian is

$$\begin{aligned}\mathscr{L} =& e\,v(c_E, h_E) + (1-e)\,u(c_U, h_U) - \psi(e) \\ &+ \lambda_E\,(A + w - \tau(b) - c_E - (1-\theta)\kappa(m_E, h_E)) \\ &+ \lambda_U\,(A + b - c_U - (1-\theta)\kappa(m_U, h_U)).\end{aligned}$$

For an interior solution, the first-order conditions are

$$e\,v_c(c_E, h_E) = \lambda_E \tag{3}$$

$$(1-e)\,u_c(c_U, h_U) = \lambda_U \tag{4}$$

$$e\,v_h(c_E, h_E)H_E'(m_E; h_0) = (1-\theta)\lambda_E\left[\kappa_m(m_E, h_E) + \kappa_h(m_E, h_E)H_E'(m_E; h_0)\right] \tag{5}$$

$$(1-e)\,u_h(c_U, h_U)H_U'(m_U; h_0) = (1-\theta)\lambda_U\left[\kappa_m(m_U, h_U) + \kappa_h(m_U, h_U)H_U'(m_U; h_0)\right] \tag{6}$$

$$\psi'(e) = v(c_E, h_E) - u(c_U, h_U). \tag{7}$$

Combining (3) with (5) and (4) with (6) yields

$$\frac{v_h(c_E, h_E)H_E'(m_E; h_0)}{v_c(c_E, h_E)} = (1-\theta)\left[\kappa_m(m_E, h_E) + \kappa_h(m_E, h_E)H_E'(m_E; h_0)\right] \tag{8}$$

$$\frac{u_h(c_U, h_U)H_U'(m_U; h_0)}{u_c(c_U, h_U)} = (1-\theta)\left[\kappa_m(m_U, h_U) + \kappa_h(m_U, h_U)H_U'(m_U; h_0)\right]. \tag{9}$$

Equations (8) and (9) show that, in both states, the individual's optimal choices of consumption and healthcare equate the marginal rate of substitution between health and consumption to the *out-of-pocket* marginal cost of healthcare.

Government's problem. The government chooses the benefit level b that maximizes the individual's expected utility, subject to a balanced government budget constraint and accounting for the individual's endogenous choices of consumption, healthcare use, and search effort,

$$\begin{aligned}&\max_b W(b) = e\,v(c_E, h_E) + (1-e)\,u(c_U, h_U) - \psi(e) \\ &\text{s.t.} \quad e\,\tau(b) = (1-e)\,b + \theta\left[e\,\kappa(m_E, h_E) + (1-e)\,\kappa(m_U, h_U)\right], \\ &\qquad e = e(b), \quad c_s = c_s(b), \quad m_s = m_s(b), \quad s \in \{E, U\}.\end{aligned} \tag{10}$$

I now prove Proposition 1 in Section 2.2.

Proof of Proposition 1. Differentiating $W(b)$ in (10) with respect to b and using the first-order conditions of the individual's problem (Envelope theorem) gives

$$\frac{dW(b)}{db} = (1-e)\,u_c(c_U, h_U) - e\,v_c(c_E, h_E)\frac{d\tau}{db}.$$

At an interior optimum b^*, $dW(b)/db = 0$, so that

$$\frac{u_c(c_U, h_U)}{v_c(c_E, h_E)} = \frac{e}{1-e}\frac{d\tau}{db}. \tag{11}$$

Totally differentiating the government budget constraint and solving for $d\tau/db$ yields

$$\frac{d\tau}{db} = \frac{1-e}{e}\left[1 + \left(1 + \theta\frac{\kappa(m_U, h_U)}{b}\right)\varepsilon_{1-e,b}\right] + \theta\,\frac{1-e}{e}\,\frac{d\kappa(m_U, h_U)}{db} + \theta\,\frac{d\kappa(m_E, h_E)}{db}, \tag{12}$$

where $\varepsilon_{1-e,b} = \frac{d(1-e)}{db}\frac{b}{1-e}$ is the elasticity of the probability of being unemployed with respect to the unemployment benefit level.

Plugging (12) into (11) and rearranging yields

$$\frac{u_c(c_U, h_U)}{v_c(c_E, h_E)} = 1 + \left(1 + \theta\frac{\kappa(m_U, h_U)}{b}\right)\varepsilon_{1-e,b} + \theta\left[\frac{d\kappa(m_U, h_U)}{db} + \frac{e}{1-e}\frac{d\kappa(m_E, h_E)}{db}\right],$$

which corresponds to the expression in Proposition 1. □

B Administrative Data Sources

My empirical analysis draws from the following administrative register data sets.

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. 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 individuals deregistered from the PES.

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). 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.

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. earnings-related benefits) under which payments were made.

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 6 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 care visits from the *National Patient Register* (Socialstyrelsen 2022b, 2022c). Primary care and dental 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. Diagnoses are recorded using the International Statistical Classification of Diseases and Related Health Problems 10th Revision (ICD-10) codes, while active ingredients are recorded using Anatomical Therapeutic Chemical (ATC) codes.

C Measuring Inpatient and Outpatient Care Use

C.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 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.[38]

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 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 28 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

38. 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.

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 6 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 4), I cannot assign costs for 0.5 percent of inpatient care visits and 1.0 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.

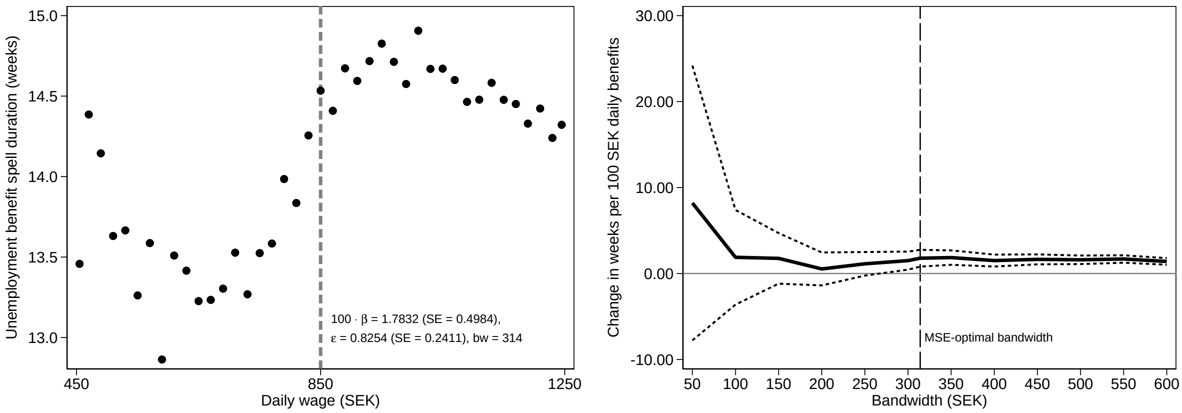
C.2 Number of Inpatient Care and Outpatient Care Visits

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.

D Supplementary Figures and Tables

Appendix Figure 1: Unemployment Benefit Spell Duration Around the Kink Point

Panel A: Binned scatterplot | Panel B: Sensitivity to bandwidth

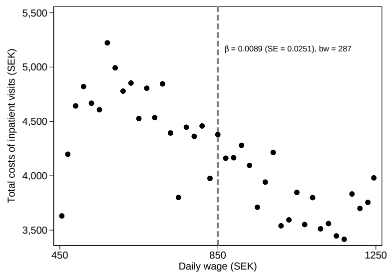


Notes. This figure shows binned scatterplots and coefficients of the effect of unemployment benefits on the duration of the unemployment benefit spell. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 4). Outcome is the duration of the unemployment benefit payment spell measured in weeks. Panel A shows a binned scatterplot of the outcome against the daily wage using a bandwidth of 400 SEK and 20 SEK bins. The plot also reports 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. Panel B 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 line indicates the MSE-optimal bandwidth (Calonico et al. 2014b), which is used for the main estimate.

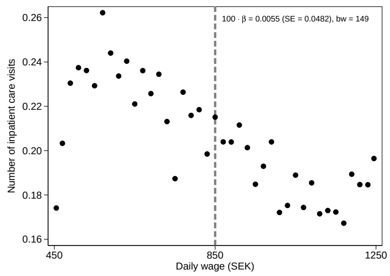
Appendix Figure 2: Inpatient and Outpatient Care Use Around the Kink Point

Panel A: Inpatient Care Visits

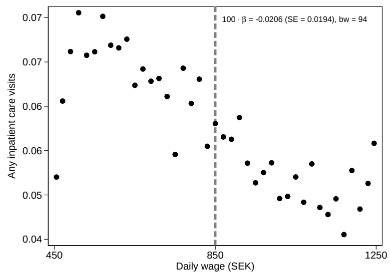
(i) Total costs



(ii) Number of visits

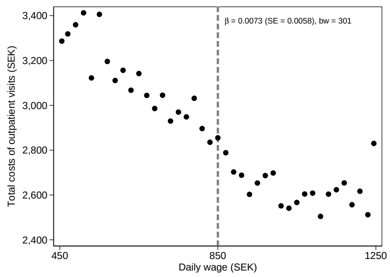


(iii) Any visits

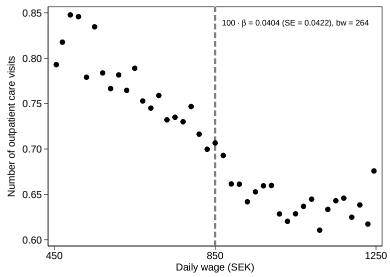


Panel A: Outpatient Care Visits

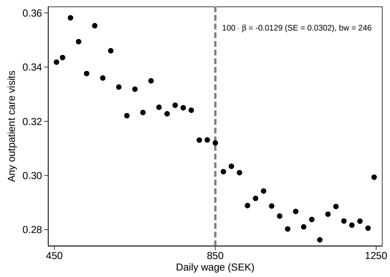
(i) Total costs



(ii) Number of visits



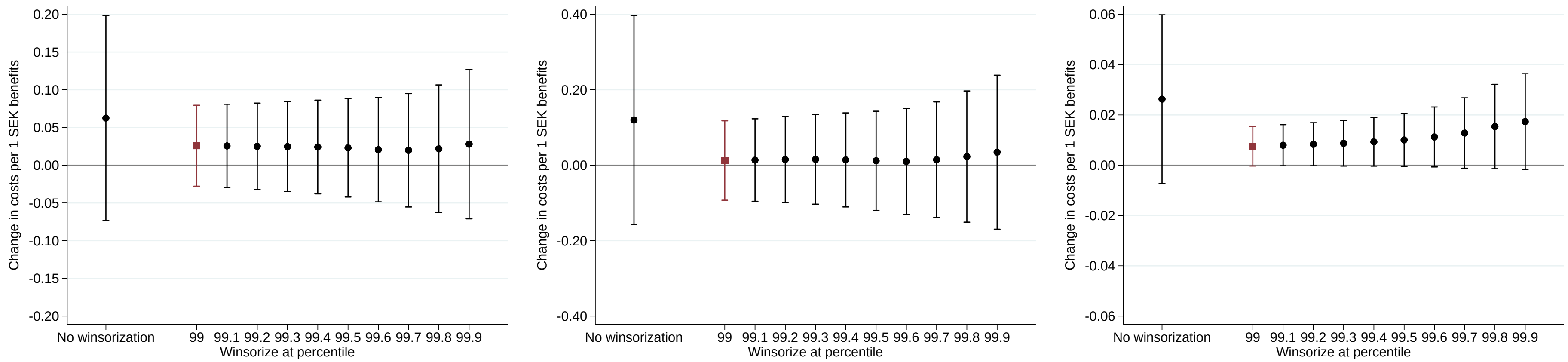
(iii) Any visits



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 4). 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

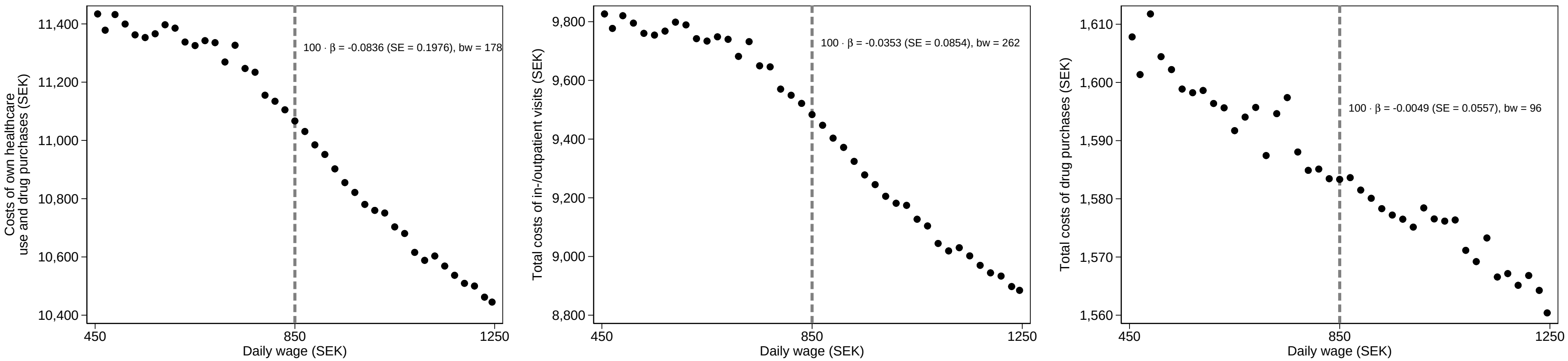
(a) Total Healthcare Use (b) Inpatient and Outpatient Care Visits (c) Drug Purchases



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 4). 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

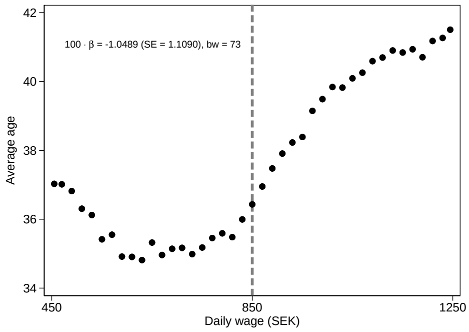
(a) Total Healthcare Use (b) Inpatient and Outpatient Care Visits (c) Drug Purchases



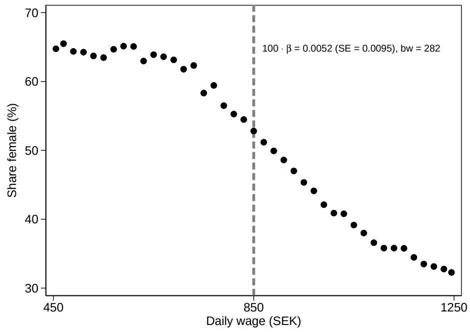
Notes. This figure shows binned scatterplots of predicted costs of healthcare use as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 4). The outcomes 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 7 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 the Kink Point

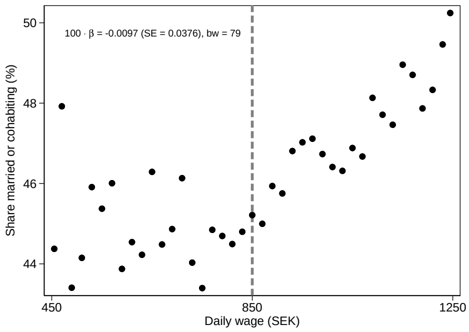
(a) Average age



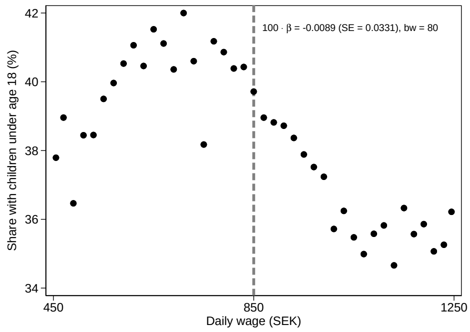
(b) Share female



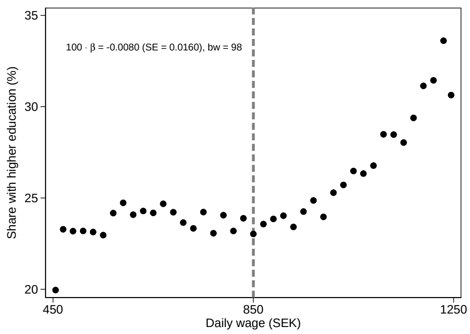
(c) Share married or cohabiting



(d) Share with children under age 18



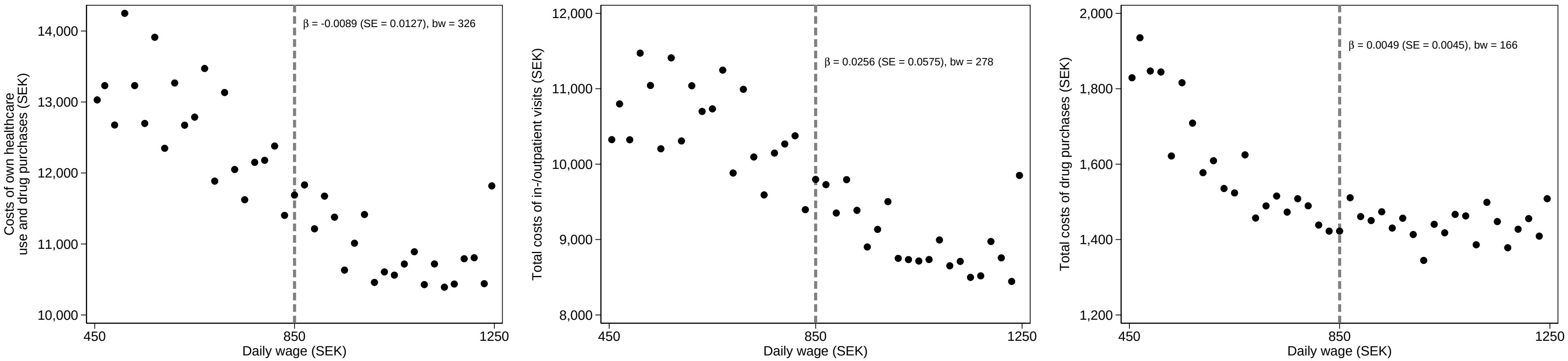
(e) Share with higher education



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 4). 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 the Kink Point

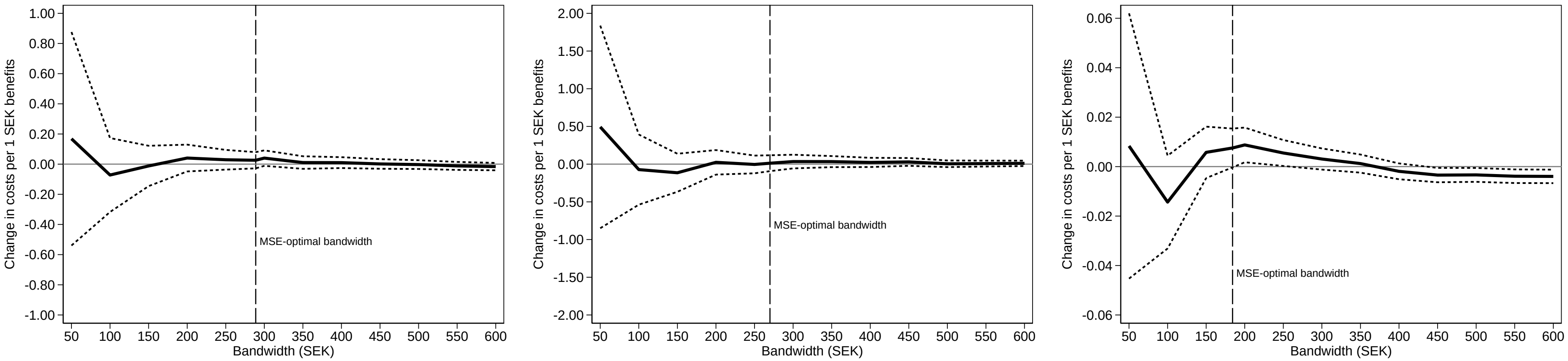
(a) Total Healthcare Use (b) Inpatient and Outpatient Care Visits (c) Drug Purchases



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 4). 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

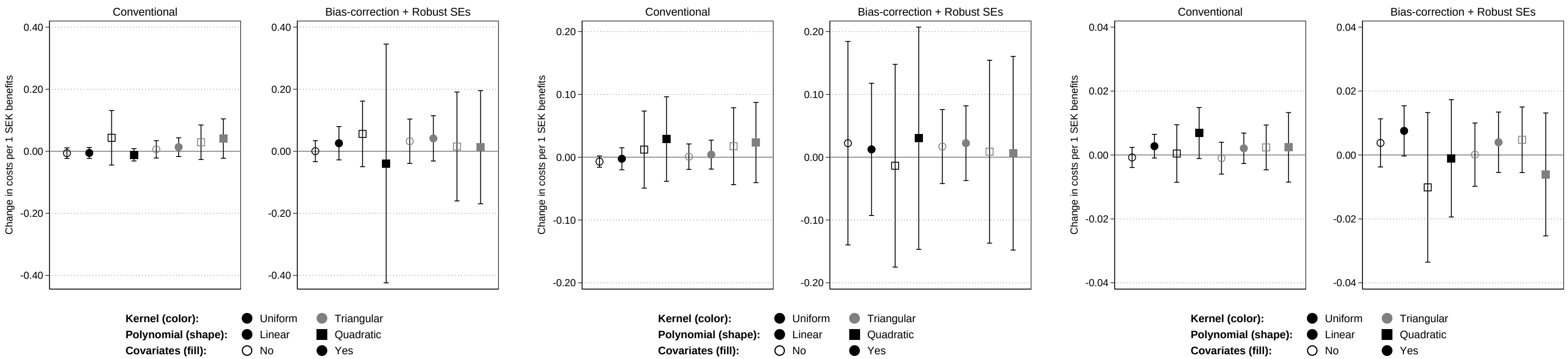
(a) Total Healthcare Use (b) Inpatient and Outpatient Care Visits (c) Drug Purchases



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 4). 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 4). 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 C describes in detail how I calculate the average per-day costs.

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

	Unemployment benefit spell duration	
First stage estimates		
Change in daily benefits per 1 SEK daily wage	-0.739***	-0.728***
	(0.004)	(0.008)
	[-0.746,-0.732]	[-0.744,-0.712]
Fuzzy RK estimates		
Change in spell length (weeks) per 100 SEK daily benefits	1.040***	1.783***
	(0.255)	(0.498)
	[0.541,1.539]	[0.806,2.760]
Implied elasticity		
% Change in spell length per 1% increase in daily benefits	0.481***	0.825***
	(0.109)	(0.241)
	[0.268,0.695]	[0.353,1.298]
Covariates		✓
Mean spell length around kink point (weeks)	14.5	14.5
Bandwidth (SEK)	221.1	314.1
Number of observations	173,611	229,551

Notes. This table presents coefficients and standard errors of the effect of unemployment benefits on the duration of the unemployment benefit spell. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 4). 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. Outcome is the duration of the unemployment benefit payment spell measured in 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), row 9 shows the MSE-optimal bandwidth, and row 10 shows 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 4). 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	-0.7349	-0.7331	-0.7363	-0.7147	-0.7206	-0.7186	-0.7363	-0.7243	-0.7340	-0.7230
per 1 SEK daily wage										
	(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	-0.6293	-0.3540	0.6572	-0.8710	-0.4992	-2.0594	1.0551	-1.2925	-1.5558	-4.0380
per 100 SEK daily benefits										
	(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	-0.0645	-0.0363	0.1359	-0.1801	-0.5765	-2.3785	0.2271	-0.2782	-0.1754	-0.4552
per 1% change in daily benefits										
	(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 healthcare use 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 4). 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 or outpatient care visits or drug purchases (columns 1–2), an indicator for having any inpatient or outpatient care visits (columns 3–4), an indicator for having any inpatient care visits (columns 5–6), an indicator for having any outpatient care visits (columns 7–8), and an indicator for having any drug purchases (columns 9–10). 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), row 9 shows the MSE-optimal bandwidth, and row 10 shows the number of observations within the bandwidth.

Appendix Table 6: Effect of Unemployment Benefits on Costs of Drug Purchases

	Total Costs		Out-of-Pocket Costs		Benefit Costs	
First stage estimates						
Change in daily benefits per 1 SEK daily wage	-0.7385	-0.7367	-0.7392	-0.7403	-0.7304	-0.7379
	(0.0038)	(0.0041)	(0.0040)	(0.0034)	(0.0065)	(0.0037)
	[-0.7460,-0.7309]	[-0.7448,-0.7286]	[-0.7470,-0.7314]	[-0.7468,-0.7337]	[-0.7431,-0.7177]	[-0.7452,-0.7305]
Fuzzy RK estimates						
Change in costs per 1 SEK benefits	0.0038	0.0075	-0.0000	0.0010	-0.0009	0.0051
	(0.0038)	(0.0040)	(0.0008)	(0.0007)	(0.0050)	(0.0031)
	[-0.0038,0.0113]	[-0.0003,0.0154]	[-0.0017,0.0016]	[-0.0004,0.0023]	[-0.0107,0.0089]	[-0.0010,0.0112]
Implied elasticity						
% Change in costs per 1% change in benefits	0.4673	0.9348	-0.0091	0.3120	-0.1898	1.0869
	(0.4951)	(0.5084)	(0.2516)	(0.2073)	(1.0468)	(0.7020)
	[-0.5031,1.4377]	[-0.0617,1.9313]	[-0.5021,0.4840]	[-0.0943,0.7183]	[-2.2414,1.8618]	[-0.2891,2.4628]
Covariates		✓		✓		✓
Mean costs around kink point (SEK)	1085.5	1085.5	427.1	427.1	632.4	632.4
Bandwidth (SEK)	209.1	184.2	177.1	198.0	238.9	393.5
Number of observations	165,208	147,479	142,263	157,499	185,378	265,537

Notes. This table presents coefficients and standard errors of the effect of unemployment benefits on costs of drug purchases. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 4). 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 total (out-of-pocket + benefit) costs of drug purchases (columns 1–2), the out-of-pocket costs of drug purchases (columns 3–4), and the benefit costs of drug purchases (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), row 9 shows the MSE-optimal bandwidth, and row 10 shows the number of observations within the bandwidth.

Appendix Table 7: 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 8: 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 4). 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 7 presents the estimation results from these regressions.

Appendix Table 9: 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 4). 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 10: 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 4). 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 11: 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 4. 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.