The Duration and Wage Effects of Long-Term Unemployment Benefits: Evidence from Germany's Hartz IV Reform*

Brendan M. Price
Federal Reserve Board

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Abstract

In 2005, Germany responded to persistently high unemployment by enacting the controversial Hartz IV reform, which lowered the generosity of long-term unemployment assistance available to workers who exhausted regular unemployment insurance. Using date-stamped administrative data, I exploit cross-cohort and within-cohort variation in the timing of the benefit cuts to estimate how Hartz IV affected workers' jobless durations and subsequent wages. Workers exposed to Hartz IV find jobs faster in anticipation of benefit cuts, exhibit a larger "spike at exhaustion" than was evident pre-reform, and are 11.1 percent less likely to experience a one-year jobless spell. The cuts had little net effect on wages: exposed workers accept lower-paying jobs, but they do so at shorter durations associated with greater wage recovery. Hartz IV's impetus to individual job-finding—if not offset in general equilibrium—may have lowered Germany's steady-state unemployment rate by 0.7 percentage points, contributing to the "employment miracle" of the late 2000s.

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1 Introduction

In the early 2000s, Germany was derided as the "sick man of Europe", afflicted with sclerotic economic growth and persistently high unemployment (Dustmann et al., 2014). Among the many explanations for Germany's poor labor market performance, many observers blamed the disincentive effects of its generous unemployment benefits, which combined an initial stream of unemployment insurance (UI) with a second stream of long-term unemployment assistance (UA) (Tompson, 2009). Under mounting political pressure, the government enacted a sweeping series of labor market reforms that culminated in January 2005 with the controversial "Hartz IV" reform. Hartz IV cut long-term benefit levels for most of the 2.3 million incumbent UA recipients—who comprised 5.7 percent of the labor force—as well as for unemployed workers who subsequently exhausted UI.[1] The Hartz reforms, and Hartz IV in particular, are often credited with kicking off Germany's "employment miracle" (Burda and Seele, 2020), which saw the unemployment rate fall from 10.7 percent in December 2004 to 7.2 percent in December 2009 and 4.6 percent in December 2014.[2]

Hartz IV is widely regarded as one of the most significant social insurance reforms in recent decades. The Economist (2004) called Hartz IV "Germany's most important labour-market reform since the war", and Hartz IV has become an international byword for efforts to strengthen search incentives among the unemployed (OECD, 2007). But despite sustained academic and policy interest, researchers have struggled to identify Hartz IV's effects on the German labor market. Hartz IV is notoriously difficult to evaluate: it was rolled out uniformly and simultaneously throughout Germany, applied to both new and incumbent claimants, and coincided with a raft of other labor market reforms. In this paper, I overcome these challenges by exploiting cross-cohort and within-cohort variation in exposure to Hartz IV, based on the potential duration of UI benefits. In doing so, I provide the first causal estimates of Hartz IV's effects on job-finding and wages among unemployed workers.

[1] Source: Federal Employment Agency; Federal Statistical Office.
[2] Source: Federal Statistical Office; International Labour Organization definition of unemployment.

Using date-stamped administrative data on UI claims filed by prime-age German workers during 2001–2005, I show that exposure to Hartz IV increases the hazard rate of reemployment beginning several months before the benefit cuts bind for a given worker. The rising hazard rate—indicative of forward-looking behavior on the part of jobseekers—culminates in a much larger spike in job-finding at UI exhaustion than was evident before the reform. I estimate that exposure to Hartz IV reduces the likelihood of having a one-year jobless spell by 4.1 percentage points (p.p.), a proportional decline of 11.1 percent. These results are robust to a slew of alternative specifications, a placebo exercise using pseudo-reforms, and an analysis that addresses the possibility of dynamic selection.

My primary employment concept excludes "mini-jobs", a legally defined class of low-paid, marginal jobs that often supplement UI receipt. Broadening the definition of work, I find that Hartz IV diverted claimants from mini-jobs into those covered by social insurance. This result adds an important nuance to arguments that the Hartz reforms fueled the rise of alternative work arrangements, which have attracted growing attention in the United States as well as Europe (Katz and Krueger, 2019; Mas and Pallais, 2020).

My research design disentangles Hartz IV from the first three Hartz reforms, and it further nets out any mechanisms unrelated to the timing of exposure to Hartz IV. While political and academic debate has centered on lower benefit generosity, Hartz IV may have operated partly through other channels—such as stricter job-search requirements or stigmatization of long-term benefits—that increase workers' sensitivity to UI exhaustion. I show that workers responded strongly to the severity as well as the timing of benefit cuts, suggesting that the cuts were indeed central to the law's overall impact.

I next analyze workers' wages upon being reemployed. Benefit cuts have theoretically ambiguous effects on wages (Schmieder et al., 2016; Nekoei and Weber, 2017). Lower benefits may prompt workers to accept lower-paying jobs at a given point in time by lowering the value of remaining unemployed. But if joblessness reduces earnings capacity through stigma or skill depreciation, then benefit cuts may instead mitigate wage declines by shortening jobless spells. Consistent with less selectivity, I find that—*conditional on completed jobless duration*—workers who accept jobs after exhausting UI earn up to 7.4 percent lower wages than they would have absent Hartz IV. But averaging across completed durations, accounting for reductions in time out of work, and correcting for selection into reemployment, I find that Hartz IV had at most a slight *unconditional* effect on mean wages. While the sign varies across specifications, my estimates rule out a change in wages exceeding 1.8 percent in either direction. In short, job gains among UI claimants do not appear to be accompanied by any substantial decline in wages, relative to what claimants would counterfactually have earned.

This paper makes two contributions. First, I present the first quasi-experimental evaluation of the effects of Hartz IV on job-finding and wages among unemployed workers.[3] Previous microeconomic studies do not convincingly address the many identification challenges presented by Hartz IV.[4] On the macroeconomic side, the existing literature has reached widely divergent conclusions about the effect of Hartz IV on Germany's unemployment rate, with headline estimates ranging from 0.1 to 2.8 p.p. (Krause and Uhlig, 2012; Krebs and Scheffel, 2013; Launov and Wälde, 2013, 2016; Bradley and Kügler, 2019; Hochmuth et al., 2019; Hartung et al., 2025). Although my paper is silent on general equilibrium mechanisms such as crowdout effects or endogenous job creation, I find that the partial equilibrium effect of Hartz IV on job-finding—if not augmented or offset by market-level forces—would have lowered Germany's steady-state unemployment rate by 0.7 p.p. Over half of this decline accrues to the long-term component of the overall unemployment rate, underscoring the role of long-term benefit levels in shaping the incidence of lengthy jobless spells.

My second contribution is to provide new evidence on how long-term unemployment benefits affect jobless durations and wages. While an extensive empirical literature has

[3] A complementary paper by Hartung et al. (2025) analyzes the effect of Hartz IV on separations among *employed* workers who differ in their exposure to benefit cuts in the event of layoff. They argue that Hartz IV lowered Germany's unemployment rate primarily by averting separations into unemployment.

[4] For example, Nagl and Weber (2016) find that UI claimants return to work faster after the reform, but their main estimates are identified by time-series variation and potentially confounded by the effects of Hartz I–III. Engbom et al. (2015) find that wages among previously displaced workers fell relative to those of non-displaced workers during the Hartz era, but their strategy hinges on a strong parallel trends assumption and does not isolate Hartz IV from other components of the reform package.

examined the labor market effects of changes in UI benefits (e.g., Card and Levine, 2000; Lalive, 2008; Johnston and Mas, 2018), long-term unemployment assistance—which is widely used in Europe (Esser et al., 2013)—has received much less attention.[5] Exploiting Swedish reforms that differentially affected benefit levels at different points in an unemployment spell, Kolsrud et al. (2018) find that jobless durations are more responsive to changes in initial benefit levels than to comparable changes in long-term benefits. I complement their paper by evaluating a more sweeping overhaul of long-term benefits and by examining effects on wages and job quality as well as jobless durations. Examining a Spanish reform that reduced the potential duration of long-term benefits for workers aged 52–54, Domènech-Arumí and Vannutelli (2025) find significant reductions in jobless duration, increased labor force exits, and lower wages upon reemployment. In contrast to their setting, Hartz IV applied to the full population of UI claimants, rather than a narrow age group. While both of our papers find large reductions in jobless duration, my results differ in that I find strong evidence of anticipatory job-finding and no substantial effect on wages.

In an influential series of papers, Ljungqvist and Sargent (1998, 2008) attributed European economies' persistently high levels of long-term unemployment to the provision of generous long-term unemployment assistance. Taken together, my results confirm that cutting long-term benefits can lead to substantial reductions in jobless durations, with no clear evidence of deleterious wage effects. My analysis leaves open the question of whether Hartz IV was welfare-improving, as long-term benefits importantly insure against the risk of experiencing a long or permanent jobless spell.

Section 2 describes the institutional setting. Section 3 and Section 4 describe the data and research design. Section 5 estimates the effects of Hartz IV on job-finding and jobless durations. Section 6 explores mechanisms. Section 7 analyzes wages and earnings. Section 8 discusses macroeconomic and welfare implications. Section 9 concludes.

[5] A parallel theoretical literature has analyzed the optimal timing of UI benefits, given that both the moral hazard costs and consumption smoothing benefits of UI may vary over the course of an unemployment spell (Shavell and Weiss, 1979; Hopenhayn and Nicolini, 1997; Shimer and Werning, 2008; Kolsrud et al., 2018).

2 The Hartz IV Reform

Hartz IV overhauled Germany's system of long-term unemployment assistance, with a new benefit schedule accompanied by stricter means testing. In this section, I describe the benefit system before and after Hartz IV. In Appendix A, I use the OECD Tax-Benefit Model to show that most claimants faced significant reductions in benefits under the new regime. In Appendix B, I show that the share of claimants receiving long-term benefits after UI exhaustion fell sharply in the years following Hartz IV.

2.1 Unemployment benefits before Hartz IV

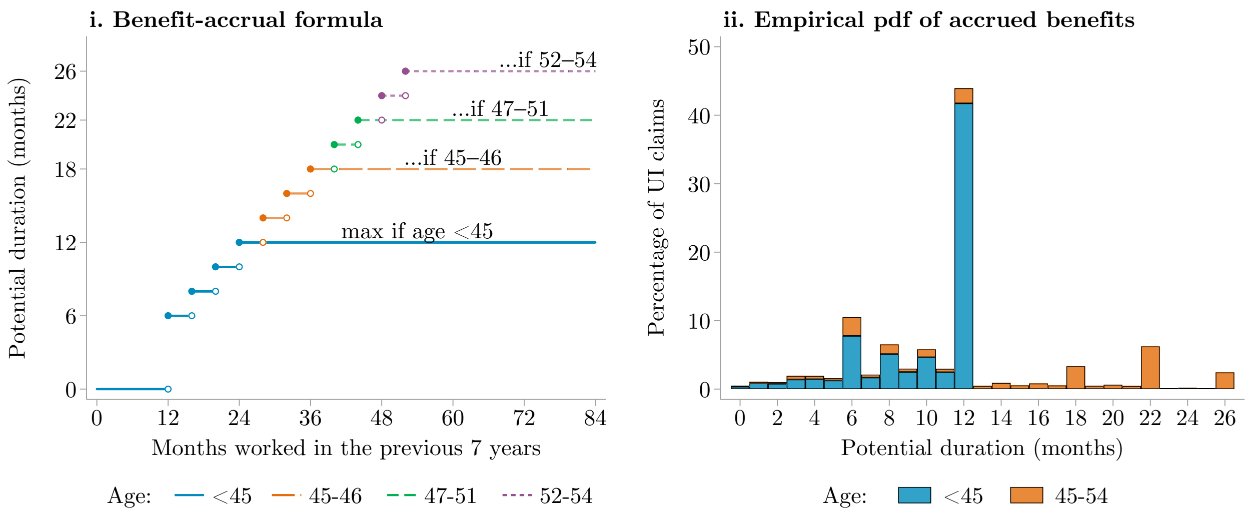
Prior to 2005, Germany had a two-tiered benefit system consisting of limited-duration unemployment insurance (*Arbeitslosengeld*) which, when exhausted, could be followed by a second stream of means-tested unemployment assistance (*Arbeitslosenhilfe*). I refer to these sequential benefit streams as "UI" and "long-term" benefits, respectively.

To receive UI, a claimant must have worked at least 12 months over the preceding 3 years in jobs covered by social insurance. Upon job loss, eligible workers were entitled to benefits equal to 60 percent of their previous after-tax earnings (or 67 percent with dependent children). Benefit payments were not taxed or means tested, and claimants could work up to 15 hours per week under a modest earnings disregard. The potential duration of UI benefits (P), in months, was a step function of months worked over the past 7 years (x), up to a maximum duration $\bar{P}(a)$ determined by age at claim initiation (a):

$$P = \min(\bar{P}(a), 2 \cdot \text{floor}(x/4)), \tag{2.1}$$

Thus 12 months of work yielded 6 months of benefits, and every 4 additional months of work extended the entitlement by 2 months. For workers under 45, benefits were capped at 12 months. For older workers, the cap rose first to 18 months (at age 45), then to 22 months (at age 47), then to 26 months (at age 52), and then to 32 months (at age 57).

Figure 1: Potential benefit duration for new UI claims filed in 2001–2005



Notes: As shown in the left panel, potential benefit duration is a step function of months worked over the past 7 years, up to a ceiling determined by age at entry into UI. The right panel shows the observed pdf of potential duration for claimants in my estimation sample (rounded to the nearest month). Although a standard UI entitlement lasts at least 6 months, seasonal workers can receive briefer entitlements, and potential duration can differ from full entitlements for claimants resuming previous, unexhausted claims.

Although new entitlements lasted at least 6 months, shorter durations were possible for seasonal workers and for those resuming earlier, unexhausted claims.[6] Figure 1 summarizes the benefit formula applicable to my sample of prime-age claimants who entered UI during 2001–2005 (see Section 3), along with the pdf of potential benefit duration in this sample.

Upon exhausting UI benefits, a claimant could apply for long-term unemployment assistance, which was means-tested on the basis of household assets and income. For a worker passing the asset test, benefits equaled 53 percent of prior net earnings (57 percent with children), with reductions for spousal earnings and other sources of income. Long-term benefits lasted indefinitely, subject to annual means testing, and poor households could also apply for means-tested social assistance to top off their unemployment benefits.

The high replacement rate and indefinite duration of long-term benefits set Germany apart from its OECD peers (Wunsch, 2006). The benefit rolls grew steadily in the early 2000s: by June 2004, 1.7 million workers claimed UI and another 2.2 million claimed long-

[6] Seasonal workers were entitled to 3 [or 4] months of benefits if they had worked for at least 6 [8] months. Claimants who exited UI before exhaustion were entitled to their remaining benefits in the event of a new job loss. As a result, potential duration (in days) could assume any integer value from 1 to the maximum.

term benefits (Appendix Figure 1). The growing fiscal burden, together with a widespread view that the safety net was too generous, created political pressure for labor market reform.

2.2 The overhaul of long-term benefits

In March 2002, the German government convened a special commission to recommend a suite of reforms. The Hartz commission's report, released in August, proposed a wide range of carrots and sticks to put the unemployed back to work. The first reform measures, which took effect in January and April 2003 (Hartz I and II) and January 2004 (Hartz III), liberalized the temporary help sector, expanded favorable tax treatment for mini-jobs, provided start-up subsidies for entrepreneurs, and restructured the Federal Employment Agency. But the centerpiece of the reform package was an overhaul of long-term benefits.[7] Hartz IV was passed by the lower house of parliament in December 2003, confirmed by the upper house in July 2004, and implemented on January 1, 2005. The benefit cuts were widely publicized and sparked widespread protests in the summer of 2004.[8]

Hartz IV left the initial stream of UI benefits unchanged. The crux of the reform was to consolidate long-term unemployment assistance and social assistance into a single means-tested income-support program. In contrast to the old regime, long-term benefits would no longer be indexed to prior earnings. Instead, each long-term claimant would receive a standard monthly payment of about €340, plus additional benefits for dependent spouses and children as well as assistance with housing and heating expenses. To ease the transition to the new system, some long-term beneficiaries were also eligible for temporary supplemental payments, which lasted for up to two years after UI exhaustion. Means testing was tightened, and changes in benefit levels were accompanied by tighter job search requirements and stricter

[7] Writing about the Hartz IV benefit cuts in *The New York Times*, Landler (2004) noted, "[E]conomists say these will be the most important measures in the whole package." See Ebbinghaus and Eichhorst (2009) for details on the broader reform package and Tompson (2009) for a nuanced account of the political context.

[8] The reform became salient in July 2004, when the government mailed existing long-term beneficiaries a 16-page questionnaire to gauge their eligibility under the new means test (Tompson, 2009). Consistent with this timing, Google Trends shows a sharp uptick in searches for "Hartz IV" and "Arbeitslosengeld" in the summer of 2004 (Appendix Figure 2). "Hartz IV" was also Germany's 2004 word of the year.

sanctions for those who violated program rules. Importantly, *incumbent claimants were not granted legacy status under the pre-reform rules*: when Hartz IV took effect, those receiving long-term benefits were immediately converted to the new benefit schedule.

The benefit cuts varied greatly across individuals. First, because Hartz IV replaced an earnings-indexed benefit with a uniform benefit, high earners faced steeper cuts. Second, some claimants—such as those with significant assets or high-earning spouses—were ineligible for long-term benefits both before and after the reform, while others who had previously been eligible were disqualified under the stricter asset test. The cuts also depended on numerous other factors, such as parental status and rental expenditures. In Appendix A, I use the OECD Tax-Benefit Model to show that most claimants faced large reductions in direct cash transfers under the new rules, with more modest reductions in net household income. Consistent with tighter eligibility requirements and weaker take-up incentives, Appendix B provides suggestive evidence (subject to data limitations) that the share of claimants receiving long-term benefits fell sharply under the new regime.

Claims initiated after February 1, 2006 were subject to additional policy changes. Under a deferred provision of Hartz III, the lookback period for establishing a UI entitlement fell from three years to two years, and special provisions for seasonal workers were eliminated. Under the Labour Market Reform Act, potential benefit durations were reduced for workers over 45. To focus on Hartz IV proper, I restrict my sample to UI spells that began prior to 2006. While these additional provisions are outside the scope of this paper, they likely contributed to the effects of Germany's broader suite of labor market reforms.

3 Data

I estimate the effects of Hartz IV using administrative data drawn from Germany's Integrated Employment Biographies, accessed under agreement with the Institute for Employment Research (IAB). I rely primarily on the IAB/IZA Administrative Evaluation Dataset (AED), a

4.7 percent random sample of individuals who registered with the unemployment office during 2001–2008 (Eberle and Schmucker, 2015). For each worker, I observe rich demographics, employment status, average daily earnings, and detailed information about periods of unemployment and UI receipt. These work/benefit histories span the years 1993–2010, and all spells are recorded at daily frequency.[9] Since the AED is not representative of pre-2001 flows into unemployment, I also draw on the SIAB, a 2 percent random sample of all individuals who appear in the underlying source data during 1975–2010 (vom Berge et al., 2013).

Sample restrictions. My estimation sample consists of prime-age job losers who entered UI during 2001–2005.[10] Using the AED, I first restrict to claimants aged 25–54 at UI entry to abstract from schooling, apprenticeship, and retirement decisions.[11] Second, to ensure that I am capturing new unemployment spells (rather than claims resumed after brief interruptions), I drop claimants who received UI benefits in the previous 90 days. Third, I restrict to claimants who separated from a socially insured job at most 30 days before entering UI.[12] With these restrictions, some claimants have multiple (disjoint) UI spells. I retain all such spells, so my estimates are representative of new entries into UI.

Jobless duration. Since benefits accrue in 60-day increments, I divide each jobless spell into 30-day "months". I follow each spell until the exact date of reemployment; I censor un-

[9] The IAB data exclude civil servants and the self-employed. Using survey data from the German Socioeconomic Panel, I estimate that 84.1 percent of employed German workers aged 25–54 held jobs that would be detected in the IAB data, with 6.0 percent employed as civil servants and 9.9 percent self-employed.

[10] Restricting to UI claimants excludes both workers ineligible for UI and those who, though eligible, decline to take up benefits. This restriction is needed for the accurate calculation of potential benefit duration. The choice of entry cohorts is dictated by the AED sampling frame—which is representative of UI inflows only from 2001 onward—and by subsequent reforms that applied to UI claims initiated after February 1, 2006.

[11] These prime-age cutoffs coincide with special benefit-sanction rules for claimants under 25 (van den Berg et al., 2014) as well as provisions for partial retirement that kick in at age 55 (Berg et al., 2020).

[12] About three-quarters of new UI spells satisfy this criterion; the remainder are preceded either by voluntary quits, which preclude a worker from claiming benefits for 12 weeks after separation, or by unobserved statuses like self-employment or civil service jobs. Requiring an observed separation lets me better measure the start of nonemployment, ensures that I observe key features of the worker's previous job, and reduces the risk that workers will exit from UI into employment statuses that are unobserved in the IAB data.

finished spells at 24 months but also report results using alternative censoring horizons.[13] In most of my analysis, I define reemployment as returning to a job covered by social insurance, which I call a "regular job". This employment concept excludes tax-favored "mini-jobs", which can pay at most €325–400 per month and which are often held concurrently with benefit receipt (Tazhitdinova, 2020). Since workers may use mini-jobs to supplement, rather than replace, their UI benefits, transitions into regular jobs are likely to be a better measure of claimants' jobless durations and reemployment wages. I revisit mini-jobs in Section 5.4.

Wages and earnings. Employers are required by law to report each worker's average daily earnings at least once per year. Since I do not observe hours worked, I refer to this measure simply as the "wage". I record prior wages using the final wage report for the last regular job preceding UI receipt; I record reemployment wages using the earliest wage report for the first post-UI job. To minimize the influence of outliers, I winsorize all wage measures at the 0.5th and 99.5th percentile of pre-UI wages within the estimation sample.[14] I deflate wages to 2005 EUR using Germany's consumer price index. I also examine monthly earnings, defined as the (daily) wage times days worked during the month, including zeroes.

Benefit receipt. Throughout my analysis period, I observe the timing and monthly benefit amounts of all UI claims. Prior to Hartz IV, I also observe the timing and amounts of long-term benefit claims. Under Hartz IV, however, I do not observe long-term benefit amounts (net of means testing), and data on long-term benefit receipt are incomplete in 2005 and 2006. These data gaps limit the conclusions I can draw about Hartz IV's effect on long-term

[13] I do not prematurely censor spells in cases of labor force exit because (i) non-participants may later rejoin the labor force, (ii) deregistration from unemployment may be endogenous to future UI benefits, and (iii) Hartz IV created a data artifact by requiring welfare recipients to register as unemployed. This data artifact confounds measurement of labor force exit but does not otherwise complicate my analysis.

[14] IAB wage records are topcoded at the maximum earnings level subject to social security contributions, but only 1.8 percent of the estimation sample has pre-UI wages exceeding 98 percent of the wage ceiling. Even fewer claimants have topcoded wages upon being reemployed. Since the 99.5th percentile of the observed wage distribution lies above the statutory topcoding threshold, winsorizing trims these potentially errant wages. I obtain virtually identical results if I use unadjusted wage reports, exclude claimants with censored pre-UI wages, or cap all wage reports at the topcoding threshold in lieu of winsorization.

benefit receipt, but they pose no other challenges for my analysis.

My research design hinges on accurate measurement of the potential duration of UI claims. Because the IAB data do not explicitly record potential benefit duration, I infer it from realized benefit duration together with a variable recording unused benefits (if any) at the end of a UI spell. Appendix C explains this imputation procedure in detail, validates my measure of potential duration, and shows that my results are robust to using an alternative imputation that relies only on information observed prior to UI entry.

Control variables. Age and work history are key controls, since they jointly determine potential benefit duration. I partition claimants into seven age bins: five-year bins below age 45, then bins mirroring the age cutoffs in the UI benefit schedule (45–46, 47–51, and 52–54). I assign workers to one-year "experience" bins of days worked in regular employment over the past seven years. As additional controls, I assign claimants to three household types, three education groups, East/West residence, and native or non-native status.[15]

Summary statistics. Appendix Table 1 presents summary statistics for the estimation sample, which consists of about 337,000 new UI claims filed by 245,000 distinct individuals. Relative to employed workers, claimants are adversely selected on predictors of potential earnings: they have less work experience and lower earnings prior to job loss, and they are more likely to reside in East Germany. About half of jobless spells end within six months, while a quarter last over two years. Among claimants reemployed within two years, average monthly earnings in the first new job were about 5 percent below average pre-UI earnings.

The right panel of Figure 1 plots the pdf of potential benefit duration across new UI spells. About half of claimants are massed at their age-specific maximums, especially the 12-month ceiling for workers under age 45. Just over one-sixth of claimants (all over 45) have entitlements over 12 months. A similar share have entitlements no longer than six months.

[15] The household types (defined at UI entry) are unmarried, married without children, and married with children. The education groups are (i) no apprenticeship or university-entrance exam, (ii) apprenticeship or entrance exam only, (iii) and university or polytechnic degree. Because employers often fail to report workers' educational attainment, I impute missing values longitudinally as in Fitzenberger et al. (2006).

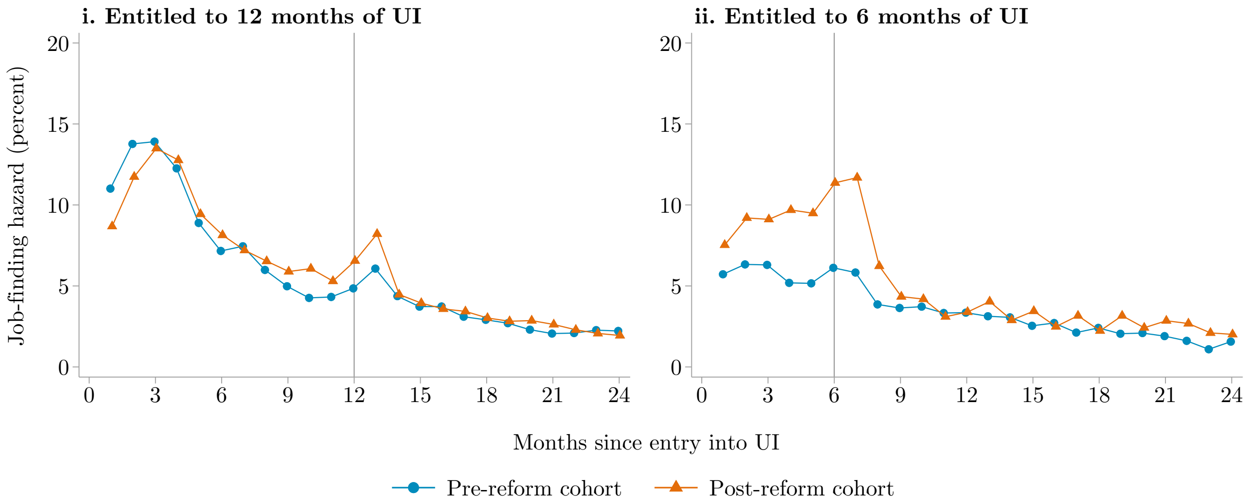
4 Research Design

Hartz IV's uniform rollout, lack of legacy status, and institutional complexity pose challenges for researchers evaluating its effects. If incumbent claimants had been shielded from the reform, one could use a regression discontinuity design to compare claimants who entered UI just before or after the benefit cuts took effect on January 1, 2005. Without legacy protection, however, both groups were subject to the new benefit regime by the time they exhausted UI. Others who entered UI before Hartz IV were *partially exposed* in the sense that the cuts took effect after exhaustion but still relatively early in their jobless spells. Drawing a clean comparison between *fully exposed* and *minimally exposed* claimants would oblige the econometrician to compare cohorts spaced several years apart, but doing so would allow intervening changes in economic conditions or institutions—such as the Hartz I–III reforms adopted in 2003 and 2004—to confound identification of Hartz IV's effects.

To overcome these challenges, I combine cross-cohort variation in the timing of UI entry with cross-claimant variation in potential benefit duration to identify the causal effects of Hartz IV. The basic intuition is that claimants with brief UI entitlements are exposed to Hartz IV soon after job loss, while those with longer entitlements are initially insulated against the benefit cuts. To formalize this idea, Appendix D adapts a standard model of job search to develop two simple predictions. First, cuts to long-term benefits increase the job-finding hazard at all durations, as forward-looking claimants increase their search intensity and lower their reservation wages. Second, these behavioral responses limit to zero as cuts lie increasingly far in the future. Claimants discount distant benefit cuts both because of pure time preference and because they are likely to be reemployed before the cuts bind.

Section 4.1 uses a simple 2×2 comparison to illustrate the logic of my identification strategy. Section 4.2 generalizes this example and introduces the hazard specification I use to estimate the effect of Hartz IV on jobless durations. Section 4.3 develops a complementary, regression-based approach that embeds the same identifying variation. Later in the paper, I adapt these methods to estimate the effects of Hartz IV on wages and other outcomes.

Figure 2: Job-finding hazard rates by timing of UI entry × potential benefit duration



Notes: Raw monthly job-finding hazard rates among claimants entering UI in 2001 ("pre-reform") or 2005 ("post-reform"). All pre-reform spells are completed or censored by December 2003, prior to final passage of Hartz IV. All post-reform spells are subject to the new benefit regime immediately upon UI exhaustion.

4.1 A simple 2 × 2 comparison

To build intuition, I begin with a "difference-in-differences" comparison of job-finding rates between claimants who entered UI in 2001—long before Hartz IV took effect—and those who entered in 2005, when it was fully in place. Within each cohort, I distinguish claimants entitled to 12 months of UI benefits from claimants entitled to only 6 months of benefits.

The left panel of Figure 2 plots the empirical hazard rate of reemployment among claimants entitled to 12 months of benefits (the modal entitlement), separately for those entering UI in 2001 vs. 2005. As in other studies of unemployment, job-finding rates initially rise with duration and then decline, with an uptick in the vicinity of UI exhaustion. All else equal, we would expect the 2005 entrants to find jobs faster than the 2001 entrants, since they incur steeper benefit drops upon exhaustion. At short durations, post-reform claimants in fact have slightly lower job-finding rates, but they exhibit a much larger increase in job-finding as they approach benefit exhaustion than pre-reform claimants do.

Of course, other relevant factors—such as labor demand, credit supply, or the composition of the claimant pool—may have changed in these years. If such changes differentially affect job-finding at different jobless durations, then the uptick in job-finding at around 12 months may not be a true causal effect of Hartz IV. To distinguish greater responsiveness to UI exhaustion from changes in duration dependence unrelated to benefit reform, the right panel plots the hazard rate among claimants entitled to only 6 months of UI. As before, we would expect the 2005 cohort to find jobs faster, but this tendency should—and does—manifest more quickly for the 6-month claimants, for whom Hartz IV binds sooner.

4.2 Benchmark hazard specification

I generalize this 2×2 example by incorporating claimants with any potential benefit duration who entered UI anytime between 2001 and 2005. To do so, I estimate a hazard model that disentangles the pre-reform "main effect" of UI exhaustion from the incremental effect of the Hartz IV benefit cuts. The model allows me to control flexibly for calendar time effects, compositional changes, and other important determinants of individual job prospects, and to obtain causal effects estimated using the full population exposed to Hartz IV.

Timing of benefit changes. Let claimant i begin a UI spell on date u_i. Grouping the data into 30-day "months" indexed by d, I define two key durations at which the benefit level changes. First, d_i^E is the duration at which the claimant is scheduled to exhaust UI:

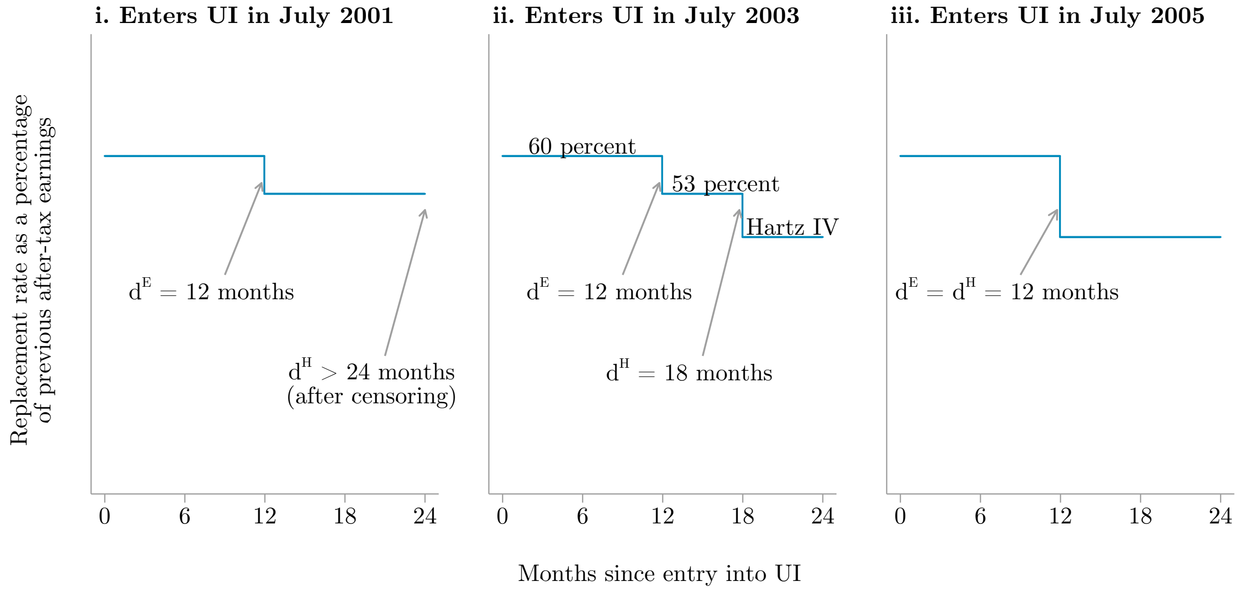
$$d_i^E \equiv P_i \tag{4.1}$$

where P_i is potential benefit duration, measured in months.

Second, d_i^H denotes the duration at which the claimant is scheduled to start receiving long-term benefits under the post-reform rules. Let

$$d_i^{2005} \equiv \min\{d \in \mathbb{N} \mid u_i + 30d \geq \text{January 1, 2005}\} \tag{4.2}$$

Figure 3: Timing of benefit drops for pre-reform, interim, and post-reform cohorts



Notes: Hypothetical benefit schedules for successive cohorts of claimants entitled to 12 months of UI benefits. The fall in replacement rate from 60 percent to 53 percent represents a childless claimant who passes the means test for long-term benefits under the pre-reform rules. The change in benefits under Hartz IV depends on a complex set of individual and household characteristics and varies across individuals (see Appendix A).

denote the first duration observed after the reform legislation takes effect. Then

$$d_i^H \equiv \max\{d_i^E, d_i^{2005}\} \tag{4.3}$$

Hence d_i^H, which may or may not coincide with d_i^E, is the duration at which Hartz IV binds for a given claimant. Figure 3 plots hypothetical examples of these events for successive cohorts of claimants entitled to 12 months of benefits.

Hazard specification. Following Prentice and Gloeckler (1978) and Meyer 1990, I estimate a discrete-time proportional hazard model using the complementary log-log link. Letting D_i denote the completed jobless duration (rounded up to the nearest month), I specify the conditional probability of being reemployed during the dth month of the spell as

$$\lambda_{id} \equiv \Pr(D_i = d \mid D_i > d-1) = 1 - \exp(-\exp(\mathbf{x}'_{id}\beta)), \tag{4.4}$$

where the instantaneous log hazard rate is given by

$$\mathbf{x}'_{id}\beta = \alpha_d + \gamma_t + \mathbf{z}'_{id}\phi + \sum_{k=-9}^{4} \delta_k^E \mathbf{1}\{\tau_{id}^E = k\} + \sum_{k=-9}^{4} \delta_k^H \mathbf{1}\{\tau_{id}^H = k\} \tag{4.5}$$

and $t \equiv u_i + 30d$ denotes the end-of-period calendar date. I estimate the model via maximum likelihood, with standard errors clustered by individual to account for repeated UI spells.

Benefit effects. The key explanatory variables in Equation 4.5 are flexible functions of event time relative to each step-down in the benefit level:

$$\begin{aligned} \tau_{id}^E &\equiv \min\{d - d_i^E, 4\} && \text{(months relative to UI exhaustion)} \\ \tau_{id}^H &\equiv \min\{d - d_i^H, 4\} && \text{(months relative to Hartz IV benefit cut)} \end{aligned} \tag{4.6}$$

The exponentiated coefficients $\exp(\hat{\delta}_k^E)$ and $\exp(\hat{\delta}_k^H)$ express the hazard rate at each event-time relative to the hazard rate 10+ months before a benefit cut. I impose the same "long-run" coefficient on observations 4+ months after each cut.[16] Since the reference group consists of claimants for whom benefit cuts lie far in the future, I am implicitly drawing on the theoretical result in Appendix D that behavioral responses to future benefit cuts limit to zero at long horizons. If claimants start responding to such cuts as early as 10 months beforehand, my estimates will be a conservative lower bound on the true causal effect.

Duration and time effects. In Equation 4.5, α_d is a full set of duration dummies, which allow job-finding rates to vary freely as a function of months since beginning a claim (i.e., there is a nonparametric baseline hazard). γ_t controls for several calendar-time effects. First, I include quarter $\times$ year interactions, which allow the hazard rate to respond to changes in labor market conditions and other aggregate time effects. Second, I include month dummies to absorb higher-frequency seasonal effects, such as retail hiring just before Christmas. Third,

[16] These endpoints trade off flexibility against the need for identifying variation. For example, if the "left" endpoint were to exceed 12 months pre-exhaustion, then some of the coefficients would be identified solely by claimants over 45. Likewise, if the "right" endpoint were to extend much beyond 4 months, then some of the post–Hartz IV coefficients would be identified by only a small subset of cohorts and benefit durations.

I also take γ_t to include interactions between quarter dummies and a set of 3-month duration bins. These interactions allow for seasonal forces that affect workers differently early vs. late in their spells, such as springtime recalls from temporary layoff. To explore sensitivity, I sometimes interact the 3-month duration bins with quarter $\times$ year (rather than quarter alone), thereby allowing patterns of duration dependence to evolve freely over time.

Additional controls. The vector $\mathbf{z}_{id}$ controls for a rich set of observable characteristics. Because τ_{id}^E and τ_{id}^H are correlated with age and experience—the determinants of potential benefit duration—$\mathbf{z}_{id}$ includes indicators for seven age bins and for one-year bins of time worked in the previous seven years. I also control for sex, East/West residence, household type, education, and German nationality, as well as quintiles of prior wage within cells defined by sex $\times$ region $\times$ year of UI entry. I interact these controls with 3-month duration bins, so that both the level and shape of the hazard function may vary across demographic groups.

4.3 A complementary (regression-based) approach

While the hazard specification is well-suited to capturing the complex timing of the Hartz IV treatment, it has two limitations. First, because Hartz IV affects who remains in the risk set at each duration, the hazard model may be biased by dynamic selection (Kiefer, 1988; van den Berg, 2001). Second, because the hazard model treats reemployment as an absorbing state, it is silent about subsequent outcomes, such as whether jobfinders *remain* employed.

To complement my hazard specification, I also use a regression specification that embeds the same identifying variation, namely that claimants with briefer UI entitlements are more exposed to long-term benefit cuts. I begin by restricting the sample to claims initiated in either 2001 ("pre-reform") or 2005 ("post-reform"), for which exposure to Hartz IV is easiest to characterize. Next, I assign each claimant i to one of seven bins $b(i) \in \{1, 2, \ldots, 7\}$ based on their potential benefit duration.[17] For an outcome y_{id} observed d months after entry into

[17] The bins are 0–3 months, 4–6 months, 7–9 months, 10–11 months, exactly 12 months, 13–17 months, and 18+ months. Alternative partitions yield similar results.

UI, I estimate the following linear regression separately for each $d \in \{1, 2, \ldots, 24\}$:

$$y_{id} = \alpha_d + \mathbf{z}_i'\beta_d + \theta_d \mathbf{1}\{\text{Post}_i\} + \sum_{b=1}^{6} \mathbf{1}\{b(i) = b\}(\gamma_d^b + \delta_d^b \mathbf{1}\{\text{Post}_i\}) + \varepsilon_{id} \tag{4.7}$$

The coefficients of interest, δ_d^b, capture pre-/post-reform differences in outcomes for claimants in bin b, relative to differences among claimants entitled to 18+ months of UI benefits (the omitted group). $\mathbf{z}_i$ includes controls for sex, region, age, experience, nationality, education, household type, and quintiles of prior wage, whose effects are allowed to vary with duration.

Each specification has its advantages. The hazard approach situates claimant behavior in "benefit-time"—bringing the empirical analysis closer to theory—and it readily handles the interim cohorts that were exposed to Hartz IV late in their jobless spells. The regression approach excludes these interim cohorts, but it is immune to dynamic selection, since the same estimation sample is used at all durations. It also allows me to analyze a broader array of outcomes, beyond the duration of the initial jobless spell.

5 Effects on Jobless Durations

How did Hartz IV affect jobless durations among UI claimants? I begin by using my benchmark hazard specification to estimate the reform's effect on each claimant's first transition back to work. After considering a suite of robustness checks, I next use my complementary regression-based approach to examine the stability of the new employment relationships induced by Hartz IV. To gauge whether the reform contributed to the rise of marginal employment, I conclude by distinguishing transitions into full-time, part-time, and mini-jobs.

5.1 Benchmark hazard estimates

Following Equations 4.4 and 4.5, I estimate the main effect of UI exhaustion and the incremental effect of the Hartz IV benefit cuts on the hazard rate of being reemployed. I then use the fitted model to estimate the effect of Hartz IV on jobless duration.

Effects on the job-finding hazard. The left panel of Figure 4 plots the main effect of UI exhaustion, expressed as a normalized hazard ratio ($\exp(\hat{\delta}_k^E) - 1$). I confirm a classic finding (Moffitt, 1985; Meyer, 1990; Katz and Meyer, 1990a): conditional on time since UI entry, the job-finding hazard rises sharply as claimants run out of UI benefits.[18] The spike is large: the hazard rate is 44 percent higher at exhaustion than 10+ months before exhaustion.

The right panel shows the effect of Hartz IV ($\exp(\hat{\delta}_k^H) - 1$). The job-finding hazard rises steadily as claimants approach the benefit cuts—suggesting forward-looking behavior—and peaks in the month after Hartz IV binds.[19] The effect of Hartz IV fades rapidly thereafter, echoing prior work that finds declining hazard rates in the aftermath of UI exhaustion. While this pattern could reflect reference-dependent preferences (DellaVigna et al., 2017) or storable job offers (Boone and van Ours, 2012), it could also reflect dynamic selection, which can bias hazard estimates. I show robustness to dynamic selection in Section 5.2 below.

Hartz IV has a large effect on job-finding: the hazard rate is 46 percent higher among claimants who have just undergone benefit cuts ($\tau_{id}^H = 1$) than among those for whom the cuts bind 10+ months in the future. Together with the main effect of UI exhaustion, these estimates imply that the job-finding hazard under Hartz IV is 106 percent greater just after exhaustion than when exhaustion lies far in the future.[20]

[18] Card et al. (2007) question the conventional wisdom about exhaustion spikes: in Austrian data, they find a large spike in exits from registered unemployment but only a small increase in job-finding. The estimates I report throughout the paper reflect true job-finding, not deregistration from unemployment.

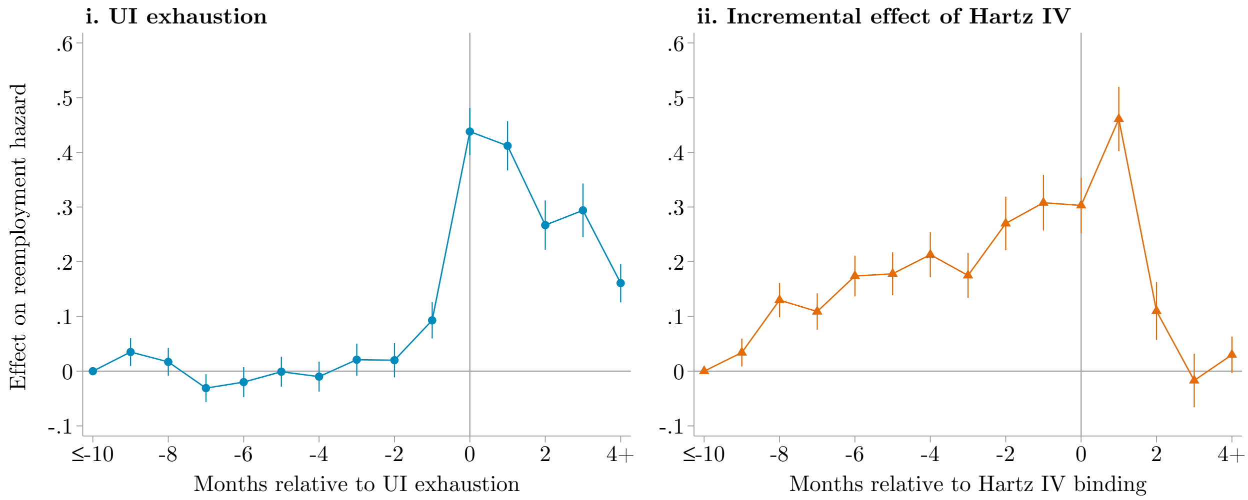
[19] Men and women show similar proportional responses to Hartz IV (Appendix Figure 3), but the main effect of benefit exhaustion is much larger for women. Because long-term benefits are means tested on the basis of spousal earnings, fewer women received such benefits both before and after the reform (see Appendix B). Consistent with this explanation, the exhaustion spike is much larger for married women than for unmarried women, whereas married and unmarried men show similar spikes.

[20] A complementary approach is to see how sensitivity to UI exhaustion differs for claimants subject to pre- vs. post-reform rules. In Appendix Figure 4, I compare job-finding behavior between claimants who enter UI in 2001 vs. 2005. Because each of these claimants is subject to a fixed benefit schedule during the 24-month observation window, I estimate a simpler version of Equation 4.5 separately for each cohort:

$$\mathbf{x}_{id}'\beta = \alpha_d + \gamma_t + \mathbf{z}_{id}'\phi + \sum_{k=-9}^{4} \delta_k^E \mathbf{1}\{\tau_{id}^E = k\} \tag{5.1}$$

The spike at exhaustion is strikingly larger for the post-reform cohort. Quantitatively, the effect of Hartz IV implied by a comparison between these cohorts is almost identical to the Hartz IV effect in Figure 4.

Figure 4: Benchmark effects of benefit drops on the hazard rate of reemployment



Notes: Proportional effects of benefit drops on transitions to employment using the hazard specification in Equations 4.4 and 4.5. The left panel reports normalized hazard ratios corresponding to the main effect of UI exhaustion ($\exp(\hat{\delta}_k^E) - 1$). The right panel reports the incremental effect of Hartz IV ($\exp(\hat{\delta}_k^H) - 1$). Here and in subsequent figures, vertical spikes represent 95 percent confidence intervals.

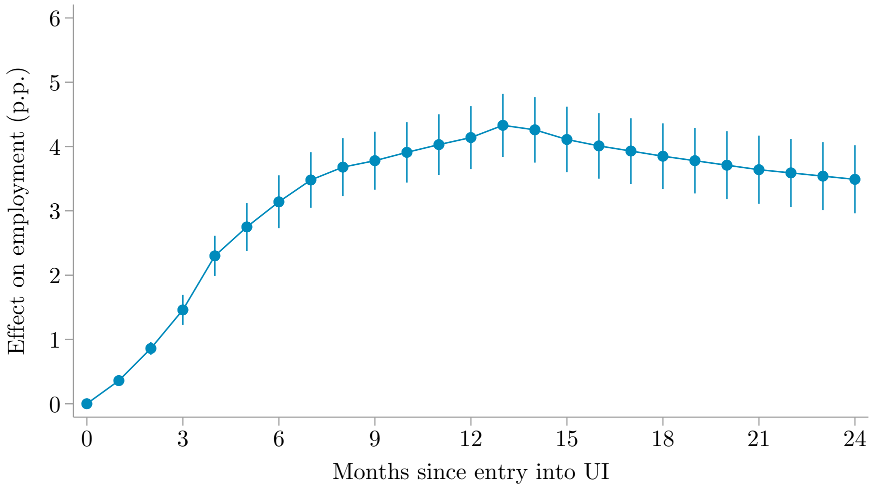
Effects on jobless duration. Hartz IV's effect on jobless durations depends not only on changes in the job-finding hazard, but also on the underlying distribution of durations.[21] To convert hazard effects into duration effects, I simulate successive UI cohorts' jobless durations both under the fitted model—which incorporates the Hartz IV benefit cuts—and under a counterfactual scenario in which these cuts do not occur.

Following Equation 4.4, I use my benchmark estimates to fit each claimant's job-finding hazards $\widehat{\lambda}_{id} = \widehat{\Pr}(D_i = d \mid D_i > d - 1)$, where D_i equals months elapsed until the claimant is reemployed for the first time. Chaining these hazard rates yields the cumulative reemployment probability $\widehat{F}_{id} \equiv \widehat{\Pr}(D_i \leq d) \equiv 1 - \prod_{s=1}^{d}(1 - \widehat{\lambda}_{is})$. To predict counterfactual reemployment rates $\widehat{F}_{id}^{cf}$ absent Hartz IV, I recompute these expressions with the time-to-Hartz variable τ_{id}^{H} recoded to the omitted category, so that the benefit cuts lie far in the future. I estimate the effect on reemployment by averaging $\widehat{F}_{id} - \widehat{F}_{id}^{cf}$ across claimants.[22]

[21] To see this, write the cumulative reemployment rate as $F(d) \equiv \sum_{k=1}^{d} f(k) = \sum_{k=1}^{d} S(k-1)\lambda(k)$, where $S(\cdot) \equiv 1 - F(\cdot)$ is the survival function. Letting $S^{cf}(\cdot)$ and $\lambda^{cf}(\cdot)$ denote counterfactual survival and hazard rates absent Hartz IV, the change in reemployment is approximately $dF(d) \approx \sum_{k=1}^{d} S^{cf}(k-1)\lambda^{cf}(k)\mathrm{dlog}\lambda(k)$. Long-term benefits have larger employment effects when workers tend to reach long-term unemployment (captured by $S^{cf}(\cdot)$) and when, upon doing so, they are on the margin of finding work ($\lambda^{cf}(\cdot)$).

[22] In the spirit of Chernozhukov et al. (2013), I construct confidence intervals by drawing 500 parameter

Figure 5: Effect of Hartz IV on the probability a claimant has ever been reemployed



Notes: Reemployment effects of Hartz IV for the 2005 cohort of UI entrants, who are fully exposed to the new benefit schedule. I use my benchmark hazard estimates to predict the probability of reemployment both under Hartz IV and under a counterfactual in which the benefit cuts do not occur, then average the gap between these probabilities across claimants. Here and in similar figures throughout the paper, I construct 95 percent confidence intervals by taking 500 draws from the estimated covariance matrix.

Figure 5 plots the path of reemployment effects for the 2005 cohort of UI entrants, who were fully exposed to the new benefit schedule.[23] The effects accrue rapidly for the first 13 months after UI entry, then fade slightly as the counterfactual series partly catches up to the observed series. The treatment effect is largely persistent at 24 months, suggesting that benefit cuts have enduring effects on cumulative job-finding even at lengthy durations.[24]

Germany defines long-term unemployment as a jobless spell lasting over one year. I estimate that Hartz IV increased the probability of being reemployed within 12 months by 4.1 p.p., relative to a counterfactual probability of 62.8 percent. In proportional terms, Hartz IV reduced the likelihood of reaching long-term unemployment by 11.1 percent.

vectors using the estimated covariance matrix, replicating the quantification exercise, and taking the standard deviation across estimated effects. I use the same procedure for similar exercises throughout the paper.

[23] Appendix Table 2 reports analogous results for earlier cohorts of UI entrants. For claimants who enter UI in 2001, the predicted effect of Hartz IV is mechanically zero: two years after entry, all workers in this cohort are still at least 10 months away from the benefit cuts, so $\widehat{F}_{id}^{cf} = \widehat{F}_{id}$. The effects of Hartz IV cumulate for successive cohorts, which are increasingly exposed to the reform.

[24] To verify that my results are not sensitive to the censoring horizon, I reestimate my benchmark specification with incomplete spells censored at either 12 months or 36 months. The net reemployment effect continues to shrink beyond 24 months but remains sizable even 36 months after UI entry (Appendix Figure 5).

5.2 Robustness

To show that these job-finding results are robust, I now (i) explore additional controls and sample restrictions, (ii) evaluate the effects of placebo reforms taking effect at other points in time, and (iii) address possible bias from dynamic selection.

Additional controls and sample restrictions. My benchmark Hartz IV effects are robust to a slew of control strategies and sample modifications. I present these robustness checks in Figure 6, with my benchmark estimates reproduced as specification 1.

While the benchmark specification already controls for a rich set of observables, the effects of Hartz IV could be confounded by *unobserved* changes in claimant characteristics across cohorts.[25] To address this concern, specification 2 allows for compositional changes among new claimants by adding an indicator for each quarter $\times$ year of entry into UI.

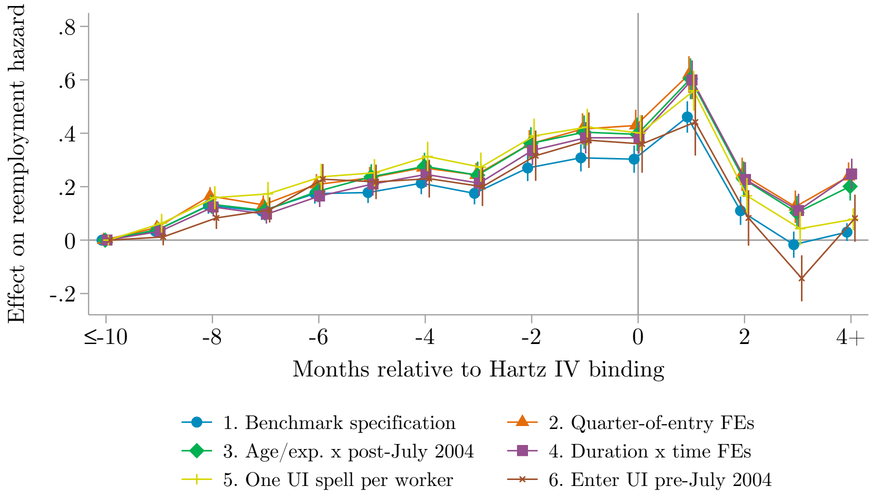
Germany's economic rebound in the mid-2000s may have especially boosted job prospects among younger or less experienced workers, for whom unemployment rates are typically higher. Because these groups tend to have briefer UI entitlements, my research design might falsely attribute increases in their job-finding rates to the benefit cuts. To address this concern, specification 3 allows the age $\times$ duration and experience $\times$ duration interactions in $\mathbf{z}_{id}$ to differ before and after July 1, 2004, when Hartz IV became salient.

The benchmark specification assumes that the shape of the baseline hazard is constant across years. To relax this assumption, specification 4 interacts each quarter $\times$ year indicator with the full set of 3-month duration bins. In effect, this demanding specification compares the job-finding patterns of similar workers who are observed at similar durations at the same point in time, but who differ in their proximity to benefit cuts.

Some claimants appear in multiple, disjoint spells. Although it is not clear why repeat spells would present any problems, specification 5 restricts to a single UI spell per individual

[25] For example, workers laid off in 2005 may be positively selected relative to those laid off during the tighter labor market of 2001 (Mueller, 2017). To assess this concern, Appendix Figure 6 plots mean predicted jobless duration by quarter of entry into UI, based on a Weibull model fitted to fixed claimant characteristics. Predicted duration falls slightly over time, but there is no discontinuity or trend break around Hartz IV.

Figure 6: Robustness of hazard effects to additional controls and sample selections



Notes: Specification 1 replicates the benchmark Hartz IV effects from Figure 4. Specification 2 absorbs cohort heterogeneity by adding quarter-of-entry fixed effects. Specification 3 allows the effects of age and experience to differ before/after Hartz IV became salient. Specification 4 adds interactions between duration and calendar time, allowing the shape of the hazard function to vary freely over time. Specifications 5 and 6 limit the sample as indicated. Here and in subsequent figures, points are offset horizontally for visibility.

by selecting one spell at random among claimants who experience multiple spells.

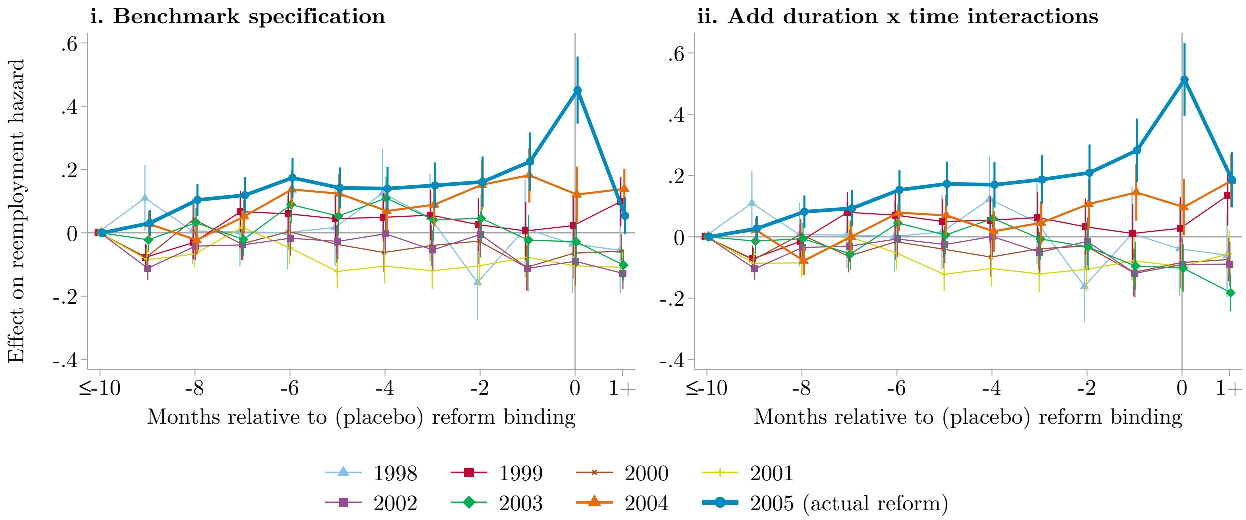
Finally, under a standard model of UI take-up (Anderson and Meyer, 1997), benefit cuts should deter some workers from claiming UI. These marginal claimants—who would claim benefits only under the pre-Hartz rules—may differ unobservably from those who would claim benefits under both regimes. To limit the scope for such differential take-up, specification 6 restricts the sample to workers who enter UI before July 2004.

Consistent with a causal interpretation of my results, the effects of Hartz IV on the job-finding hazard are strikingly stable across these specifications.[26]

Placebo reforms. Was January 2005 unique in generating these effects? Consider two threats to identification. First, suppose that claimants became steadily more responsive to UI exhaustion over time for reasons unrelated to the Hartz reforms. This could occur if, for example, the supply of consumer credit contracted as economic conditions worsened in

[26] My research design exploits variation in potential benefit duration stemming from both age and experience. As an additional robustness check, in Appendix Figure 7 I obtain qualitatively similar results if I restrict the sample to claimants under 45 (isolating variation due to work history) or claimants with maximal durations given their age (isolating variation due to age).

Figure 7: Hazard effects of real and placebo UI reforms



Notes: Proportional hazard effects of pseudo-reforms taking effect on either the actual reform date (January 1, 2005) or a placebo date (January 1 of 1998, ..., 2004). For each reform year Y, I construct a 2 percent sample of new UI claims begun between January 1 of year $Y-4$ and June 30 of year Y, then recode the time-to-Hartz event-time variable to measure time until the benefit cuts bind. The right panel augments each specification with interactions between duration and calendar time.

the early 2000s. Second, suppose that the earlier Hartz measures—implemented in January 2003 and January 2004—differentially affected job-finding among claimants who were close to exhausting benefits. Either phenomenon could potentially result in spurious "Hartz IV" effects even if the benefit reform itself had no causal effect on job finding.

To assess these threats, I estimate placebo specifications that alter the assumed date of the reform to January 1 of each year $Y \in \{1998, 1999, \ldots, 2005\}$.[27] Figure 7 confirms that 2005 was different. In the left panel, the "actual reform" series replicates the benchmark estimates using the smaller SIAB sample, whose sampling frame is better suited to this exercise. For the 1998 through 2003 pseudo-reforms, the estimated placebo effects are close to zero, militating against a secular rise in sensitivity to benefit exhaustion. For 2004, I do find positive placebo effects, but they are reassuringly much smaller than the true Hartz IV effects. In Appendix E, I discuss three possible explanations for these modest placebo effects:

[27] For placebo year Y, I select UI claims initiated between January 1 of $Y-4$ and June 30 of Y, then reestimate Equation 4.5 with event-time recoded based on the placebo reform date. I censor incomplete spells on June 30 of Y to avoid misattributing the causal effect of Hartz IV to the placebo reforms. Given the abbreviated post-"reform" period, I impose a single coefficient for all post-event periods ($\tau_{id}^{H} \geq 1$).

an earlier tightening of the asset test for long-term benefits; a short-lived increase in the frequency of benefit sanctions; and anticipatory responses to Hartz IV itself.

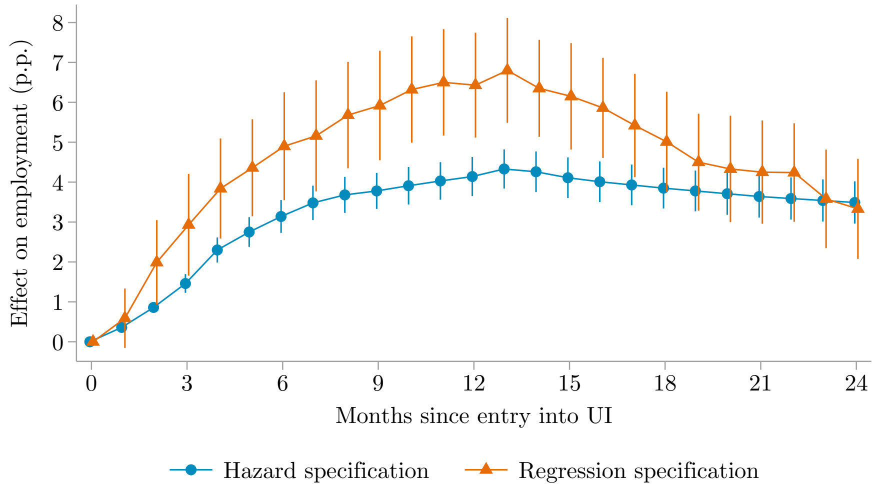
The right panel presents a richer specification that allows the baseline hazard to evolve flexibly over time. This refinement, which more stringently partials out changes in duration dependence unrelated to benefit cuts, strengthens the true effects while attenuating the 2004 placebo effects, lending further support to a causal interpretation of these results.

Dynamic selection. By inducing some claimants to find jobs faster, Hartz IV affects which claimants remain in the risk set of jobless individuals used by the hazard model at each duration. If job-finding rates vary across claimants in ways not captured by observables, then such dynamic selection may bias the estimated treatment effects. In particular, if claimants who are more sensitive to benefit cuts return to work when the cuts bind, then at later durations the risk set will skew towards claimants who are less responsive to the cuts.

To address this concern, I re-estimate the effect of Hartz IV on jobless durations using a regression approach that is immune to dynamic selection (Section 4.3). Let y_{id} denote whether claimant i has ever been reemployed by duration d. Pooling claimants who entered UI in either 2001 or 2005, I run the regression in Equation 4.7 for each $d \in \{1, 2, \dots, 24\}$, with exposure to Hartz IV captured by interactions between a post-reform dummy and bins of potential benefit duration. As in Section 5.1, I quantify the effect of Hartz IV by simulating $\widehat{y}_{id}$ under the fitted model, subtracting a counterfactual fit $\widehat{y}_{id}^{cf}$ under a scenario in which the benefit cuts do not occur, and averaging the difference across the 2005 cohort.

Figure 8 plots the effect of Hartz IV on the probability a claimant is reemployed by each duration. Relative to the hazard-based estimates (in blue), the regression-based estimates (in orange) indicate larger employment effects, which peak at 6.8 p.p. and fade out more quickly at longer durations. Despite somewhat different magnitudes, however, the overall pattern is broadly similar across specifications, suggesting that my results are robust against dynamic selection (and, if anything, larger than my benchmark estimates suggest).

Figure 8: Effect of Hartz IV on the probability a claimant has ever been reemployed (comparing hazard-based vs. regression-based estimates)



Notes: Reemployment effects of Hartz IV for the 2005 cohort of UI entrants, using either the benchmark hazard specification in Equations 4.4 and 4.5 or the regression specification in Equation 4.7. I use the fitted values to predict reemployment both under under Hartz IV and under a counterfactual scenario in which the benefit cuts do not occur, then average the difference in these probabilities across claimants.

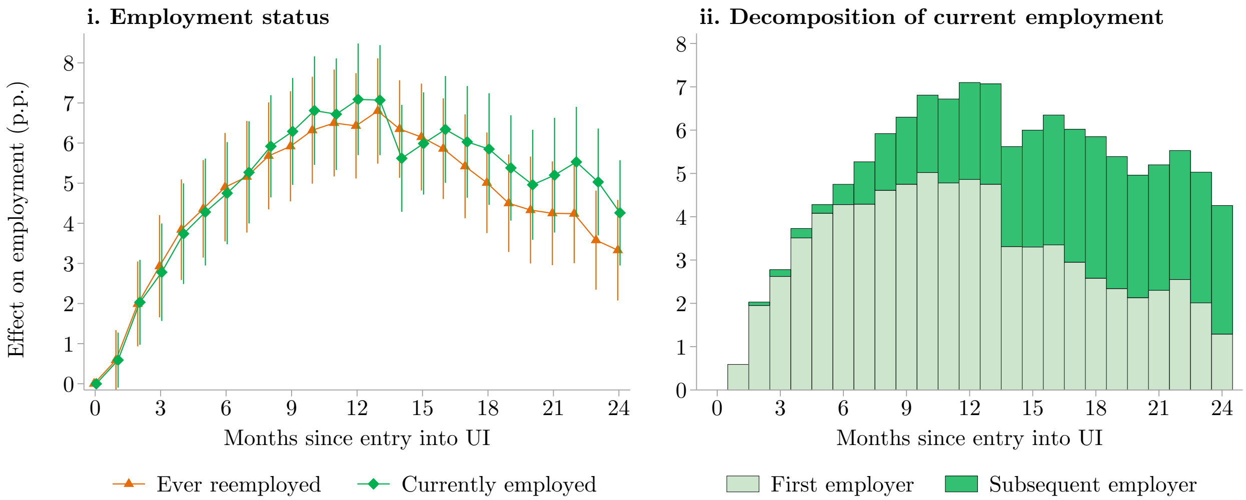
5.3 Stable employment?

Did the jobfinders remain employed? Employment relationships induced by Hartz IV may have been fragile and short-lived, and new hires may have quickly slipped back into unemployment. Alternatively, Hartz IV may have sustained higher employment rates by fostering better matches, deterring separations, or reducing the duration of subsequent jobless spells.

To answer this question, I estimate the effect of Hartz IV on the probability that a claimant is *currently* employed at a given point in time. Because hazard models treat reemployment as an absorbing state, I again follow the regression approach described in Section 4.3 and applied to analyze dynamic selection at the end of Section 5.2.

In the left panel of Figure 9, the orange series reproduces the regression model's effect of Hartz IV on the probability a claimant has *ever* been reemployed (from Figure 8). The green series instead shows effects on *current* employment. The two sets of estimates are strikingly similar, suggesting that Hartz IV induced relatively stable employment. Two years after UI entry, claimants exposed to Hartz IV are 4.3 p.p. more likely to be employed

Figure 9: Effects of Hartz IV on the probability of ever vs. currently being reemployed (regression-based estimates)



Notes: Effects of Hartz IV on the probability an individual has ever been reemployed or is currently employed, using the regression specification in Equation 4.7. In the left panel, the "ever reemployed" series replicates the regression-based series in Figure 8. The right panel decomposes the effect on current employment between the employer where a claimant is initially reemployed vs. any subsequent employer.

than their counterparts who entered UI too early to be exposed.

To further probe employment dynamics among reemployed claimants, the right panel decomposes the effect on current employment based on the identity of the employer. In the first six months, the boost to employment is driven almost entirely by claimants in their first job since entering UI. At later durations, the employment gains are increasingly driven by different employers as workers transition to second or subsequent jobs.

5.4 Full-time, part-time, and mini-jobs

The Hartz reforms coincided with—and may have contributed to—a marked rise in the prevalence of part-time and non-traditional work within the German labor market.[28] To explore how Hartz IV affected employment along the intensive margin of labor supply, I now present competing-risks specifications that track how UI claimants transition into different

[28] Between 2001 and 2007, part-time jobs grew from 25.6 to 30.5 percent of total employment (own calculation, public statistics from the Federal Employment Agency), reflecting roughly equal growth in mini-jobs and in other part-time jobs (Appendix Figure 8). This statistic excludes mini-jobs held as secondary jobs, which first appear in these data in April 2003 and also grew rapidly in subsequent years.

kinds of jobs. Starting with the socially insured jobs I have analyzed up to this point, I first distinguish between full-time and part-time jobs. I then broaden the employment concept to encompass low-paid mini-jobs often held during UI receipt.

Full-time vs. part-time. I adapt my hazard model to allow for competing risks of accepting a full-time or part-time job. For now, I continue to restrict attention to regular jobs: although some part-time jobs are legally classified as mini-jobs, many others are covered by social insurance and hence fall within the employment concept used up to this point. I treat each job type as an absorbing state, abstracting from subsequent transitions between job types. I estimate a separate discrete-time hazard specification for each job type, censoring spells if and when a worker is reemployed into the other kind of job. I use the same explanatory variables as in my benchmark (single-risk) hazard specification.

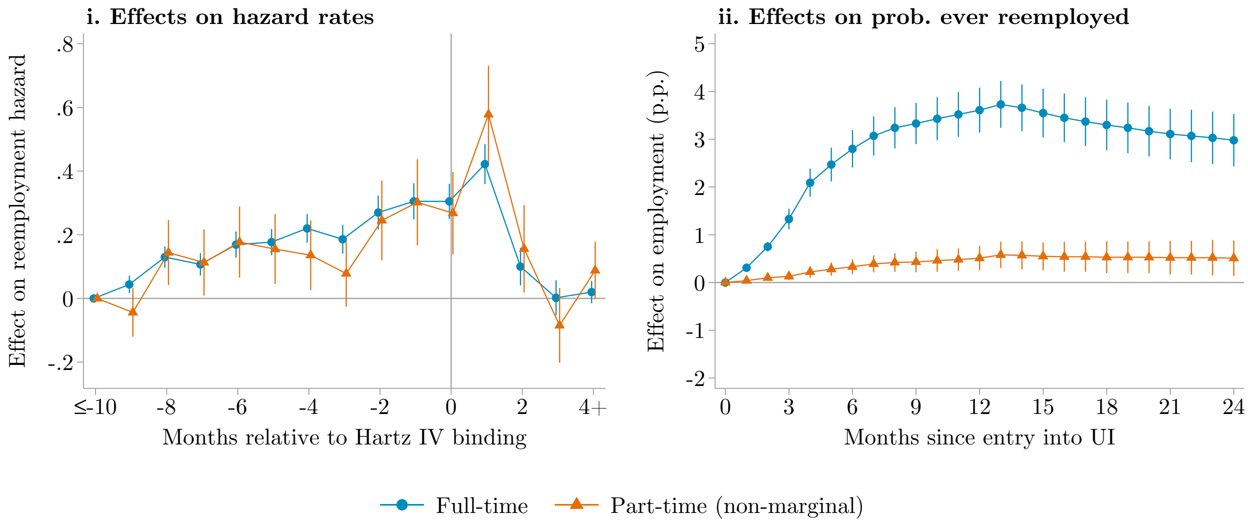
The left panel of Figure 10 plots the estimated effect of Hartz IV on transitions into full-time or part-time jobs. The reform has similar, positive proportional effects on both hazards. But cause-specific hazards are hard to interpret without the strong assumption of independent risks (Heckman and Honoré, 1989). In addition, the hazard effects do not directly reveal how Hartz IV affected the share of workers entering each type of job.

I therefore use the fitted model to estimate cumulative incidence functions, which measure the share of workers absorbed into each job type at each duration (Fine and Gray, 1999).[29] Let D_i denote completed jobless duration, and let $J_i \in \{\text{full-time}, \text{part-time}, \text{no job}\}$ denote the first type of job obtained (with $J_i = \text{no job}$ if $D_i = \infty$). Then the cumulative incidence for job type j at duration d is

$$I_{id}^{j} \equiv P(D_i \leq d \cap J_i = j). \tag{5.2}$$

[29] Cumulative incidence is interpretable without assuming independent risks. To see why, consider an analogy to a randomized controlled trial of a program that helps unemployed workers apply to different kinds of jobs. Under random assignment, the cumulative incidences—i.e., the effect of the treatment on reemployment into each job type—are identified by simple differences of means. But without further assumptions, the econometrician cannot tell how the treatment affected individual search intensity towards each job type.

Figure 10: Effects of Hartz IV on transitions to full-time vs. part-time jobs



Notes: The left panel shows proportional effects of Hartz IV on the competing risks of transitioning into socially insured full-time vs. part-time jobs. The right panel shows the effects of Hartz IV on the cumulative incidence of reemployment into each job type, as computed using UI claims initiated in 2005.

The overall reemployment rate F_{id} can be expressed as a sum of cumulative incidences:

$$F_{id} \equiv I_{id}^{\text{full}} + I_{id}^{\text{part}} \tag{5.3}$$

This identity allows me to decompose the effect of Hartz IV on the path of reemployment rates into full-time and part-time components. Adapting the procedure used in Section 5.1, I obtain predicted and counterfactual cumulative incidence functions for each UI claim. I compute the gap between these functions at durations $d \in \{1, 2, \dots, 24\}$, and I average this gap across all UI claims begun in 2005 and thus fully exposed to the new benefit schedule.

The gap is plotted in the right panel of Figure 10. Net employment gains predominantly represent full-time jobs, consistent with the cross-sectional fact that most regular jobs are full-time. My estimates imply that Hartz IV left the part-time share of employment among reemployed claimants essentially unchanged at 12.5 percent.

Socially insured vs. mini-jobs. My analysis up to this point has excluded "mini-jobs", a special class of low-paid, part-time jobs that are partly exempt from social insurance contributions (Tazhitdinova, 2020; Gudgeon and Trenkle, 2024).[30] Critics of Hartz IV allege that it has fueled the growth of such marginal positions by forcing the unemployed to accept any work they can find, potentially at the expense of job security and other amenities. But long-term benefit cuts have theoretically ambiguous effects on UI claimants' incentives to seek marginal employment. Thanks to the earnings disregard, UI receipt and mini-jobs are not mutually exclusive. The income loss from a benefit cut may induce some claimants to obtain mini-jobs in lieu of pure unemployment, but it also reduces the attractiveness of dual UI receipt/mini-job employment relative to seeking a regular job.[31]

I therefore broaden the definition of reemployment to put regular and mini-jobs on the same footing. To focus on transitions that occur after the onset of UI receipt, I drop the 6.5 percent of claimants who hold a mini-job at baseline. The left panel of Figure 11 plots the estimated effects of Hartz IV on the competing risks of entering regular jobs or mini-jobs, together with the effect on the single risk of entering any job whatsoever. While transitions into regular jobs become more likely as workers approach Hartz IV (the blue series), transitions into mini-jobs become *less* likely (the orange series).

The right panel of Figure 11 plots the implied effects on the cumulative incidence functions. Regular jobs more than account for the net employment gains, and Hartz IV modestly reduced the share of workers drawn into mini-jobs. Though perhaps surprising,

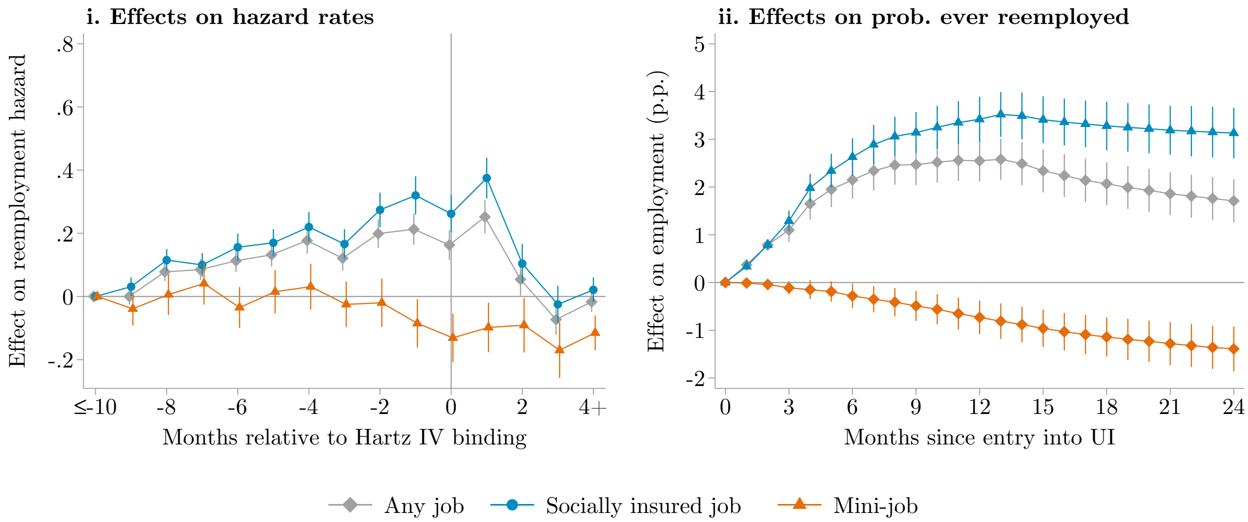
[30] Workers in mini-jobs are exempt from paying social insurance and income taxes, but employers are still liable for their portion of social insurance contributions. Until 2003, mini-jobs were capped at €325 per month, with a maximum of 15 hours per week. In April 2003, Hartz II eliminated the hours ceiling, raised the earnings ceiling to €400 per month, and allowed workers in regular employment to hold a mini-job on the side without increasing their total tax liability. In June 2004, mini-jobs held as a worker's primary job accounted for 15.3 percent of total employment (Appendix Figure 8).

[31] To illustrate, suppose workers choose among pure unemployment with UI benefit b, benefit receipt coupled with a mini-job at wage w_M, or a regular job at wage $w_R > b + w_M$. The corresponding payoffs are

$$\begin{aligned} u_1 &= u(b) \\ u_2 &= u(b + w_M) - c_M \\ u_3 &= u(w_R) - c_R \end{aligned}$$

where c_M and $c_R > c_M$ are the disutilities of marginal and regular work. A benefit cut reduces both u_1 and u_2 while leaving u_3 unchanged; moreover, if $u(\cdot)$ is concave, $\frac{d}{db}(u_1 - u_2) > 0$, so that lower benefits reduce the value of pure unemployment relative to dual claiming/working. The result is that workers switch from strategy 1 to strategies 2 and 3 and from strategy 2 to strategy 3, and the net effect on mini-jobs is unclear.

Figure 11: Effects of Hartz IV on transitions to regular vs. mini-jobs



Notes: In the left panel, the grey series plots the proportional effects of Hartz IV on the hazard rate of entry into a job of any kind, redefining employment to include "mini-jobs" alongside regular jobs. The blue and red series plot proportional effects of benefit cuts on the competing risks of entry into regular vs. mini-jobs. The right panel shows the effects on average reemployment rates (grey) and cumulative incidence functions (blue and orange) for the 2005 cohort. (Equation 5.3 holds only approximately in the estimated model, so the single-risk series does not exactly equal the sum of the cause-specific series.)

this result is quite plausible, since Hartz IV made it less attractive for workers to supplement long-term benefit receipt with marginal employment in lieu of a regular job.[32]

6 Benefit Cuts and Alternative Mechanisms

I have shown that Hartz IV increased the sensitivity of job-finding to UI exhaustion. While reductions in long-term benefit levels are the obvious explanation for this result, the reform may have operated partly through other channels, such as changes in caseworker behavior, greater stigmatization of long-term transfer receipt, or media hype about the severity of the cuts. To establish the central role of benefit levels themselves, I next compare job-finding behavior between claimants exposed to different-sized cuts. These results establish tight links among the timing, magnitude, and effects of long-term benefit cuts, and they militate

[32] Although Hartz IV appears to have acted as a brake on transitions from UI into mini-jobs, the broader package of Hartz reforms (particularly the mini-job reform of April 2003) may well have fostered marginal employment. My analysis also abstracts from any general equilibrium mechanisms through which benefit cuts may have altered aggregate job composition.

against a major role for some alternative mechanisms.

For this analysis, I focus on claimants who exhausted UI benefits under the old rules and hence were immediately subject to the new regime on January 1, 2005. Using the SIAB, I select UI claimants who exhausted their benefits between January 1994 and June 2004, just before Hartz IV became salient. For these "exhaustees", I observe realized long-term benefit receipt and, conditional on receipt, benefit levels net of means testing. I compare job-finding (i) between exhaustees who subsequently received long-term benefits and those who did not, and (ii) between benefit recipients subject to larger vs. smaller benefit cuts.

For each exhaustee, let $\text{LTB}_i \in \{0, 1\}$ indicate receipt of long-term benefits within 30 days of exhaustion.[33] I estimate complementary log-log models of job-finding at quarterly frequency over 1999–2006.[34] I model the instantaneous log hazard rate as

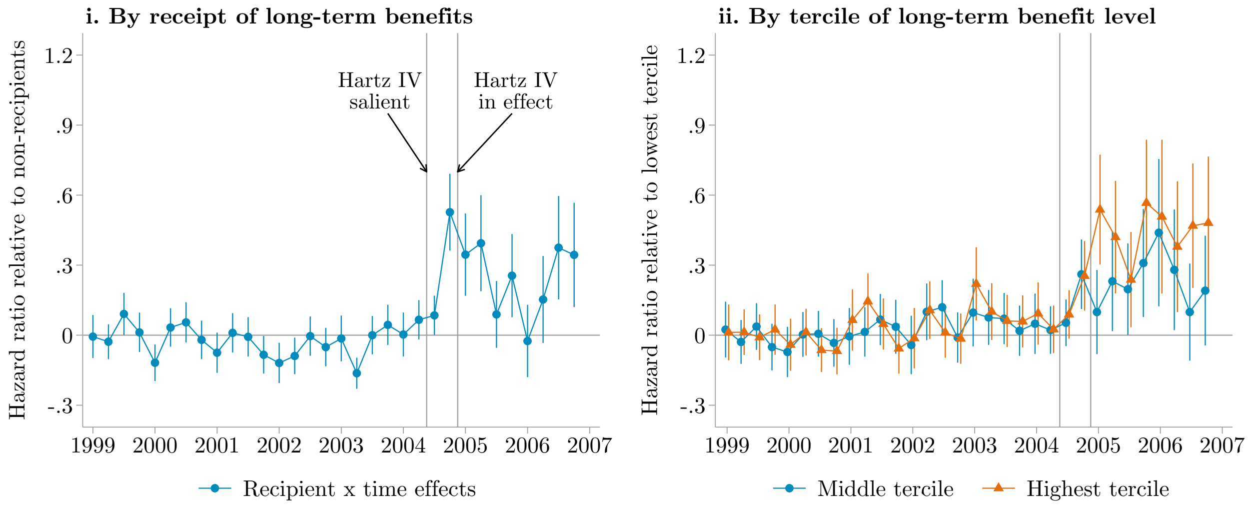
$$\mathbf{x}'_{id}\beta = \alpha_d + \gamma_t + \theta_t \text{LTB}_i + \mathbf{z}'_{id}\phi, \tag{6.1}$$

where α_d is a set of indicators for quarters since UI exhaustion, γ_t is a full set of quarter × year interactions, and $\mathbf{z}_{id}$ controls for demographic characteristics (sex, region, age, household structure, education, and nationality) interacted with duration and season effects. The coefficients of interest, θ_t, capture differences in job-finding between long-term benefit recipients and non-recipients. Exhaustees who did not receive long-term benefits under the old regime—either because they were ineligible or because they chose not to apply—are unlikely to be impacted by the Hartz IV benefit cuts. But since most of them remain registered with the unemployment office, they may still respond to changes in job-placement services or changes in cultural norms about long-term joblessness. Comparing job-finding rates between recipients and non-recipients can therefore help purge any channels through which Hartz IV

[33] Among exhaustees, 69.4 percent transition to long-term benefits within two days of exhaustion, and 72.0 percent do so within 30 days. Few exhaustees transition to long-term benefits beyond that point, suggesting that most non-receipt reflects ineligibility or non-take-up rather than application or processing delays.

[34] I start this analysis in 1999 (retaining left-censored spells) so that the earliest time effects and time × LTB_i interactions are not identified by only a small set of new exhaustees. Because the sample becomes thin at long durations and at later calendar dates, I censor incomplete spells 60 months after entry to long-term benefits or at the end of 2006, whichever comes first.

Figure 12: Relative job-finding rates among UI exhaustees by exposure to benefit cuts



Notes: Using a 2 percent sample of UI claimants who exhausted benefits between January 1994 and June 2004, I define long-term benefit recipients as those who receive such benefits within 30 days of exhaustion. I further assign recipients to terciles based on their initial long-term benefit level (net of means testing). I then estimate hazard models of job-finding at quarterly frequency over 1999–2006. The left panel plots coefficients on benefit receipt × quarter × year interactions, which show how job-finding rates among recipients evolve relative to non-recipients. The right panel plots coefficients on tercile × quarter × year interactions, which show how job-finding rates among the top two terciles evolve relative to the lowest tercile.

affected unemployed jobseekers generally, independently of lower benefit levels.

The left panel of Figure 12 plots $\hat{\theta}_t$. Until mid-2004, long-term benefit recipients and non-recipients return to work at similar rates. That changes sharply in the last quarter of 2004, when recipients exhibit a dramatic and sustained increase in relative job-finding. The timing of this response closely matches that of Hartz IV announcement and implementation.

These patterns suggest an increase in the intensity or efficacy of job search among workers drawing on long-term benefits, net of any factors common to all exhaustees regardless of subsequent benefit status. Lower benefit levels, stronger sanctions for inadequate job search, and greater stigmatization of long-term transfer receipt could all contribute to this result. By contrast, I have partialed out any changes in job-placement services available to all exhaustees, as well as any broader stigmatization of long-term joblessness per se.[35] The

[35] Under a little-noted provision of Hartz IV, local employment offices had to remit a lump-sum payment to the central government for each UI claimant who went on to claim long-term benefits. This created an institutional incentive for local agencies to spur job-finding prior to benefit exhaustion through sanctions or other means. But claimants who exhausted UI benefits before the last quarter of 2004 were exempt from this financing scheme. The evidence in Figure 12 shows that Hartz IV significantly boosted job-finding rates

persistence of these effects also weighs against the idea that media hype led UI claimants to respond proactively to cuts that proved to be modest in the end: any uncertainty about the size of the cuts was resolved early in 2005, but incumbent beneficiaries continued to find jobs at excess rates thereafter. These results also have an important policy implication: that Hartz IV not only reduced new inflows into long-term unemployment assistance, but also boosted job-finding among existing beneficiaries—perhaps its principal objective.

If benefit cuts are responsible for the effects of Hartz IV, then workers facing larger cuts should respond more strongly. Because the law replaced an earnings-indexed benefit with a uniform benefit unrelated to prior earnings, claimants with higher benefit levels under the old rules typically faced steeper cuts under the new rules. To exploit this variation, I restrict the exhaustee sample to long-term benefit recipients, then assign them to terciles $B_i \in \{1, 2, 3\}$ of long-term benefit level, stratifying by sex, region, household type, and year of exhaustion. I then estimate a variant of Equation 6.1, with the log hazard modeled as

$$\mathbf{x}'_{id}\beta = \alpha_d + \gamma_t + \sum_{b\in\{2,3\}} \theta_t^b \mathbf{1}\{B_i = b\} + \mathbf{z}'_{id}\phi. \tag{6.2}$$

Here, θ_t^2 and θ_t^3 capture the evolution of job-finding rates among middle- and top-tercile recipients, relative to bottom-tercile recipients.

The right panel of Figure 12 plots θ_t^2 and θ_t^3. Conditional on other observables, job-finding rates were quite similar across terciles from 1999 through early 2004. Beginning at the start of 2005, however, there is a clear divergence: the second and (especially) third terciles show sharp job-finding gains relative to the first. As with the contrast between recipients and non-recipients, these stark time patterns establish a tight temporal link between the post–Hartz IV shift in job-finding dynamics and concurrent changes in benefit levels. Furthermore, the fact that workers responded monotonically to the magnitude of the benefit cuts offers strong direct evidence for the benefit-cut mechanism.[36]

among claimants for whom these institutional incentives were absent.

[36] A related question is whether, among new UI claimants, groups that faced larger benefit cuts were especially responsive to Hartz IV. In Appendix Figure 9, I estimate my benchmark hazard model separately

7 Effects on Wages and Earnings

Hartz IV spurred UI claimants to find jobs faster, but how did it affect their wages upon being reemployed? Benefit cuts can reduce wages by depressing reservation wages or weakening workers' bargaining power, but they can also increase wages by shortening jobless spells that erode earnings capacity (Schmieder et al., 2016; Nekoei and Weber, 2017). Relative to identifying Hartz IV's duration effects, identifying its wage effects is further complicated by the fact that wages are only observed for claimants who become reemployed (Heckman, 1979; Ham and Lalonde, 1996). In this section, I develop a framework for estimating and decomposing Hartz IV's net effect on wages, accounting for selection into employment. I then estimate its overall effect on earnings, reflecting effects on both employment and wages.

7.1 A framework for wage effects

Following Schmieder et al. (2016), we can express a claimant's expected reemployment wage as a weighted average across possible jobless durations. Let D_i be claimant i's realized duration, let $p_{id} \equiv \Pr(D_i = d)$ be the associated pdf, and let $F_{id} \equiv \sum_{s=1}^{d} p_{is}$ be the probability that i is reemployed by month d. Next, let w_i denote the realized wage, and let $\mu_{id} \equiv \mathbb{E}(w_i \mid D_i = d)$ denote i's expected wage conditional on being reemployed in month d. Then the expected wage for a claimant reemployed within 24 months is

$$\mathbb{E}(w_i \mid \mathbf{x}_i, D_i \leq 24) = \sum_{d=1}^{24} q_{id}\mu_{id}, \tag{7.1}$$

where $\mathbf{x}_i$ is a vector of observables and $q_{id} \equiv \Pr(D_i = d \mid D_i \leq 24) = \frac{p_{id}}{F_{i,24}}$ is the conditional pdf of jobless duration. Iterating expectations, the mean wage among all claimants

for 36 cells defined by sex, region, household type, and UI benefit level. Across cells, the change in job-finding in the month Hartz IV binds is negatively correlated with both simulated changes in long-term cash benefits ($\hat{\rho} = -.33$) and simulated changes in net household income ($\hat{\rho} = -.31$).

reemployed within 24 months is

$$\mathbb{E}(w_i \mid D_i \leq 24) = \sum_{i=1}^{N} \pi_i \sum_{d=1}^{24} q_{id}\mu_{id}, \tag{7.2}$$

where $\pi_i \equiv \frac{F_{i,24}}{\sum_{j=1}^{N} F_{j,24}}$ is claimant i's (probabilistic) share of the reemployed pool. Labeling counterfactual values "*cf*", the effect of Hartz IV on the mean observed wage is

$$\Delta\mathbb{E}(w_i \mid D_i \leq 24) \equiv \sum_{i=1}^{N} \pi_i \sum_{d=1}^{24} q_{id}\mu_{id} - \sum_{i=1}^{N} \pi_i^{cf} \sum_{d=1}^{24} q_{id}^{cf}\mu_{id}^{cf} \tag{7.3}$$

There are two problems with interpreting Equation 7.3 as the causal effect of Hartz IV on individual claimants' reemployment wages. The first is a compositional bias: Hartz IV changes the set of claimants who are reemployed within 24 months, so that individuals are given different weights in the two terms above ($\pi_i \neq \pi_i^{cf}$). The second is a more subtle form of selection that would arise even if claimants were identical. Because Hartz IV shifted the distribution of jobless durations to the left, we observe wage realizations for a set of spells that would otherwise have gone unobserved. Since these are likely to be longer spells associated with less wage recovery, including them without an appropriate adjustment would likely impart a negative bias to the estimated wage effect.

In Appendix D.3, I develop a selection correction that addresses both of these issues. The basic idea is to define a *truncated* distribution of jobless durations that traces out each claimant's reemployment trajectory under Hartz IV, then discards the "excess" probability mass that would have been allocated past the censoring horizon in the absence of Hartz IV. Employing this correction, I estimate the quantity

$$\Delta\widetilde{\mathbb{E}}(w_i \mid D_i \leq 24) \equiv \sum_{i=1}^{N} \pi_i^{cf} \left(\sum_{d=1}^{24} \widetilde{q}_{id}\mu_{id} - \sum_{d=1}^{24} q_{id}^{cf}\mu_{id}^{cf} \right) \tag{7.4}$$

where $\widetilde{q}_{id}$ is the conditional pdf of jobless durations under Hartz IV using the truncated distribution. This expression can be interpreted as the wage effect of Hartz IV in cases where a claimant would have been reemployed even if Hartz IV had not been enacted.

Let $\bar{q}_{id} \equiv \frac{1}{2}(\widetilde{q}_{id} + q_{id}^{cf})$ and $\bar{\mu}_{id} \equiv \frac{1}{2}(\mu_{id} + \mu_{id}^{cf})$. I decompose the net wage effect as

$$\Delta\widetilde{\mathbb{E}}(w_i \mid D_i \leq 24) = \underbrace{\sum_{i=1}^{N} \pi_i^{cf} \sum_{d=1}^{24} \bar{q}_{id}(\mu_{id} - \mu_{id}^{cf})}_{\text{selectivity effect}} + \underbrace{\sum_{i=1}^{N} \pi_i^{cf} \sum_{d=1}^{24} (\widetilde{q}_{id} - q_{id}^{cf})\bar{\mu}_{id}}_{\text{time-out-of-work effect}} \tag{7.5}$$

Holding jobless durations fixed, a benefit cut may reduce reemployment wages by depressing mean accepted wages μ_{id} at each possible duration (which I call the *selectivity effect*). Holding μ_{id} fixed, a benefit cut may increase wages by shifting claimants to shorter durations associated with higher wages (which I call the *time-out-of-work effect*).

7.2 Wages conditional on jobless duration

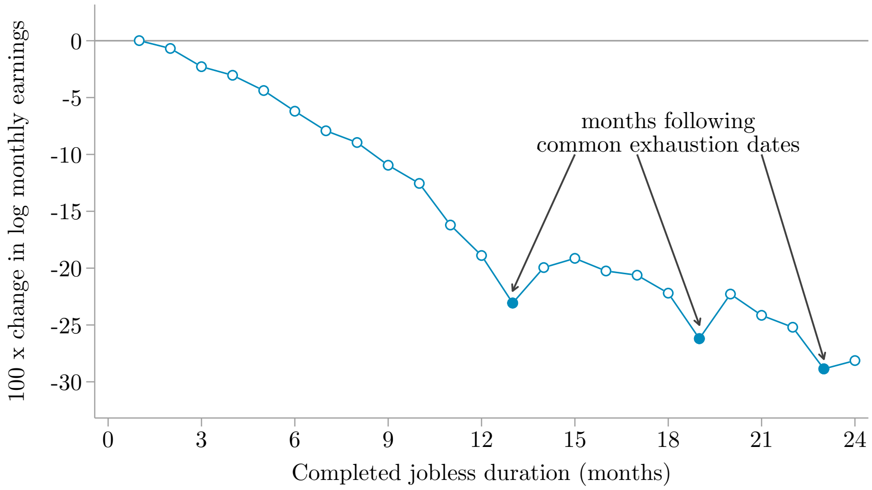
To implement this framework, the first step is to estimate average wages conditional on jobless duration, both under Hartz IV and under a counterfactual in which the benefit cuts do not occur. To motivate this analysis, Figure 13 plots the mean difference in log wages before and after a jobless spell as a function of completed duration. Longer spells are associated with much steeper wage declines, which may reflect both causal effects of nonemployment and compositional differences between claimants reemployed at different durations. In addition, the degree of wage recovery drops sharply just after common UI exhaustion points. Controlling for a rich set of observables, I show that Hartz IV has contributed to this pattern by deepening the post-exhaustion dip in accepted wages.

Let w_{id} denote the log ratio of reemployment wages to pre-UI wages for a claimant reemployed d months after entering UI. Using the same explanatory variables as in the hazard model of Equation 4.4, I regress w_{id} on functions of benefit status at the time of hire:

$$w_{id} = \alpha_d + \gamma_t + \mathbf{z}'_{id}\phi + \sum_{k=-9}^{4} \delta_k^E \mathbf{1}\{\tau_{id}^E = k\} + \sum_{k=-9}^{4} \delta_k^H \mathbf{1}\{\tau_{id}^H = k\} + \varepsilon_{id} \tag{7.6}$$

The logic of this regression is to compare wage recovery among similar claimants who took equally long to find new jobs, and did so concurrently, but who were differentially exposed to Hartz IV at the time they found work. The event-time coefficients δ_k^E and δ_k^H capture how

Figure 13: Pre-/post-UI wage changes as a function of completed jobless duration



Notes: Mean pre-/post-UI wage changes experienced by UI claimants reemployed within 24 months of entering UI. Each claimant's wage change is defined as the difference in log wages between the job held prior to UI entry and the first regular job obtained thereafter.

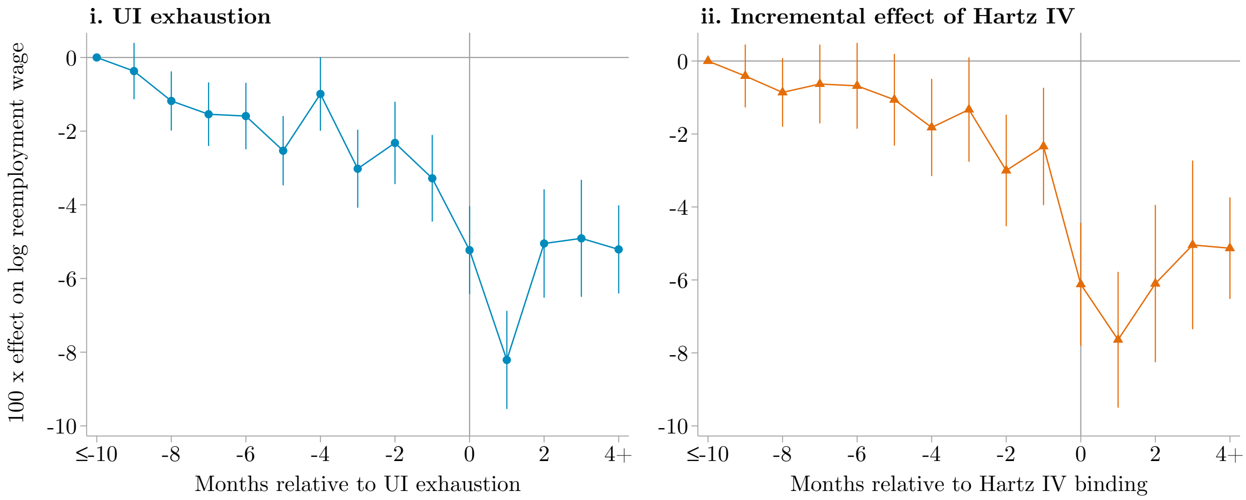
reemployment wages evolve in a window around each benefit drop. The controls in $\mathbf{z}_{id}$ and γ_t allow wage recovery to vary flexibly across demographic groups, along the distribution of prior wages, and over time, while the dummies α_d absorb duration dependence due to either causal effects of joblessness or selection on unobservables.

Figure 14 plots estimated changes in reemployment wages as claimants approach the two step-downs in the benefit schedule. As shown in the left panel, reemployment wages decline gradually in the lead-up to UI exhaustion, then drop sharply once benefits run out. All else equal, claimants who accept jobs in the month after exhaustion, when the effect is largest, receive wages 7.9 percent lower than those reemployed 10+ months before exhaustion. These negative effects—identified by cross-sectional variation in potential benefit durations among claimants with similar observables and the same completed jobless duration—suggest that reservation wages fall as workers approach benefit drops.[37]

The right panel shows the *incremental* effect of Hartz IV. Wages start to decline

[37] An alternative explanation relates to bargaining: even if workers' reservation wages do not change, employers may offer lower wages if they can identify which job applicants are on the verge of benefit drops. Given the complexity of the benefit calculation—which requires precise information on the exact timing of prior work and UI spells—this story would place a heavy information burden on potential employers.

Figure 14: Benchmark effects of benefit drops on log reemployment wages



Notes: Effects of UI exhaustion and Hartz IV on the log ratio of wages in the first regular job after UI to wages in the job that preceded entry to UI, from the regression specified in Equation 7.6.

about six months before the new rules bind, with effects peaking at 7.4 percent for claimants taking jobs in the month after the benefit cut. These falling wages—which suggest that claimants become less choosy about job offers as their outside options deteriorate—augment the "drop at exhaustion" already evident in wage offers accepted prior to the reform.[38] I estimate that, under the new UI schedule, jobs accepted just after benefit exhaustion pay 14.7 percent lower wages than jobs accepted 10+ months before benefits run out.

Robustness. Appendix Figure 11 explores the robustness of these estimates to control strategies analogous to those used in the earlier hazard analysis. Adding additional controls—such as cohort dummies, interactions between the age/experience effects and an indicator for the post-reform period, and duration × calendar time interactions—attenuates the effects by up to about one-half, but I find negative impacts on accepted wages in all specifications.

A potential concern is that these wage estimates represent dynamic selection—that is, treatment-induced changes in the types of workers who are reemployed at each duration—

[38] As further evidence that jobseekers exposed to Hartz IV are less selective about job offers, Appendix Figure 10 shows that Hartz IV increased the hazard rate of recall to a previous employer, as well as transitions to new employers. Insofar as recall is "a process not requiring search" (Katz and Meyer, 1990b)—because workers are contacted by their former employer and simply exercise or decline the option to return—the increase in recalls confirms a reduction in workers' perceived continuation value of remaining unemployed.

rather than causal effects of Hartz IV on individual wages. While Equation 7.6 controls for several forms of selection on observables, claimants who find jobs in the face of Hartz IV might exhibit wage dynamics that are *unobservably* different from those of other claimants. In Appendix F, I use repeat spells to show that allowing for unobserved heterogeneity in wage recovery does not qualitatively alter my estimates.

7.3 Net effects on wages

Consistent with search theory, Hartz IV caused claimants to find jobs more quickly but to accept lower wages conditional on completed duration. To calculate Hartz IV's net effect on wages, I combine fitted values from the hazard and wage equations to predict $\widehat{\lambda}_{id}$, $\widehat{\mu}_{id}$, and counterfactuals that set the Hartz IV event-time variable τ_{id}^H to its omitted value.[39] Using the identity $p_{id} \equiv \lambda_{id} \prod_{s=1}^{d-1}(1-\lambda_{is})$, I then compute each term in Equation 7.5.

Table 1 reports estimates of the overall, selectivity, and time-out-of-work effects. In column 1, which uses my benchmark hazard and wage models, I estimate that exposure to Hartz IV reduced the mean reemployment wage by 1.0 percent. This modest negative impact masks offsetting selectivity and time-out-of-work effects: holding jobless durations fixed at their counterfactual distribution, the downward shift in the path of accepted wages accounts for a 2.0 percent reduction in the mean reemployment wage, but the shift towards shorter jobless spells yields a countervailing 1.0 percent wage gain. At the 95 percent level, I am able to rule out a net negative wage impact exceeding -1.5 percent.[40]

Column 2 presents estimates from a more flexible specification that adds interactions between 3-month duration bins and quarter $\times$ year time dummies to both the hazard and wage equations. Doing so yields a slight *positive* impact, as the additional controls aug-

[39] I estimate the hazard and wage equations jointly by maximum likelihood (Caliendo et al., 2013). I specify the reemployment hazard λ_{id} as in the benchmark specification of Section 5.1. I specify the log wage as $w_{id} = \mu_{id} + \varepsilon_{id}$, where μ_{id} is the fitted value of wages in Equation 7.6 and where I assume $\varepsilon_{id} \sim N(0, \sigma_\varepsilon^2)$.

[40] The selectivity effect does not depend on $\widetilde{q}_{id}$, the selection-corrected conditional pdf. If the time-out-of-work effect is weakly positive (as both theory and my estimates suggest), the selectivity effect itself provides a lower bound on the net wage impact that does not rely on my selection correction. This more conservative lower bound rules out negative wage impacts larger than -2.5 percent at the 95 percent level.

Table 1: Implied effects of UI reform on mean log reemployment wages

	Benchmark specification (1)	**Time-varying baseline hazard** (2)
Overall wage effect	−1.04	1.13
	(0.26)	(0.32)
Selectivity effect	−2.00	−0.14
	(0.25)	(0.29)
Time-out-of-work effect	0.96	1.26
	(0.08)	(0.10)

Notes: Effects of Hartz IV on reemployment wages for claimants entering UI in 2005 (in 100 × log points), as defined in Equations 7.4 and 7.5. Column (1) is based on my benchmark duration and wage specifications. Column (2) is based on specifications that add interactions between duration and calendar time.

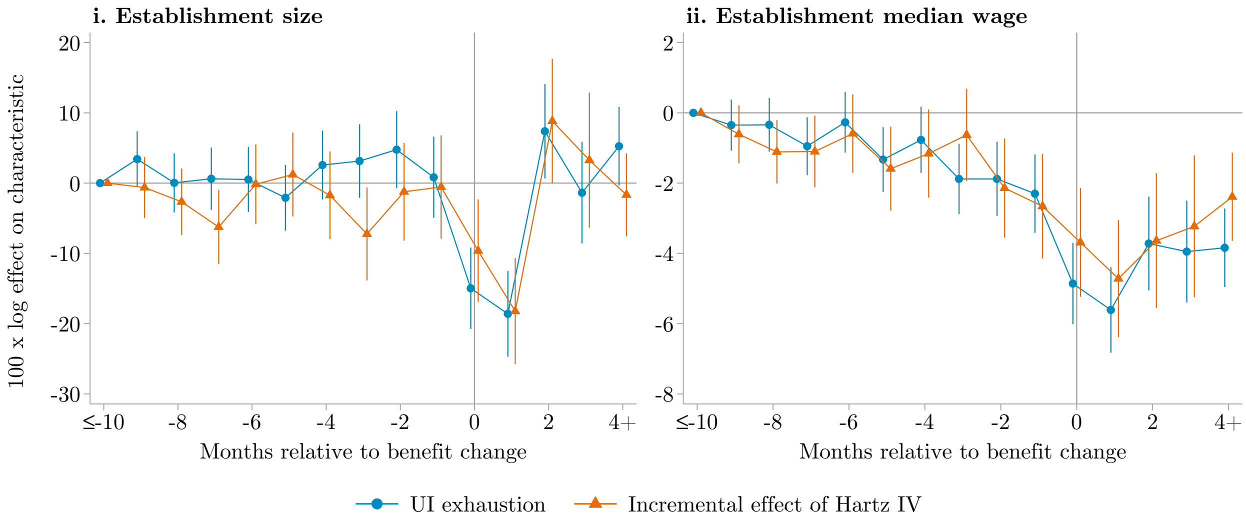
ment the hazard effects (Figure 6) while attenuating the conditional wage effects (Appendix Figure 11). In this specification, I can rule out a net wage gain greater than 1.8 percent.

While the sign of the overall wage effect varies across specifications, it is consistently small in magnitude. My analysis suggests that the substantial employment gains found in Section 5 are not accompanied by any substantial degradation in initial wages. In addition, the finding in Section 5.4 that Hartz IV did not affect the part-time share of (regular) employment suggests that the relative constancy of wages—defined as daily earnings—is unlikely to reflect offsetting shifts in hourly wages and hours worked. Though I cannot rule out shifts in hours *within* part-time and full-time work, the available evidence suggests that my wage estimates do not mask substantial intensive-margin changes in labor supply.

7.4 Effects on establishment characteristics

The Hartz reforms were enacted at a time of rising wage inequality (Dustmann et al., 2009), driven in part by a proliferation of low-wage employers and increased assortative matching of less-skilled workers to lower-paying establishments (Card et al., 2013). To see whether Hartz IV consigned UI claimants to jobs at lower-quality employers, Figure 15 plots changes in the size and average wages of hiring establishments as claimants approach benefit reduc-

Figure 15: Effects of benefit drops on establishment characteristics



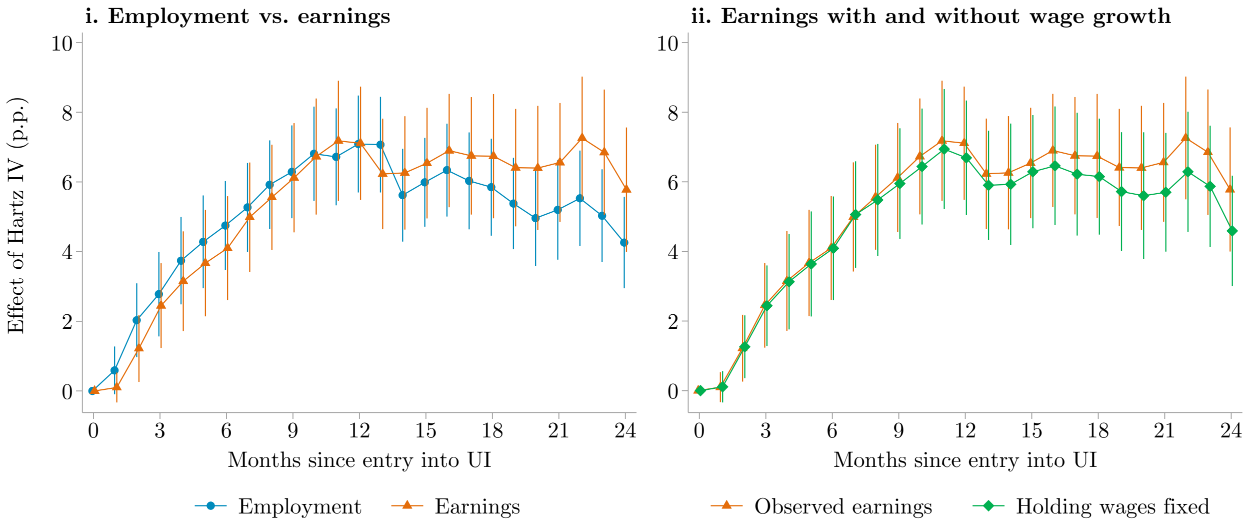
Notes: Effects of UI exhaustion and Hartz IV on log establishment size (the number of regular workers) and log establishment median wage (among full-time workers), conditional on completed jobless duration, from the regression specified in Equation 7.6..

tions. Claimants who accept jobs at the point of UI exhaustion enter smaller, lower-paying establishments, and Hartz IV reinforced that pattern. Accounting for the effects of reduced time out of work, as in Section 7.3, I find that claimants exposed to Hartz IV are reemployed at establishments that are 3.4 to 3.8 percent smaller and pay 0.6 to 1.1 percent lower wages, across specifications with or without calendar time × duration interactions.

7.5 Earnings trajectories

To close out this section, I estimate the effect of Hartz IV on claimants' earnings, reflecting the combined effects on employment rates and wages. In the left panel of Figure 16, the employment series (in blue) replicates the effect of Hartz IV on the probability a claimant is currently employed, as estimated in Section 5.3 using the regression approach described in Section 4.3. The earnings series (in orange) estimates the same specification using monthly earnings, defined as the daily wage times days worked (and including claimants with zero days worked). Hartz IV's positive effects on employment and earnings track each other quite closely at shorter durations, consistent with my findings in Section 7.3 that Hartz IV had at

Figure 16: Effects of Hartz IV on earnings



Notes: In the left panel, the "employment" series reproduces the "currently employed" series from Figure 9, while the "earnings" series shows the effect of Hartz IV on monthly earnings—daily wage × days worked, divided by earnings prior to UI entry—using the regression specified in Equation 4.7. The right panel uses a counterfactual earnings measure that holds daily wages fixed from the moment of reemployment onward.

most a small effect on a claimant's first reemployment wage. At longer durations, however, the earnings effects are modestly larger (though the confidence intervals overlap), suggesting that claimants exposed to Hartz IV may experience faster growth in daily wages.

To probe this idea further, the right panel introduces a counterfactual earnings series that shuts off post-reemployment wage dynamics by holding a claimant's daily wage fixed at the point when the claimant was first hired. The observed earnings effects are slightly (albeit insignificantly) larger than the effects on counterfactual earnings, suggesting that a positive effect on wage growth may contribute to Hartz IV's effect on earnings.

8 Implications

As a large-scale, nationally implemented reform to the social safety net, with no legacy protections for incumbent beneficiaries, Hartz IV may have had significant effects on both Germany's macroeconomic performance and social welfare. I consider these in turn.

8.1 Macroeconomic implications

Once derided as the "sick man of Europe" (Dustmann et al., 2014), the German economy embarked on a remarkable turnaround soon after Hartz IV took effect: between December 2004 and December 2014, the unemployment rate fell by 6.1 p.p. To what extent did Hartz IV contribute to this dramatic decline?

My research design identifies partial equilibrium effects of benefit cuts on job-finding among individual UI recipients. In general equilibrium, these direct effects may be either mitigated by crowdout effects or amplified by job creation effects. Although a full reckoning of Hartz IV's aggregate impact is beyond the scope of this paper, I now use my microeconomic estimates to calibrate the partial equilibrium impact of Hartz IV on Germany's unemployment rate. I then review the recent literature on general equilibrium effects.

Partial equilibrium estimates. As detailed in Appendix G, I construct a panel dataset of all individuals observed in the SIAB data anytime during 1995–2010, then track their labor market status during 2001–2003, just before Hartz IV was enacted. While the rest of the paper focuses on prime-age individuals, here I retain all individuals aged 16–64 so as to obtain estimates representative of the overall labor market. To align employment concepts with household surveys as closely as possible, I code individuals as employed if they hold either a socially insured job or a marginal job; as unemployed if they are registered as such with the Federal Employment Agency; and as non-participants otherwise, including periods when an individual is absent from the SIAB data. Since Hartz IV influences job-finding rates both during UI receipt and after UI exhaustion, I classify jobless individuals as UI recipients or non-recipients based on whether they receive UI benefits *at any point in their jobless spell.*

Next, I calculate monthly "pre–Hartz IV" hazard rates of transition between seven labor market states: employment, short-term unemployment, long-term unemployment, and non-participation, with each jobless state partitioned between UI recipients and non-recipients. Using my benchmark estimates of the effects of Hartz IV on job-finding rates, I then estimate "post–Hartz IV" hazard rates by boosting job-finding among short-term and long-term unemployed UI recipients. Finally, I use the estimated hazard rates to compute the steady-state unemployment rate with the effects of Hartz IV turned either on or off.

I find that increased job-finding due to Hartz IV reduced Germany's unemployment rate by 0.7 p.p.—a modest, but meaningful contribution to its "employment miracle".[41][42] Importantly, over half of this effect comes from a 0.4 p.p. reduction in the *long-term* unemployment rate. This finding underscores the point that long-term benefit generosity is especially relevant for prolonged jobless spells, and it echoes the Ljungqvist and Sargent (1998, 2008) hypothesis that generous UI benefits may contribute to persistently high levels of long-term unemployment.

General equilibrium considerations Long-term benefit cuts can affect the unemployment rate not only through direct effects on search behavior among UI claimants, but also through indirect effects on labor market tightness and aggregate demand (Landais et al., 2018). On the one hand, if labor demand is not perfectly elastic, then increased search effort among claimants will partly crowd out job opportunities for other workers, putting upward pressure on the unemployment rate through a *rat-race effect* (Lalive et al., 2015; Marinescu, 2017). In addition, unemployed workers might cut back on consumption, again putting upward pressure through an *aggregate demand effect* (Kekre, 2023). On the other hand, since benefit cuts weaken claimants' outside options in wage negotiations, they may incentivize firms to post more vacancies, putting downward pressure on the unemployment rate through a *job creation effect* (Hagedorn et al., 2019). Because these indirect effects have opposite signs, the general equilibrium effect of Hartz IV could be either larger or smaller than the

[41] Insofar as workers can influence the probability of layoff (for example, by choosing how much effort to exert on the job), benefit cuts may discourage some separations by increasing the cost of becoming unemployed. Hartung et al. (2025) argue that Hartz IV reduced Germany's unemployment rate primarily by reducing separation rates, with increased job-finding making a smaller contribution.

[42] Soon after Hartz IV, a companion measure reduced the maximal duration of UI benefits for older workers entering UI after February 1, 2006. Cuts to UI benefit durations and cuts to long-term benefit levels are likely to have complementary effects, as the former cause the latter to bind earlier in a worker's spell. Such an interaction would magnify the implied partial equilibrium impact of Hartz IV.

partial equilibrium effect calculated above.

Quasi-experimental analyses of the macroeconomic effects of UI policy typically exploit policy variation across local labor markets (e.g., Lalive et al., 2015; Marinescu, 2017; Chodorow-Reich et al., 2018; Hagedorn et al., 2019; Boone et al., 2021). Unfortunately, because Hartz IV was implemented uniformly and simultaneously throughout Germany, it is poorly suited to that approach. Instead, numerous papers have attempted to calibrate the effect of Hartz IV through structural models and calibration exercises. These papers reach widely disparate conclusions about the effect of Hartz IV on Germany's steady-state unemployment rate, with estimates ranging from essentially zero (Launov and Wälde, 2013, 2016; Bradley and Kügler, 2019) to reductions exceeding 1.0 p.p. (Krebs and Scheffel, 2013) or even 2.0 p.p. (Krause and Uhlig, 2012; Hochmuth et al., 2019; Hartung et al., 2025). Alongside major differences in model specification, this lack of consensus reflects different assumptions about (i) the size of the benefit cuts, (ii) the micro-elasticity of job-finding to benefit levels, and (iii) the effects of the earlier Hartz I–III reforms. Given the uncertainty about these parameters, as well as the many degrees of freedom in model specification, it is difficult to adjudicate among these conflicting studies.

Given the difficulty of estimating general equilibrium effects in the Hartz IV context itself, it is useful to consider well-identified estimates of the effects of other changes in UI generosity. Recent quasi-experimental studies have tended to find that the "macro elasticity" of unemployment to benefit generosity is smaller than the "micro elasticity" (Lalive et al., 2015; Marinescu, 2017)—or that large changes in UI generosity have small aggregate effects (Chodorow-Reich et al., 2018; Boone et al., 2021), suggesting the same result—though Hagedorn et al. (2019) offer an opposing view. If indirect effects do tend to offset the direct effect, then my estimate that Hartz IV reduced the German unemployment rate by 0.7 p.p. is likely to be an upper bound on the true effect. My results therefore fall towards the lower end of existing macroeconomic estimates of the effects of Hartz IV.

8.2 Welfare implications

While empirical analyses of long-term benefits are relatively scarce, a larger theoretical literature has explored the optimal timing of UI benefits as a function of unemployment duration (e.g., Shavell and Weiss, 1979; Hopenhayn and Nicolini, 1997; Shimer and Werning, 2008). These papers come to differing conclusions about whether benefits should be flat, rising, or falling with duration, depending on assumptions about which policy instruments are available, whether workers can borrow and save, and whether job prospects are duration-dependent. Of particular relevance, Kolsrud et al. (2018) develop a sufficient-statistics approach for evaluating the optimality of an observed benefit schedule given exogenous variation in benefit levels at both shorter and longer jobless durations.[43]

Although Hartz IV may appear to be a promising setting for implementing the Kolsrud et al. approach, institutional and data limitations preclude such an analysis. There are three main difficulties. First, Kolsrud et al.'s methodology requires estimates of the elasticity of jobless duration with respect to changes in benefit levels at different durations. As detailed in Appendix A, however, there is a great deal of uncertainty about how Hartz IV affected long-term benefit levels and household incomes, and different assumptions about benefit eligibility would imply a wide range of duration elasticities. Second, even if an appropriate elasticity could be calculated for long-term benefits, Hartz IV does not provide the variation needed to identify a comparable elasticity for the *initial* UI benefit level.[44] Third, as discussed in Section 6, Hartz IV may have operated partly through other mechanisms that amplified the effect of the benefit cuts on claimants' responsiveness to benefit exhaustion. The literature on optimal UI would benefit from additional estimates of the welfare effects of changing long-term benefit levels in settings where these limitations do not arise.

[43] Exploiting Swedish policy reforms in 2001–2002, Kolsrud et al. (2018) find that UI benefits have smaller moral hazard costs and larger consumption smoothing benefits at longer durations. Their estimates imply local welfare gains from simultaneously lowering short-term benefits and raising long-term benefits.

[44] Kolsrud et al.'s approach also requires estimates of consumption drops at shorter and longer durations. Consumption is not observed in the IAB data, nor do the data contain the necessary information about assets and income to back out a residual measure of consumption.

9 Conclusion

Hartz IV was a seminal reform of the German safety net, and its effect on the German labor market has been hotly debated. Overcoming the many identification challenges that have hampered previous research, I provide the first causal estimates of Hartz IV's effects on job-finding and wages among unemployed workers. I show that Hartz IV led to significant reductions in jobless durations with no clear evidence of a deleterious effect on wages. The partial equilibrium effect of Hartz IV on job-finding—if not augmented or offset in general equilibrium—may have lowered Germany's steady state unemployment rate by 0.7 p.p., contributing to the "employment miracle" of the late 2000s (Burda and Seele, 2020).

I also contribute to the sparse emerging literature about the labor market effects of long-term unemployment assistance for workers who have exhausted their initial stream of unemployment insurance benefits. Long-term benefits insure workers against the risk of experiencing long or permanent jobless spells, which erode job prospects, deplete savings, and impose fiscal externalities throughout the tax and transfer system (Kroft et al., 2013; Ganong and Noel, 2019; Nekoei and Weber, 2017). Long-term benefits also take on added importance during recessions, when more workers exhaust their initial entitlements (Schmieder et al., 2012), and—through moral hazard effects—may increase the prevalence of long-term unemployment. Complementing previous studies that analyze more modest changes in the benefit schedule, my findings shed new light on what is arguably the most significant overhaul of long-term unemployment assistance in recent decades.

Focusing too heavily on Hartz IV's steady-state effects would overlook the population it most immediately affected: the 2.3 million workers receiving long-term benefits on the eve of the reform, many of whom had relied for years on Germany's unusually generous UI system. The German safety net was historically protected by a "reform bottleneck" that stymied efforts to bring long-term replacement rates in line with international practice (Jacobi and Kluve, 2007; Tompson, 2009). Given political gridlock, displaced workers—especially older workers—who entered UI under the pre-reform regime might reasonably have expected to claim generous, earnings-indexed benefits until aging into retirement, effectively becoming labor market participants in name only. Hartz IV presented such claimants with a stark choice: either accept a lower consumption stream or return to the workforce after a long hiatus, under the shadow of scarring effects or skill depreciation. A closer look at how benefit cuts impacted these long-standing beneficiaries may yield fresh insights about how extended periods out of work affect human capital and earnings potential.

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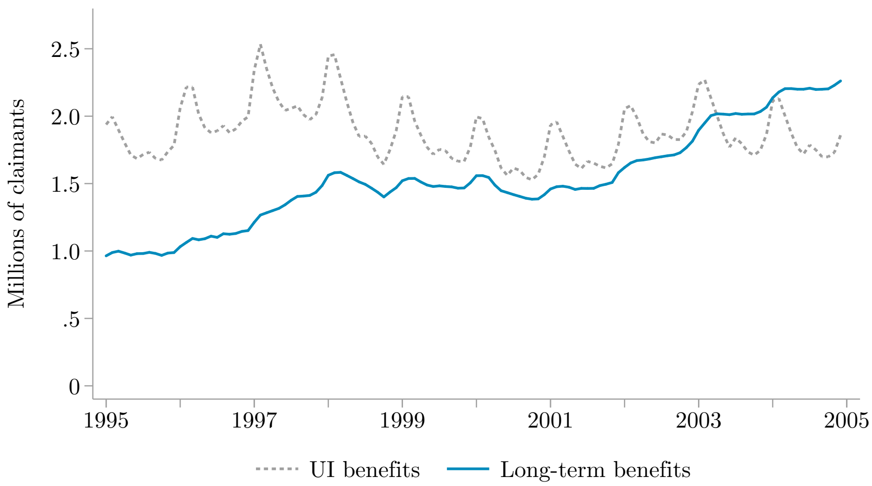
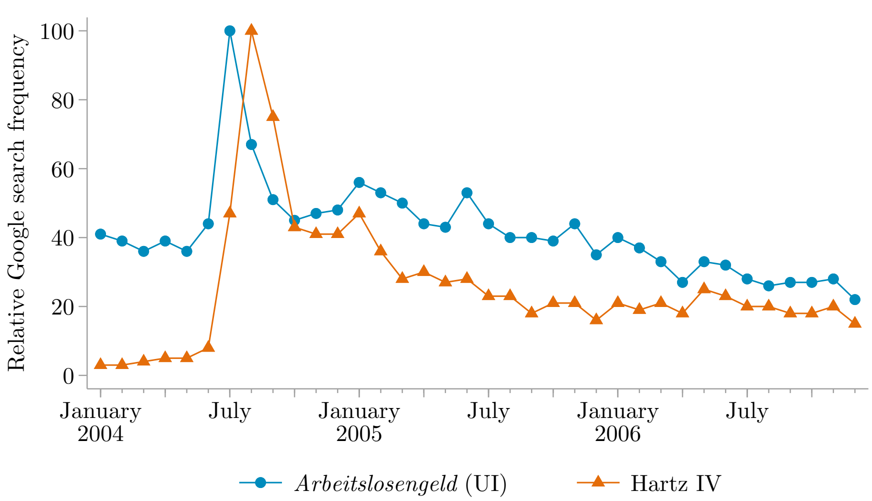
Additional Figures and Tables

Appendix Figure 1: The rise of Germany's benefit caseload in the lead-up to Hartz IV



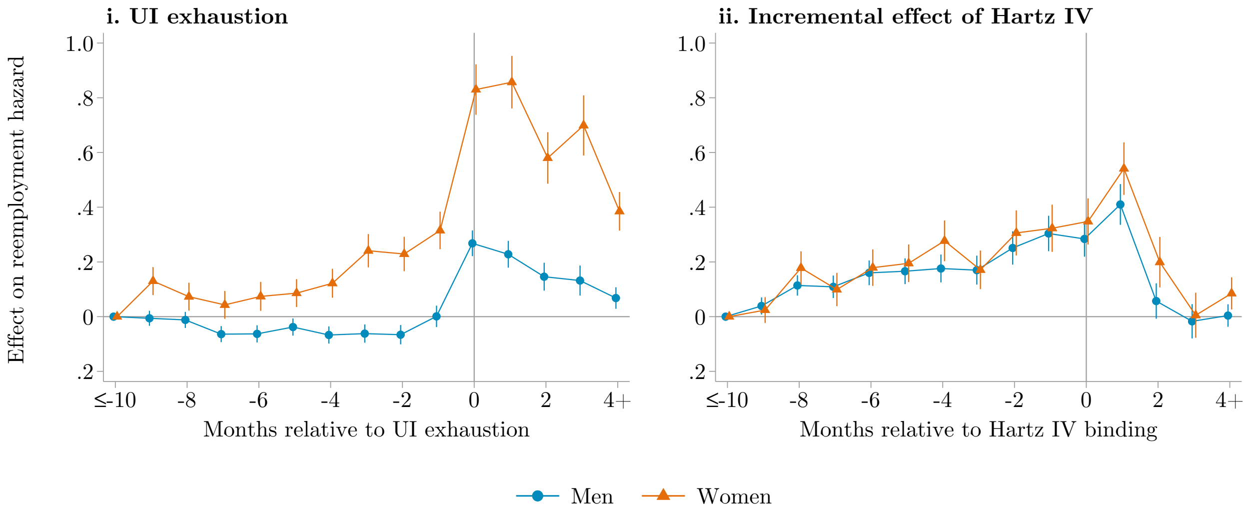
Notes: Monthly aggregate *Arbeitslosengeld* and *Arbeitslosenhilfe* caseloads published by the Federal Employment Agency. Hartz IV consolidated long-term unemployment assistance and social assistance into a single income-support program, precluding comparisons between the caseloads before and after January 2005.

Appendix Figure 2: The summer 2004 spike in UI-related Google searches



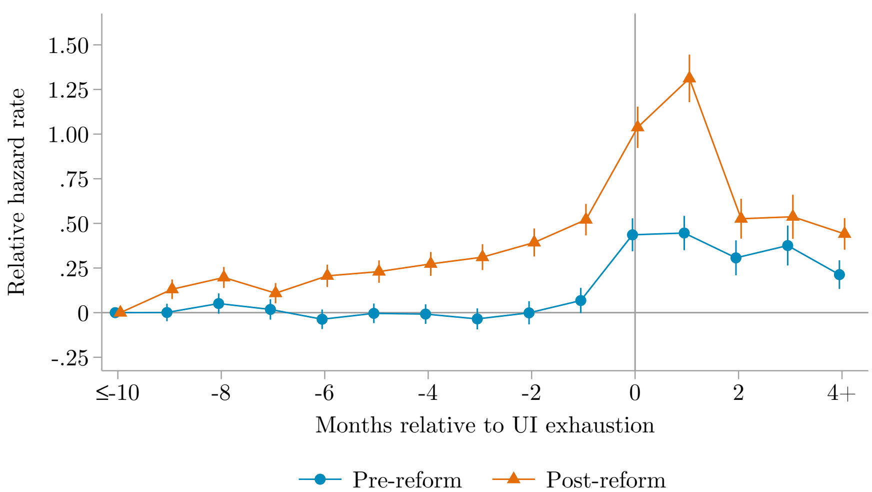
Notes: Monthly Google searches for the terms "*Arbeitslosengeld*" (German for "unemployment insurance") and "Hartz IV" originating in Germany and normalized to 100 in the peak month during the period shown.

Appendix Figure 3: Benchmark effects of benefit drops, estimated separately by sex



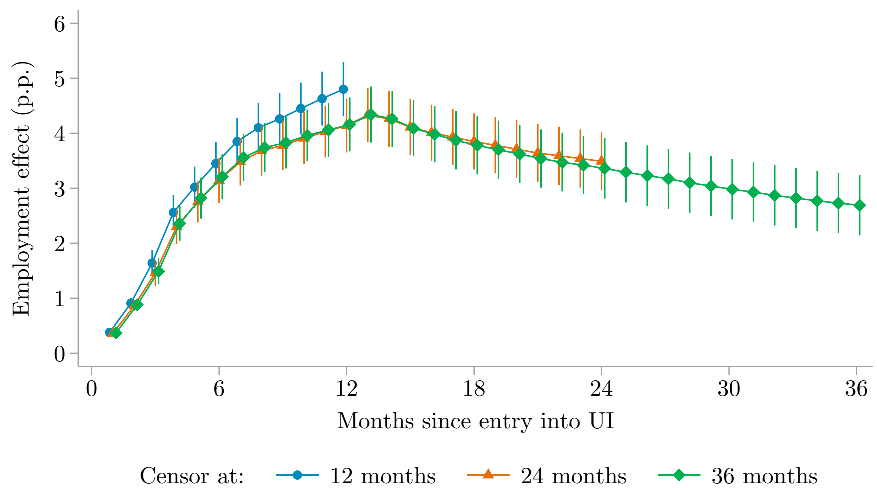
Notes: Proportional effects of benefit drops on transitions to employment using the hazard specification in Equations 4.4 and 4.5, estimated separately by sex. See notes to Figure 4.

Appendix Figure 4: Sensitivity to UI exhaustion for pre- vs. post-reform cohorts



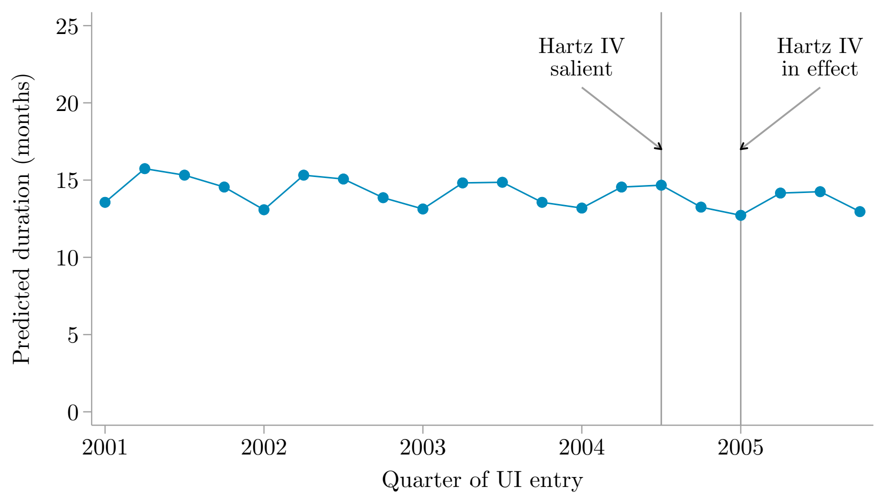
Notes: Proportional effects of UI exhaustion on transitions to employment using the hazard specification of Equation 5.1. Each series plots normalized hazard ratios corresponding to UI exhaustion for claimants entering UI in either 2001 (pre-reform) or 2005 (post-reform).

Appendix Figure 5: Effect of Hartz IV on the probability a claimant has ever been reemployed (alternative censoring horizons)



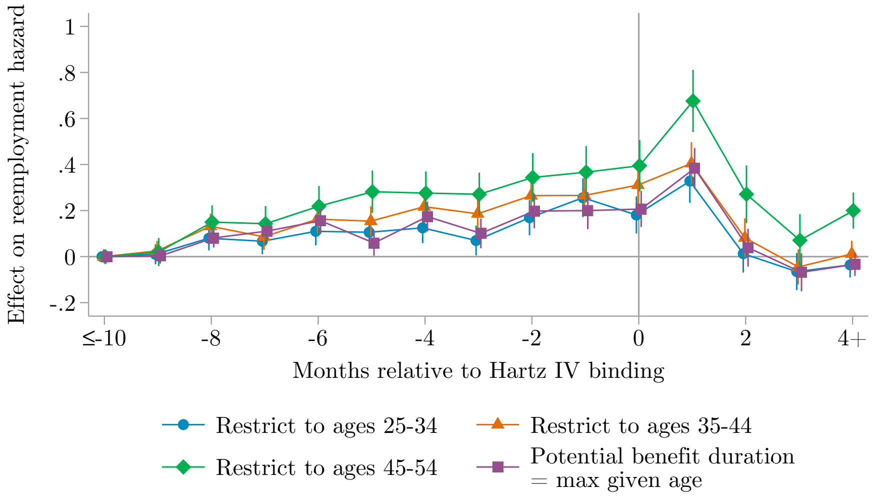
Notes: Reemployment effects of Hartz IV for the 2005 cohort of UI entrants, obtained by reestimating the benchmark hazard specification with incomplete spells censored at either 1 year, 2 years, or 3 years.

Appendix Figure 6: Predicted jobless duration by quarter of UI entry



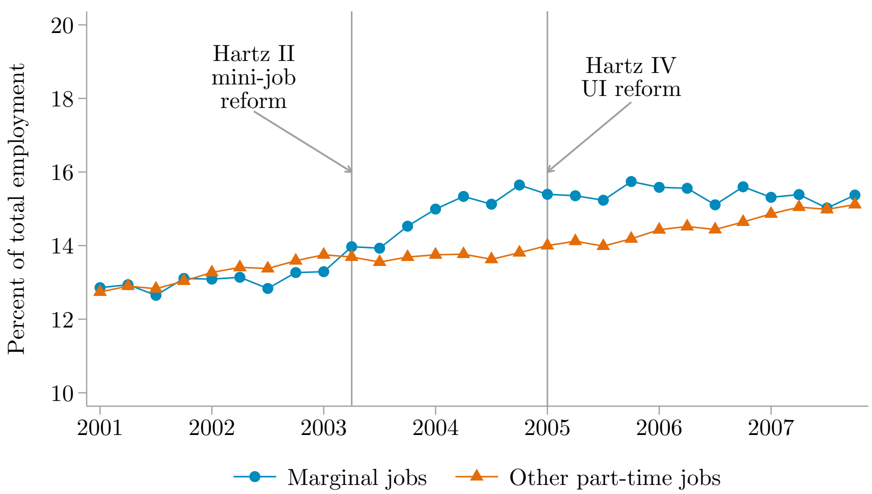
Notes: Mean predicted jobless durations for claimants entering UI in each quarter, using fitted values from a Weibull model of the job-finding hazard among UI claims that began in 2001. I estimate the model separately by sex × East/West residence, then aggregate up based on the sex and regional composition of each entry cohort. The explanatory variables are seven age bins, seven experience bins, three household types, three education groups, German nationality, and quintiles of prior wage. I also control for quarter × year effects but, when forming predictions, set these interactions to the reference category. By construction, temporal variation in each series reflects only compositional changes in the characteristics of new claimants.

Appendix Figure 7: Isolating two sources of variation in potential benefit duration



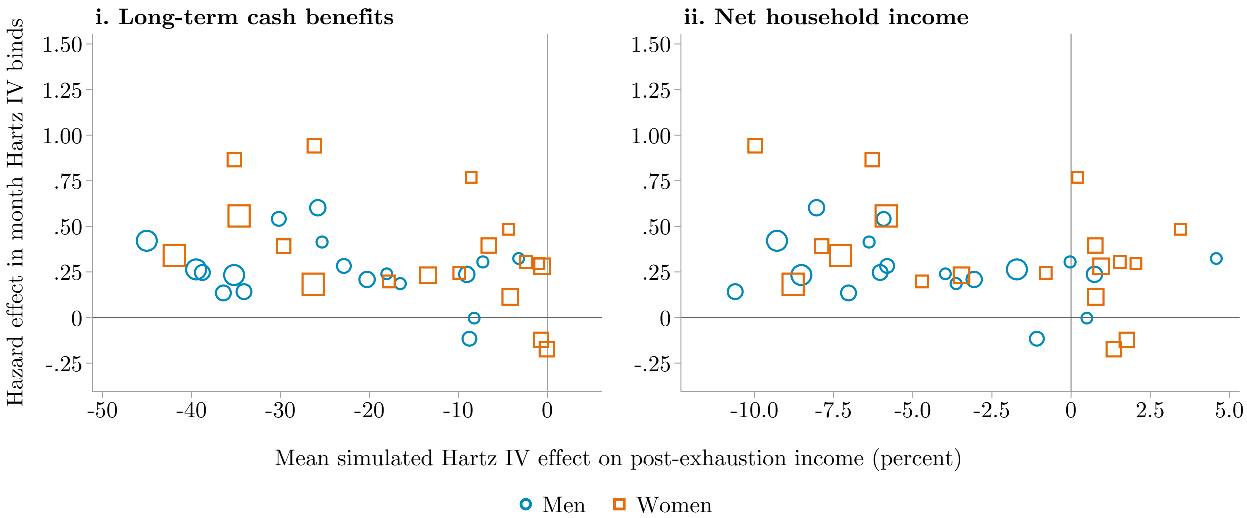
Notes: Proportional effects of Hartz IV on the hazard rate of reemployment, isolating variation in potential benefit duration stemming from either age or experience. Because all workers under 45 are subject to the same age-determined duration ceiling, variation in potential duration within the 25–34 (blue) and 35–44 (orange) age groups is driven solely by differences in time spent employed in the 7 years preceding UI entry. The purple series instead restricts attention to claimants with the maximum possible benefit duration given their age, so that the variation in duration is driven only by age. For completeness, I also include estimated effects for ages 45–54 (green series), a group for whom benefit duration varies for both reasons.

Appendix Figure 8: Aggregate part-time marginal and non-marginal employment



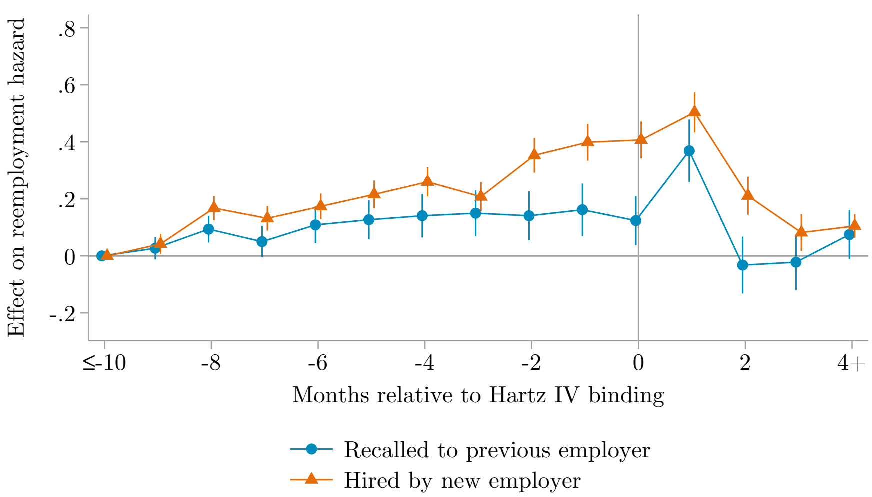
Notes: Part-time marginal and non-marginal employment as a share of total employment, using public data from the Federal Employment Agency. The April 2003 Hartz II reform expanded the definition of marginal jobs ("mini-jobs") by eliminating the hours ceiling, raising the earnings ceiling, and permitting socially insured workers to hold mini-jobs on the side without incurring extra tax liability. I exclude such side-jobs from both the numerator and the denominator of the measures shown here.

Appendix Figure 9: Cross-group relationship between the severity of benefit cuts and the increase in job-finding in the month Hartz IV binds



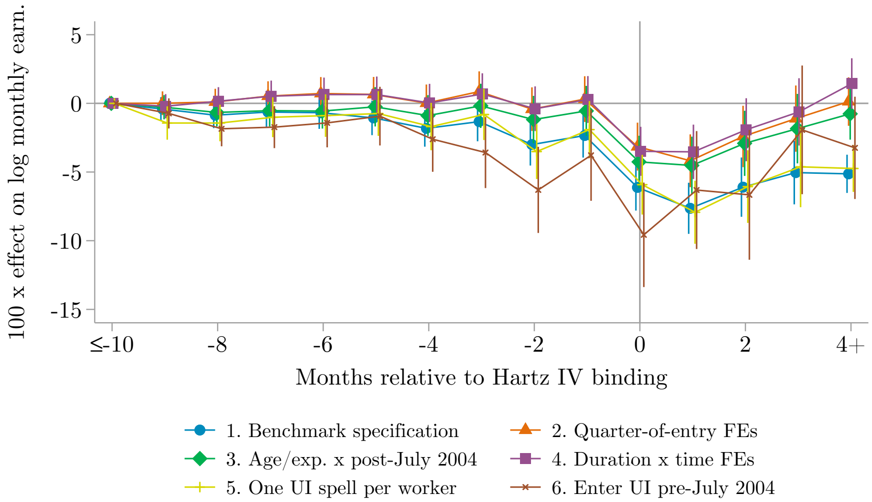
Notes: Each point represents one of 36 cells defined by sex × region × household type × terciles of UI benefit level. I estimate my benchmark hazard model separately for each cell, pooling the event months $\{-9, -8, -7\}$, $\{-6, -5, -4\}$, $\{-3, -2, -1\}$, $\{0\}$, $\{1, 2, 3\}$, and $\{4, 5, 6, \ldots\}$ to improve precision in these subgroup models. For each cell, I plot the estimated hazard effect for the month Hartz IV binds ($\exp(\hat{\delta}_0^H) - 1$) against the mean change in either long-term cash benefits (left panel) or post-exhaustion net household income (right panel), as simulated in Appendix A. Marker sizes are weighted by number of claimants.

Appendix Figure 10: Effects of Hartz IV on transitions to previous vs. new employers



Notes: Proportional effects of Hartz IV on the competing risks of either being recalled to the previous employer or being hired by a new employer (based on whether pre- and post-UI employer identifiers coincide).

Appendix Figure 11: Robustness of wage effects to additional controls and sample selections



Notes: Effects of Hartz IV on the log ratio of monthly earnings in the first regular job after UI to monthly earnings in the job that preceded entry to UI, from variants of the regression estimated in Figure 14. See the notes to Figure 6 for a discussion of the control variables and sample restrictions used in each specification.

Appendix Table 1: Summary statistics for the estimation sample and comparison group

	Estimation sample:			Comparison:
	Full sample	**Enter UI in 2001**	**Enter UI in 2005**	
	(1)	(2)	(3)	(4)
Baseline characteristics				
Basic demographics				
Female	37.7	39.6	36.9	43.8
Age	38.8	38.7	38.8	39.9
	(8.3)	(8.1)	(8.4)	(8.0)
East German resident	34.6	40.5	31.5	20.5
Non-German native	10.4	9.7	10.5	8.1
Education				
No vocational training, no university exam	8.3	8.8	7.5	8.4
Vocational training or university exam	77.4	78.3	77.5	76.6
University degree (incl. *Fachhochschulen*)	14.4	12.9	14.9	15.0
Household structure				
Unmarried	48.1	45.8	49.8	*
Married without children	25.9	26.2	25.4	*
Married with children	26.0	28.0	24.9	*
Wage (pre–job loss) × 30 days, 2005 EUR	1,860.9	1,846.9	1,806.6	2,517.8
	(881.0)	(845.4)	(870.8)	(1,172.8)
Initial monthly UI benefit, 2005 EUR	806.9	795.8	815.4	*
	(304.1)	(287.6)	(319.0)	*
Employed 4+ of last 7 years	68.8	64.2	74.4	83.7
Claimant outcomes				
Reemployed into socially insured job within ...				
6 months	46.9	47.1	50.8	*
12 months	61.9	61.5	66.5	*
24 months	73.3	72.7	77.8	*
36 months	78.3	77.6	82.4	*
Wage (when reemployed) × 30 days, 2005 EUR	1,776.1	1,805.6	1,728.7	*
Sample size				
Number of UI claims	336,634	60,065	61,486	*
Number of distinct individuals	244,666	58,012	58,762	*

Notes: The estimation sample consists of prime-age individuals (ages 25–54) who initiate a UI claim during 2001–2005 within 30 days of being displaced from a socially insured job. The comparison group is a 2 percent sample of prime-age workers observed in 2001–2005 who are employed in socially insured jobs and not claiming UI. Household structure is reported in the unemployment register, not in the employment records, and hence cannot be constructed for the comparison group. Values are percentages except where indicated.

Appendix Table 2: Effect of Hartz IV on the probability a claimant has ever been reemployed (by year of entry into UI)

Year of UI entry (1)	**Month** (2)	**% reemployed w/reform** (3)	**% reemployed w/o reform** (4)	**Effect of Hartz IV** (5)
De facto unexposed				
2001	6	47.16	47.16	0.00
2001	12	61.68	61.68	0.00
2001	24	72.34	72.34	0.00
Partially exposed				
2002	6	46.22	46.22	0.00
2002	12	60.59	60.59	0.00
2002	24	71.79	71.48	0.31
2003	6	45.99	45.66	0.32
2003	12	60.57	59.50	1.07
2003	24	71.88	69.86	2.03
2004	6	45.73	43.02	2.70
2004	12	61.26	57.27	3.99
2004	24	73.23	69.67	3.56
Fully exposed				
2005	6	50.65	47.51	3.14
2005	12	66.92	62.78	4.14
2005	24	78.25	74.76	3.49

Notes: Effects of Hartz IV on the probability that an individual is reemployed within 6, 12, or 24 months of entering UI. I use my benchmark hazard estimates to predict the probability of reemployment both under Hartz IV and under a counterfactual in which the benefit cuts do not occur, then average the gap between these probabilities across claimants in each cohort.

A How Did Hartz IV Affect Long-Term Benefit Levels?

To characterize Hartz IV's impact on benefit levels and household income, I adapt the OECD Tax-Benefit Model to simulate the benefit cuts applicable to each claimant in my sample.[1] In Appendix A.1, I provide a brief overview of my methodology. In Appendix A.2, I show that the new benefit schedule subjected most claimants to large declines in direct cash transfers and more modest declines in household income. Since my estimates do not fully account for stricter means testing, they are likely a lower bound on Hartz IV's true effect on benefits and income. Appendix A.3 describes my simulation methodology in greater detail and summarizes key provisions of the German tax/benefit system.

A.1 Simulation methodology: a brief overview

The OECD Tax-Benefit Model provides a rich characterization of the German welfare state. Given a vector of household attributes, the model applies statutory rules to calculate income tax liability, social insurance contributions, UI and long-term benefit levels, means-tested social assistance, and a variety of other transfers.

I observe many key inputs into these calculations, including prior earnings, East/West residence, marital status, the number of dependent children, and the age of the youngest child. I impute spousal earnings—another key input—by exploiting Germany's use of "income-splitting" in the taxation of married couples. Since I do not observe assets, I assume that households have negligible assets by the time they exhaust UI. Additional assumptions are needed for rental expenses, the ages of older children, and other inputs. Following the OECD, I assume that households apply for any public benefits for which they are eligible.

Using the estimation sample described in Section 3, I feed observed and imputed household characteristics into Germany's 2004 and 2005 tax/benefit rules to simulate each claimant's household income just after UI exhaustion. To gauge the effect of Hartz IV on claimants' finances, I first compute the change in long-term cash transfers—inclusive of social assistance and temporary supplements—that a household faces under the new rules. Next, I compute the overall change in household income, accounting for housing assistance and any offsetting changes in other taxes and benefits. To put these measures on an apples-to-apples basis, I denominate both measures by post-exhaustion income under the pre-reform rules.

A.2 Simulation results: the effect of Hartz IV on benefit levels

Appendix Figure 12 plots smoothed pdfs of the two measures. The left panel shows that Hartz IV cut long-term cash benefits sharply for most claimants, with a mean decline equal to 24.2 percent of counterfactual income. There is a point mass around zero driven by a subset of married claimants, mainly women, who are ineligible for long-term benefits under both old

[1] My simulations are adapted from the OECD's publicly available Stata programs, downloadable at `http://www.oecd.org/els/soc/Models.zip`. I have also drawn on code from Alexandre Desbuquois's Stata package TAXBENEXTRACT. Information in this section is taken from the German Social Code, the series *Social Security at a Glance* published by the German Federal Ministry of Labour and Social Affairs, and the annual OECD publication *Taxing Wages*.

and new rules by virtue of having high-earning spouses.[2] A small share of claimants actually saw modest *increases* in benefits (Bloß and Rudolph, 2005). Even so, large majorities of both men and women faced steep cuts to their long-term cash benefits. The right panel examines household income. Though housing assistance and other transfers considerably mitigate the direct loss in cash benefits, I find a mean net income decline of 4.4 percent.

Hartz IV likely reduced benefits by more than these numbers suggest. First, my simulations conservatively assume that the asset test never binds, but some claimants were likely disqualified under the reform's more stringent asset test. Second, the "on-impact" income losses shown in the figure were reinforced by additional cuts once claimants' temporary supplemental benefits ran out. Third, Hartz IV shifted the composition of net transfers towards housing assistance, which may be valued less than nominally equivalent cash transfers.

A.3 Simulation methodology: details

Step 1: imputing spousal earnings

Spousal earnings are a key determinant of long-term benefit levels, but spouses are not linked in my data.[3] To circumvent this problem, I infer spousal earnings by exploiting two provisions of Germany's tax/benefit system. First, UI benefits are indexed to *after-tax* earnings in the lead-up to job loss, enabling me to infer each claimant's income tax liability. Second, an individual's tax liability is an explicit function of both own and spousal earnings, so I can invert the tax formula to approximate spousal earnings.

Inferring income tax liability. A worker's initial monthly UI benefit is calculated as

$$\text{UI benefit} = \text{RR} \times (\text{gross earnings} - \text{income tax} - \text{social insurance contributions}) \quad \text{(A.1)}$$

I observe the initial UI benefit level as well as gross monthly earnings prior to UI receipt. Benefits are indexed to average earnings over the 1 or 2 years preceding job loss, whichever is more favorable to the claimant, so I compute prior earnings on this basis. Social insurance contributions are a simple function of earnings and hence easily calculated.[4] The replacement rate (RR) equals 60 or 67 percent, depending on whether the claimant has dependent children (which I also observe). I can therefore invert Equation A.1 to express a claimant's income tax liability as a function of observables.

Inferring spousal earnings. Income taxes are levied on gross household earnings, net of deductions. Married couples typically file jointly, and—crucially for my purposes—tax cal-

[2] A second mass point, with benefit drops around 40 percent of counterfactual income, reflects the role of temporary supplemental payments that cushion the drop in benefits for a subset of eligible claimants.

[3] Goldschmidt et al. (2017) match cohabiting spouses in IAB data on the basis of surnames and residential addresses, but only a minority of German couples are successfully matched and the matched sample is not fully representative. Furthermore, only in a small and non-random share of cases would both partners appear in the data extracts I am able to access.

[4] In 2004, workers owed 9.75 percent of gross earnings for pension contributions, 3.25 percent for UI, 7.00 percent for health insurance, and 0.85 percent for long-term care insurance, up to ceilings that are seldom reached by claimants in my sample. Employers make equal-sized contributions. Mini-jobs (and so-called "midi-jobs") are partially exempt from these contributions.

culations employ the so-called "income splitting" method: the spouses' earnings are added together and divided by two to determine each spouse's taxable income. Tax liability is then computed as a piecewise quadratic function of taxable income (plus a "solidarity surcharge"). Because of income splitting, married individuals face tax bills of the form

$$\text{income tax} = f(0.5 \times [\text{own earnings} + \text{spousal earnings} + \text{other taxable income} - \text{deductions}]) \tag{A.2}$$

where $f(\cdot)$ is a known function. I observe own gross earnings and, as noted above, I can deduce income tax liability. I can directly calculate deductions for social insurance contributions and dependent children; following the OECD, I also assume that individuals claim deductions for work-related and special expenses.[5] After inverting Equation A.2 and netting out these terms, I am left with the sum of spousal earnings and other taxable income. I assume that other taxable income is zero, yielding a proxy for spousal earnings.[6]

Despite the intricacy of this procedure, my measure of spousal earnings passes a suite of validity checks reported in Price (2017):

1. Imputed spousal earnings are much higher for female claimants (with male spouses) than for male claimants, consistent with the gender gap in employment and earnings. This gender gap is larger in West Germany, where fewer women are employed.
2. Pre-UI own earnings are positively correlated with imputed spousal earnings, as expected given assortative matching on earnings potential.
3. Among male claimants (with female spouses), spousal earnings are decreasing in the number of children, with an additional drop if the youngest child is under age 5.
4. Calculating "spousal earnings" for unmarried claimants yields much smaller numbers.

For claimants who were married at entry into UI, I therefore impute spousal earnings on this basis. For unmarried claimants, I set spousal earnings equal to zero.

Step 2: calculating benefits under the 2004 rules

Long-term unemployment assistance

Upon exhausting UI (*Arbeitslosengeld*), claimants could apply for long-term unemployment assistance (*Arbeitslosenhilfe*) financed out of general revenues. Long-term assistance was means tested on the basis of both household assets and household income. Workers could claim these benefits indefinitely, provided they continued to satisfy annual means testing.

Exhaustees could not transition to long-term benefits unless household assets fell below an exemption level. Prior to 2003, the asset exemption equaled €520 times the sum of the claimant's age and (if married) spouse's age. Under Hartz I, the asset limit fell to

[5] Households with dependent children may claim either a child tax allowance or a child tax credit. Since the federal tax office makes this choice on households' behalf, acting to minimize their tax liability, I select the tax-minimizing option.

[6] The other main sources of taxable income are investment returns and self-employment. Since the workers in my sample are adversely selected on baseline earnings, they are unlikely to have much investment income; since they have separated from socially insured jobs, they are unlikely to have had much much self-employment income during the lookback period.

€200 times own + spousal age for long-term benefit claims initiated after January 1, 2003. Certain protected assets, such as "reasonable" owner-occupied houses, automobiles, and some pension accounts, did not count against these limits. While I do not observe asset holdings in the IAB data, tabulations in the German Socioeconomic Panel (SOEP) suggest that only a small share of UI claimants would face a binding asset test, even under the post-2003 rules.[7] As noted above, my simulations assume that claimants have negligible assets by the time they exhaust UI, so that the asset test never binds.

Conditional on satisfying the asset test, long-term benefits replaced up to 53 percent of prior net earnings for childless claimants and 57 percent for claimants with one or more children. Benefits are reduced one-for-one for earnings above the UI earnings disregard, as well as for alternate sources of income such as rentals.[8] In all simulations, I assume that claimants have zero earned income while unemployed. Benefits are also reduced one-for-one for spousal earnings net of taxes, work-related expenses, and a disregard level. I use imputed spousal earnings to apply this income-based means test.

Other benefits

Sufficiently poor households could also apply for supplemental means-tested welfare benefits, known as social assistance (*Sozialhilfe*). The level of social assistance was calculated as

$$\text{social assistance} = \max(0, \text{assessed need} - \text{household income net of means testing}), \quad \text{(A.3)}$$

where a household's assessed need is the sum of individual allowances and housing/heating allowances.

Individual allowances were revised annually and varied across municipalities. For 2004, the OECD reports that the base allowance paid to household heads averaged €295 in the West and €285 in the East; additional payments were made for dependent spouses (80 percent of the base rate) and children (50–90 percent depending on age), with extra assistance available to single parents. I impute the ages of older children by assuming a two-year birth interval between children, then compute base benefits using these OECD averages. Households eligible for social assistance were reimbursed for all "reasonable" housing and heating expenditures. Since local definitions of reasonable cost are not readily available, I use a schedule of allowed housing/heating costs as a function of household size published by the Berlin Senate in 2005.

Once need had been assessed, social assistance was means tested on the basis of household income—specifically, gross earnings (net of a disregard) plus unemployment benefits plus alimony minus income taxes minus social security contributions. I use the OECD's alimony rate for single parents with children under age 6, and I calculate the other components of the means test as described earlier in this appendix.

[7] The 2002 SOEP wave asked respondents whether they held financial assets exceeding €2500 and, if so, the value of these assets. Among prime-age respondents receiving UI benefits on the survey date, I calculate that 11.7 percent had own + spousal financial assets in excess of the age-based limit applicable to post-2003 claims. Asset holdings are likely further decumulated by the time these claimants reach UI exhaustion.

[8] The earnings disregard is the larger of €165 or 20 percent of the full benefit amount; workers are also limited to 15 hours of work per week. Hartz IV reduced the earnings disregard for some workers by eliminating the 20 percent minimum disregard.

Both before and after Hartz IV, German households could also apply for means-tested housing benefits (*Wohngeld*) distinct from social assistance. The benefit level is a function of both household income and housing expenditures, up to a ceiling that depends on household size and the municipality's rent designation. I follow the OECD in using the highest rent designation for all households, regardless of where they reside.

Step 3: calculating benefits under the 2005 rules

Hartz IV replaced long-term unemployment assistance and social assistance with a single, means-tested benefit (*Arbeitslosengeld II*). Unlike the old system of long-term unemployment assistance, the new benefits are not indexed to prior earnings, but rather based (like social assistance) on the household's assessed need. The core benefits consist of (i) a basic cash payment, (ii) assistance with housing and utility expenses, and (iii) a temporary supplement to cushion the transition to the new regime.

In 2005, the base monthly allowance equaled €345 in the West and €331 in the East. Single adults receive 100 percent of the base allowance; married couples receive 90 percent per spouse. Each dependent child below [above] age 15 receives a benefit equal to 60 [80] percent of the base allowance. I use the claimant's region and household characteristics to assign these cash benefits. Next, the level of housing assistance is not explicitly stated in the law; instead, as under the previous system of social assistance, municipalities are instructed to cover "reasonable" costs. I again use Berlin's 2005 schedule of allowed housing costs.

To ease the transition to the new system, Hartz IV includes a temporary supplement for workers who exhaust UI. The supplement depends on the difference between the value of UI benefits just prior to benefit exhaustion and the value of long-term benefits thereafter.[9] In the first year after UI exhaustion, the supplement equals $\frac{2}{3}$ of the assessed difference, up to a ceiling that depends on household structure (€160 for singles and €320 for couples, plus €60 for each child). The supplemental payment and the payment ceiling are cut in half after one year and expire completely after two years.[10] I compute net replacement rates just after UI exhaustion, when the supplement is maximized.

[9] Departing from the OECD Tax-Benefit Model, I include housing/heating allowances in the post-reform benefit level when performing this calculation. Informational materials published by the Federal Employment Agency during the mid-2000s state explicitly that these allowances belong in the supplement formula.

[10] The two-year time limit begins on the date of UI exhaustion, even if exhaustion predated the onset of Hartz IV. For example, a claimant who exhausted UI on December 31, 2002 would not be eligible for the supplement even if still unemployed in 2005. A claimant who exhausted UI on June 30, 2003 would be eligible for a one-half supplement from January 1, 2005 through June 30, 2005 if unemployed during that period. For simplicity, I compute the supplement as though the claimant were at the beginning of the two-year grace period. This is a conservative assumption that maximizes the value of the temporary supplement (and hence the generosity of the post-reform system).

B Recipiency of Long-Term Benefits

Hartz IV reduced the share of claimants who received long-term benefits after exhausting UI, with two channels at play. First, fewer claimants were eligible for long-term benefits under the stricter means test. Second, the reform reduced the attractiveness of long-term benefits via lower benefit levels, stricter job search requirements, and heightened stigma. Given the logistical and psychic costs of applying for government transfers, we would expect a smaller share of eligible claimants to take up long-term benefits under the new regime. With long-term benefit amounts no longer indexed to prior wages, take-up incentives became especially weak for high earners. Consistent with these observations, I find a dramatic decline in benefit recipiency under the new rules, especially for claimants facing steeper benefit cuts.

B.1 Trends in benefit recipiency

To examine long-term benefit receipt, I use the SIAB data to construct a sample of all prime-age claimants who exhausted UI benefits between 1998 and 2010. For each such claimant, I observe the exact date of UI exhaustion as well as the earliest date on which the claimant began receiving long-term benefits (either *Arbeitslosenhilfe* before Hartz IV or *Arbeitslosengeld II* under the new regime). Because the vast majority of claimants who take up long-term benefits do so almost immediately, I focus on whether a claimant transitions to long-term benefits within 30 days of UI exhaustion.

Appendix Figure 13 plots recipiency of long-term benefits by date of UI exhaustion. The recipiency rate was relatively stable at around 70–75 percent between 1998 and 2002. The asset test was tightened somewhat in January 2003, under Hartz I, and the recipiency rate appears to have been slightly lower in subsequent quarters. The recipiency rate fell more noticeably in the second half of 2004, to about 65 percent, suggesting that fewer claimants sought long-term benefits after Hartz IV became salient but before it had taken effect.

Hartz IV reorganized Germany's employment services, and data collection was partly disrupted during the ensuing transition. The IAB notes that records on long-term benefit receipt are incomplete in 2006 and 2007 and cautions users against using data from these years. With that caveat, recipiency of long-term benefits appears to have fallen sharply at the onset of Hartz IV, and it declined further by the end of 2006. Recipiency averaged just 30–35 percent for claimants exhausting benefits during 2007–2010. The sharp drop in benefit recipiency underscores Hartz IV's dramatic transformation of the German benefit system.

B.2 Who received long-term benefits?

To further probe Hartz IV's effect on long-term benefit receipt, I next examine the relationship between demographic characteristics and benefit receipt before and after the reform. Omitting the years with suspect data quality, I look at UI claimants who exhausted benefits between January 2001 and June 2004 (the pre–Hartz IV sample) or between January 2007 and December 2008 (the post–Hartz IV sample). For each sample, I regress an indicator variable for long-term benefit receipt on successive combinations of sex, East/West residence, age, household structure, and terciles of UI benefit levels.

Appendix Table 3 reports the results. Prior to Hartz IV (column 1), benefit recipiency was 16.1 p.p. lower among women than among men, 16.9 p.p. higher among residents of East Germany relative to the West, and slightly lower among older claimants in a regression with no other controls. Adding interactions between sex, marital status, and parental status (column 2), I find that the gender gap in recipiency is driven almost entirely by married women, many of whom were ineligible for benefits due to high spousal earnings. I find only a weak and non-monotonic relationship between UI benefit levels (a proxy for prior earnings) and long-term benefit receipt (column 3). The high recipiency rate among East Germans is robust to additional controls and could reflect a weaker labor market, less asset accumulation, historical path dependence, or other factors like cultural norms.

Most of these patterns persist after Hartz IV (columns 4–6), with one notable exception. Under the new regime, claimants who had been entitled to higher benefit amounts—by virtue of high prior earnings—were much less likely to receive long-term benefits. Since Hartz IV replaced an earnings-indexed benefit with a uniform benefit unrelated to prior earnings, such claimants faced especially steep benefit cuts under the new system, and they consequently had less incentive to apply for long-term benefits.

C Computing Potential Benefit Durations

My research design hinges on my ability to accurately measure potential benefit duration (PBD) at the onset of a new UI claim. Although PBD is not recorded directly in the administrative data, I can impute it in two ways. Here I describe both imputation procedures in detail, assess the validity of my primary approach, and show that my hazard results are robust both to using the alternative procedure and to restricting attention to workers whose PBD is easiest to gauge.

Primary measure ("ex post")

My primary approach, which I call the *ex post measure*, imputes PBD based on the realized duration of a UI spell plus any residual UI entitlement remaining at the end of the spell. In the administrative data, each period of benefit receipt consists of one or more "notifications", with a variety of events (such as adjustments in benefit level) triggering a new notification. Critically, the data report the number of days of unused UI benefits (*Restanspruch*), if any, remaining at the end of each notification period.[11]

Let D denote the completed duration of the first notification period corresponding to a new UI claim, let R denote the duration of unused benefits at the end of this period, and let $\bar{P}(a)$ denote the claimant's age-specific maximum PBD, with all variables expressed in days. I compute start-of-claim PBD, in days, as

$$P^{\text{ex post}} \equiv \min(\bar{P}(a), D + R) \tag{C.1}$$

[11] Nagl and Weber (2016) report finding data irregularities in the *Restanspruch* variable, related to mid-claim updates to a claimant's residual benefit duration. Using a correction module provided by the IAB, I find that using the corrected variable developed by Nagl and Weber never alters my PBD calculation, evidently because the issues they discovered never apply to the *initial* UI notifications required for my procedure. My PBD measure is thus immune to the concerns they raise.

That is, I set PBD equal to the (observed) completed duration plus any time remaining in the worker's claim, overriding the result if it exceeds the legal maximum.[12] The resulting PBD distribution makes sense given the eligibility rules: in particular, the distribution of $D + R$ has large point masses at 180 days (the starting duration for a newly established UI entitlement) and at the age-specific maximums $\bar{P}(a)$. To conduct the analysis at monthly frequency, I express $P^{\text{ex post}}$ in months by rounding it to the nearest 30-day increment.

Alternative measure ("ex ante")

A potential concern with the ex post measure is that $P^{\text{ex post}}$ may deviate from start-of-claim PBD in the event that benefit entitlements are modified mid-claim. For example, participating in certain vocational training programs can slow the rate at which UI benefits are consumed, effectively prolonging the potential duration of UI benefits. In such cases, the ex post measure of PBD may be endogenous to post-claim worker behavior, which may in turn vary with effective exposure to the Hartz IV benefit cuts.[13]

To ensure that my results are not driven by such mid-stream benefit changes, I also impute PBD using an alternative *ex ante measure*. To construct this measure, I use a worker's observed employment and unemployment record—together with the programmatic rules applicable to UI claims initiated during 2001–2005—to calculate PBD solely on the basis of information observed by the start of a worker's UI claim. For each new UI claim beginning on date t_0, I do the following:

1. Determine whether the worker received UI benefits at any time in the 7 years preceding t_0. If so, let $R^{\text{prev}} \geq 0$ denote the number of days of UI benefits remaining (and thus recyclable) at the end of the previous claim.

2. Let $E \in \{0, 1\}$ indicate whether the worker established a fresh UI entitlement by being employed in socially insured jobs for at least 360 days out of the 3 years preceding t_0 (counting only jobs held since the previous UI claim, if any).

3. Let x denote the number of insured days worked during the last 7 years (again counting only jobs held since the previous UI claim). Since UI-entitled workers accrue 60 days of benefits for every 120 days employed, define $X \equiv 60 \cdot \text{floor}(x/120)$ as benefit-days accrued within a new entitlement.

4. Record each worker's age a as of t_0. Since I observe year of birth but not the exact date, I calculate a as though every worker were born on July 1. This assumption minimizes the number of classification errors, since workers who claim UI early [late] in the year tend to be younger [older] on t_0.

[12] Because I observe year but not date of birth, I cannot perfectly determine $\bar{P}(a)$ for workers who turn 45, 47, or 52 in the year of initial UI receipt. In these ambiguous cases, I assume that a claimant's birthday occurs before the beginning of the UI claim, to minimize the number of benefit durations that I override.

[13] In practice, the notification logic militates against this concern: the ex post procedure relies only on the *first* UI notification period, but subsequent changes in program participation should appear as fresh notifications not used in my procedure.

5. Seasonal workers with $E = 0$ may qualify for special 90- or 120-day UI entitlements if they have worked in socially insured jobs for at least 180 or 240 days, respectively. Although I cannot identify seasonal eligibility directly, I assign seasonal benefits $S \in \{0, 90, 120\}$ to all workers (i) who have worked sufficiently many days within the preceding 3 years and (ii) whose UI claims begin between October and January, when seasonal layoffs are concentrated in these data.

6. Adapting Equation C.1, I compute start-of-claim PBD, in days, as

$$P^{\text{ex ante}} \equiv \min(\bar{P}(a), E \cdot X + (1 - E) \cdot S + R^{\text{prev}}) \tag{C.2}$$

(rounding to the nearest 30 days when using a monthly version of this variable). That is, workers can accrue UI benefits through a standard entitlement or a seasonal entitlement, augmented with any prior unused benefits, up to the age-determined cap.

7. In some cases, the above procedure suggests that a worker is entitled to zero days of UI benefits. Since the fact of initiating a UI claim belies this result, I recode $P^{\text{ex ante}} = 180$ days when this occurs. Doing so makes sense if, for example, slight inaccuracies in employment dates result in workers appearing to just fall short of the threshold for establishing a new entitlement.

There are several reasons to expect that this procedure will not work perfectly. First, while I account for residual benefits from any previous UI claim, I cannot capture the full complexity of the rules mapping individual employment records into new benefit accruals. Although the above procedure is likely to perform well for workers who have not drawn on UI in the past, Schmieder et al. (2012) caution that calculating PBD in Germany "is not as clear cut for workers with intermittent unemployment spells because of complex carry-forward provisions in the law".[14] Second, workers can accrue UI benefits through military service, civil service, and other activities that are not recorded in the IAB data. Third, I can impute seasonal benefits only imperfectly. Fourth, $\bar{P}(a)$ is mismeasured for claimants in the vicinity of ages 45, 47, and 52. Finally, the discontinuous nature of benefit accrual means that slight errors in recorded employment dates can generate large errors in imputed PBD.

For all of these reasons, I view ex post PBD as a better measure of true start-of-claim PBD. In effect, the observed benefit duration D and residual benefit duration R serve as sufficient statistics for the complex (and not fully observable) administrative procedures that determine benefit eligibility. Consistent with this reasoning, I show below that hazard estimates using ex ante PBD are qualitatively similar to—but, as we should expect, attenuated relative to—those based on ex post PBD.

[14] For example, suppose that a new labor market entrant loses a job after paying into the UI system for 13 months. My algorithm will correctly award this claimant 6 months of UI benefits, effectively giving credit for 12 of these 13 months employed. If the claimant then exits UI after 3 months, the claimant will return to work not only with 3 months of residual benefits, which I capture, but also with 1 month of residual *employment* that may be credited towards a new UI entitlement. It is not clear how to carry forward such residual employment. Since accounting for all possible carry-forward scenarios requires knowledge of a worker's complete employment record, the problem is worsened by the absence of East German data prior to reunification.

Comparing the two measures

Appendix Table 4 reports summary statistics for the ex post and ex ante PBD measures, using the daily versions of these measures prior to rounding to the nearest month. The measures have very similar means and standard deviations, and the correlation between the measures across all claimants is 0.90. The two measures agree exactly, in days, for 68 percent of claimants; they differ by at most 30 days for 76 percent of claimants. Although we should not expect a perfect correspondence between the two measures (given the difficulties noted above in the calculation of ex ante PBD), the similarities between them are encouraging.

Because much of the difficulty in computing ex ante PBD stems from carryover of residual benefit entitlements from past UI claims, I also report these statistics separately for "UI veterans"—claimants who received UI benefits sometime in the 7 years preceding the present claim—and "UI newcomers", who have not done so. As expected, the two measures coincide much more often for newcomers (81 percent of cases) than for veterans (62 percent). Since the ex ante measure is free from any potential endogeneity concerns, the high level of agreement within this subsample suggests there is at most limited scope for the ex post measure to be contaminated by mid-claim changes in benefit duration.

Robustness to method of computing potential duration

My main hazard results are robust to changing either the PBD measure or the sample to minimize any threat from endogeneity in my primary measure. The first series in Appendix Figure 14 replicates my benchmark specification, which uses the ex post PBD measure to calculate time relative to benefit drops. The second series instead uses the ex ante PBD measure. The third series restricts the sample to claimants for whom the two measures coincide. The final series uses the ex post measure but restricts the sample to claimants who did not claim UI in the previous 7 years (for whom PBD is not complicated by carry-forward of unused benefits from previous spells). Raessuringly, the estimates are qualitatively and quantitatively similar across these specifications.

In sum, my results are unlikely to be materially influenced by any endogenous feedback whereby mid-claim claimant behavior influences my primary measure of PBD.

D A Framework for Jobless Durations and Wages

In this appendix, I develop a simple, non-stationary search model along the lines of Mortensen (1977) and van den Berg (1990) that illustrates the effect of long-term benefit cuts on jobless durations and reemployment wages. Building on the model, I then develop an econometric framework for estimating Hartz IV's overall wage effect, which I decompose into offsetting contributions from decreased selectivity and reduced time out of work.

D.1 Model setup

Consider a discrete-time model in which a displaced worker searches for a job until reemployed. At each duration $d \in \{1, 2, \ldots\}$ since job loss, search yields a job offer with probability s and entails utility cost $\phi_d(s)$, which is increasing and convex in s and satisfies

Inada conditions that ensure an interior optimum. Wage offers are drawn from a continuous distribution $G_d(w)$. Once accepted, a new job lasts forever.

Benefit schedule. The worker receives flow utility of consumption $u(\cdot)$, discounted at rate $\beta \in (0,1)$, and lives hand-to-mouth. The worker is entitled to UI benefits with potential duration P, followed by an indefinite stream of long-term benefits. The benefit level equals

$$b(d) \equiv \begin{cases} \bar{b} & \text{if } d \leq P \\ \underline{b} & \text{if } d > P \end{cases} \qquad \text{with } 0 < \underline{b} < \bar{b} \tag{D.1}$$

I model Hartz IV as a reduction in $\underline{b}$.

Duration dependence. Alongside the stepdown in benefits, I allow for two additional sources of duration dependence. First, the marginal cost of job search rises with duration: $\phi'_{d+k}(s) \geq \phi'_d(s)$ for all $k \geq 1$. This assumption aligns with empirical evidence that the efficacy of job search diminishes with time spent out of work, for example due to worker discouragement (Krueger and Mueller, 2011) or employer beliefs (Kroft et al., 2013). Second, the distribution of wage offers shifts left over time. In particular, I assume that $G_d(\cdot)$ satisfies a monotone likelihood ratio property: for $w' > w$, we have $\frac{g_{d+k}(w')}{g_d(w')} \leq \frac{g_{d+k}(w)}{g_d(w)}$. While the results that follow also hold in the special case of stationary search costs and wage offers, adding these forms of duration dependence helps to account for the properties of job-finding hazards and reemployment wages that I observe in the data.

Optimal policies. Let $U(d)$ denote the value of unemployment at duration d, and let $J(w) = \frac{u(w)}{1-\beta}$ denote the value of being employed forever at wage w. The optimal policy is a cutoff strategy with reservation wage $\underline{w}$, so that $U(d)$ admits a Bellman representation:

$$U(d) = \max_{s,\underline{w}} \; u(b(d)) - \psi_d(s) + \beta \left[\lambda(d)\mathbb{E}_d(J(w)|w \geq \underline{w}) + (1-\lambda(d))U(d+1)\right] \tag{D.2}$$

where $\lambda(d) \equiv s(1 - G_d(\underline{w}))$ is the hazard rate of being reemployed, which entails first obtaining and then accepting a job offer. The solution consists of policy functions $s^*(d)$ and $\underline{w}^*(d)$ denoting the search effort and reservation wage chosen at each duration.

To characterize the optimal policies, observe first that $U(d)$ is decreasing in d, since the three sources of duration dependence—rising search costs, deteriorating wage offers, and benefit exhaustion—all reduce utility over time. Since the optimal reservation wage satisfies $J(\underline{w}^*(d)) = U(d)$, it follows that $\underline{w}^*(d)$ is decreasing in d. By contrast, optimal search effort $s^*(d)$ may either rise or fall with duration. Heuristically, both the rising cost of job search and the worsening wage distribution increasingly discourage job search, but the stepdown in the benefit schedule increasingly incentivizes it.

On net, the hazard rate of reemployment $\lambda^*(d)$ may also either rise or fall over time. Under suitable functional form assumptions and parameter values, the model can generate a hazard profile that matches the data: decreasing early in the spell, rising as workers approach benefit exhaustion, and decreasing again after exhaustion.

D.2 The effects of benefit cuts on jobless durations

The following result underpins my research design, which posits that claimants with longer UI entitlements are effectively shielded from Hartz IV.

Proposition.

(a) *A long-term benefit cut increases search intensity and decreases the reservation wage at all durations. That is,* $\frac{ds^*(d)}{d\underline{b}} < 0$ *and* $\frac{d\underline{w}^*(d)}{d\underline{b}} > 0$ *for all* d*. It follows that* $\frac{d\lambda^*(d)}{d\underline{b}} < 0$*, so that a benefit cut increases the hazard rate of reemployment at all durations and therefore reduces the expected duration of the jobless spell.*

(b) *These behavioral responses limit to zero for benefit cuts lying sufficiently far in the future. That is, holding* d *constant,*

$$\lim_{P\to\infty} \frac{ds^*(d)}{d\underline{b}} = \lim_{P\to\infty} \frac{d\underline{w}^*(d)}{d\underline{b}} = \lim_{P\to\infty} \frac{d\lambda^*(d)}{d\underline{b}} = 0$$

Proof. Using the envelope theorem, we can express changes in the value of unemployment in terms of future utility flows discounted to reflect both time preference and the probability of remaining unemployed at each duration. After benefit exhaustion ($d > P$), we have

$$\frac{dU(d)}{d\underline{b}} = \sum_{k=0}^{\infty} \beta^k S(d+k) u'(\underline{b}) > 0 \tag{D.3}$$

where $S(d+k) \equiv \prod_{i=1}^{k}(1-\lambda^*(d+i))$ is the probability that the worker is still unemployed at $d+k$ (conditional on being unemployed at d). Before benefit exhaustion ($d \leq P$), we have

$$\frac{dU(d)}{d\underline{b}} = \beta^{P+1-d} S(P+1) \frac{dU(P+1)}{d\underline{b}} > 0 \tag{D.4}$$

Note that the Inada conditions on $\phi_d(\cdot)$ ensure that $S(P+1) \in (0,1)$, so that the worker exhausts UI with positive probability.

The FOC for search effort is

$$\phi_d'(s^*(d)) = (1 - G_d(\underline{w}^*(d)))(\mathbb{E}_d(J(w)|w \geq \underline{w}^*(d)) - U(d)) \tag{D.5}$$

Since changes in benefit levels do not affect the value of employment, the implicit function and envelope theorems imply that

$$\frac{ds^*(d)}{d\underline{b}} = \frac{-(1 - G_d(\underline{w}^*(d)))}{\phi_d''(s^*(d))} \frac{dU(d)}{d\underline{b}} < 0 \quad \text{for all } d \tag{D.6}$$

Furthermore, since $J(\underline{w}^*(d)) = \frac{u(\underline{w}^*(d))}{1-\beta} = U(d)$, we have

$$\frac{d\underline{w}^*(d)}{d\underline{b}} = \frac{1-\beta}{u'(\underline{w}^*(d))} \frac{dU(d)}{d\underline{b}} > 0 \quad \text{for all } d \tag{D.7}$$

The fact that $\frac{d\lambda^*(d)}{d\underline{b}} < 0$ immediately follows.

To see that these behavioral responses limit to zero, observe that, in Equation D.4, $\lim_{P\to\infty} \beta^{P+1-d} S(P+1) = 0$ and $\frac{dU(P+1)}{d\underline{b}}$ is bounded, which implies that $\lim_{P\to\infty} \frac{dU(d)}{d\underline{b}} = 0$. Since $\frac{ds^*(d)}{d\underline{b}}$ and $\frac{d\underline{w}^*(d)}{d\underline{b}}$ are proportional to $\frac{dU(d)}{d\underline{b}}$, the result follows. □

D.3 An econometric framework for reemployment wages

For each claimant i and duration d, the model above yields a hazard rate of reemployment λ_{id}. Let D_i denote the realized jobless duration, let $p_{id} \equiv \Pr(D_i = d)$ be the associated pdf, and let $F_{id} \equiv \sum_{s=1}^{d} p_{is}$ denote the probability that i is reemployed by horizon d. These expressions are related to the hazard rate by the identity $p_{id} \equiv \lambda_{id} \prod_{s=1}^{d-1}(1-\lambda_{is})$.

The model also yields a reservation wage $\underline{w}_{id}$. Let w_i denote the realized (log) wage, and let $\mu_{id} \equiv \mathbb{E}(w_i \mid D_i = d)$ denote i's expected wage realization conditional on being reemployed at duration d. I condition $(\lambda_{id}, p_{id}, F_{id}, \mu_{id})$ on a vector of observables $\mathbf{x}_i$ used in the empirical analysis.

If we could follow claimants indefinitely, and if all claimants were eventually reemployed, then we could express the effect of Hartz IV on mean reemployment wages as

$$\Delta\mathbb{E}(w_i) = \sum_{i=1}^{N}\sum_{d=1}^{\infty} p_{id}\mu_{id} - \sum_{i=1}^{N}\sum_{d=1}^{\infty} p_{id}^{cf}\mu_{id}^{cf}, \tag{D.8}$$

where the superscript "*cf*" indicates counterfactual values in the absence of Hartz IV.

In practice, however, I observe reemployment wages only for a subset of claimants. As I show in Section 7.1, the effect of Hartz IV on the mean wage observed among claimants reemployed within 24 months of UI entry can be expressed as

$$\Delta\mathbb{E}(w_i \mid D_i \leq 24) \equiv \sum_{i=1}^{N}\pi_i \sum_{d=1}^{24} q_{id}\mu_{id} - \sum_{i=1}^{N}\pi_i^{cf} \sum_{d=1}^{24} q_{id}^{cf}\mu_{id}^{cf}, \tag{D.9}$$

where $\pi_i \equiv \frac{F_{i,24}}{\sum_{j=1}^{N} F_{j,24}}$ is claimant i's (probabilistic) share of the reemployed pool and $q_{id} \equiv \Pr(D_i = d \mid D_i \leq 24) = \frac{p_{id}}{F_{i,24}}$ is the conditional pdf of jobless duration.

Equation D.9 reflects not only the effect of Hartz IV on individual claimants' wages, but also selection into reemployment induced by the reform. To correct for selection, I modify this expression to gauge how Hartz IV affected wage realizations that would have been observed whether or not Hartz IV had been implemented.

My selection correction exploits the fact that—as predicted by the model and confirmed in my estimates—Hartz IV accelerated cumulative job-finding for all workers in the sample, so that $\widehat{F}_{id} \geq \widehat{F}_{id}^{cf}$ for all i and all d. This enables me to define a *truncated* distribution of jobless durations under Hartz IV:

$$\widetilde{F}_{id} \equiv \min(F_{id}, F_{i,24}^{cf}) \quad \text{for } d \in \{1, 2, \ldots, 24\} \tag{D.10}$$

with corresponding truncated pdfs $\widetilde{p}_{id} \equiv \widetilde{F}_{id} - \widetilde{F}_{i,d-1}$ and $\widetilde{q}_{id} \equiv \widetilde{p}_{id}/\widetilde{F}_{i,24}$. Intuitively, the modified distribution $\widetilde{F}_{id}$ traces out each worker's reemployment trajectory under Hartz IV, then discards the "excess" probability mass that would have been allocated past the censoring horizon in the absence of Hartz IV.[15] By construction, $\widetilde{F}_{i,24} \equiv F^{cf}_{i,24}$, which in turn implies that $\widetilde{\pi}_i = \pi^{cf}_i$ for all i, where $\widetilde{\pi}_i \equiv \widetilde{F}_{i,24} / \sum_{j=1}^{N} \widetilde{F}_{j,24}$ denotes a worker's share of the reemployed pool under $\widetilde{F}_{id}$. Using the modified distribution places the same weight on each claimant in the factual and counterfactual scenarios, thereby eliminating compositional bias.

Employing this correction, I estimate the quantity

$$\Delta\widetilde{\mathbb{E}}(w_i \mid D_i \leq 24) \equiv \sum_{i=1}^{N} \widetilde{\pi}_i \sum_{d=1}^{24} \widetilde{q}_{id}\mu_{id} - \sum_{i=1}^{N} \pi^{cf}_i \sum_{d=1}^{24} q^{cf}_{id}\mu^{cf}_{id} \tag{D.11}$$

$$= \sum_{i=1}^{N} \pi^{cf}_i \left(\sum_{d=1}^{24} \widetilde{q}_{id}\mu_{id} - \sum_{d=1}^{24} q^{cf}_{id}\mu^{cf}_{id} \right), \tag{D.12}$$

which can be interpreted as the wage effect of Hartz IV among workers who, probabilistically, would have been reemployed within 24 months even if Hartz IV had not been implemented.

E Further discussion of the placebo exercise

In Section 5.2, I model the effects of pseudo–Hartz IV reforms taking effect on January 1 of each year over 1998–2004. As shown in Figure 7, the 1998–2003 pseudo-reforms have no material effects on the job-finding hazard, but the 2004 pseudo-reform has positive effects. What explains these modest placebo effects?

One possibility is that, despite truncating this placebo analysis in June 2004, I may still be picking up anticipation of Hartz IV itself. Although final passage of Hartz IV did not occur until July of that year, the law passed the lower house of parliament in December 2003. If UI claimants were sufficiently attentive, patient, and concerned about potentially draconian benefit cuts, they might have begun reacting to Hartz IV far in advance of the actual implementation date. If so, these placebo coefficients might be regarded as causal effects of the policy change itself.

Second, I may be detecting lagged effects from a stricter asset test applicable to long-term benefit claims initiated after January 1, 2003. The IAB data do not report assets, but I observe a small decline in the share of UI exhaustees who transition to long-term benefits after this date, suggesting that the tighter asset limits did bind for some claimants. Interacting the main effect of benefit exhaustion (τ^E_{id}) with an indicator for being subject to the new asset rules has little impact on the 2004 placebo effects, weighing against this explanation. Adding these interactions also has little impact on my estimated Hartz IV effects.

[15] We can think of job search as a strategy that maps states of the world ω into realized jobless durations. Letting $D_i(\cdot)$ and $D^{cf}_i(\cdot)$ denote i's Hartz IV and no-Hartz strategies, define the crossover date $\widetilde{D}_i$ such that $\Pr(D_i(\omega) \leq \widetilde{D}_i) = \Pr(D^{cf}_i(\omega) \leq 24)$. My estimates imply that $\widetilde{D}_i \leq 24$ for all i. Let $\Omega^{cf}_{i,24} \equiv \{\omega \mid D^{cf}_i(\omega) \leq 24\}$ be the set of uncensored states under the no-Hartz strategy, and let $\Omega_{i,\widetilde{D}_i} \equiv \{\omega \mid D_i(\omega) \leq \widetilde{D}_i\}$ be the set of states yielding reemployment before $\widetilde{D}_i$ under Hartz IV. Suppose that $D^{cf}_i(\omega) \leq D^{cf}_i(\omega') \iff D_i(\omega) \leq D_i(\omega')$ for all ω, ω'. Under this rank-preservation assumption, $\Omega^{cf}_{i,24} = \Omega_{i,\widetilde{D}_i}$. The modified distribution $\widetilde{F}_{id}$ seeks to compare wages over this common set of states, for which wages are observed under both strategies.

Third, the placebo effects may reflect a mid-2003 increase in the frequency of benefit sanctions. Hartz I narrowed the grounds on which claimants could turn down job offers, and in April 2003 the Federal Employment Agency instructed caseworkers to apply sanctions more vigorously (Müller and Steiner, 2008). The Agency's data show that the number of sanctions imposed for refusing job offers tripled over the course of 2003, peaking in September at 16,900, but it then declined by half through the end of 2004. Since sanction rates were *falling* in the run-up to Hartz IV, they are unlikely to explain my core results.

To my knowledge, sanction risk was not directly tied to time remaining until benefit exhaustion.[16] But a spurious correlation between sanctions and the placebo-reform event-time variable could potentially arise if the likelihood of being sanctioned varies with duration since entry into UI. This is quite possible, since the set of jobs that a claimant is expected to accept broadens with time spent out of work (Ebbinghaus and Eichhorst, 2009). That fact motivates the right panel of Figure 7, where I augment my benchmark specification by allowing the shape of the baseline hazard rate to vary flexibly over time. These additional controls absorb any temporal changes in job-finding that are linked to *duration since entry*, as distinct from *duration until benefit cuts*. The fact that adding these controls dampens the placebo coefficients (while strengthening the effects of the true 2005 reform) is consistent with such a spurious correlation.

F Unobserved heterogeneity in wage recovery

The wage effects estimated in Section 7.2 could potentially be biased by dynamic selection into reemployment induced by Hartz IV. Importantly, Equation 7.6 already accounts for three different forms of selection. First, by comparing pre- and post-UI wages, I net out any time-invariant differences in earnings levels across workers. Second, by conditioning on realized jobless durations, I also net out compositional changes in the pool of the unemployed at each duration. Third, by controlling for sex, age, and other observables, I allow different groups of workers to experience differential wage recovery after job loss. Even so, my estimates may still be confounded by unobserved heterogeneity in workers' wage recovery after job loss. For example, if the marginal jobseekers who find jobs in response to Hartz IV are less comfortable in job interviews (and hence less likely to negotiate their starting salaries), they may exhibit below-expected wage recovery regardless of when they are reemployed.

To assess this concern, I use repeat UI spells to test—and control—for time-invariant unobserved heterogeneity in post-UI wage recovery. For each UI claim in my sample, I use employment records dating back to 1993 to identify the worker's *previous* claim (if any). Let w_i^{prev} denote the log pre/post wage change associated with this earlier claim. I reestimate Equation 7.6 with w_i^{prev} as the outcome variable, leaving unchanged all of the explanatory variables associated with the current claim i. If the workers who find jobs when Hartz IV binds are prone to experiencing worse-than-average wage losses in the aftermath of job loss, then we should see a similar pattern of declining δ_k^H coefficients in this modified specification.

The left panel of Appendix Figure 15 plots the estimated δ_k^H coefficients from my

[16] Using an RD design, Schmieder and Trenkle (2020) show that—at least during their 2008–2010 sample period—the hazard rate of being sanctioned is invariant to potential benefit duration throughout a UI spell, suggesting that caseworkers do not disproportionately sanction claimants whose benefits are running out.

benchmark wage specification alongside the corresponding coefficients from this prior-claim regression, with both models estimated on the subset of UI claims for which I can identify a valid prior claim. The current-claim coefficients are quite similar to those estimated in the full sample. The prior-claim coefficients tend to have negative point estimates, but the estimates are small in magnitude and most are statistically indistinguishable from zero. An alternative is to subtract the prior wage change from the current wage change (putting $w_{id} - w_i^{\text{prev}}$ on the left-hand side), which differences out any time-invariant individual wage-loss effect. The right panel of Appendix Figure 15 shows that this double-difference specification yields coefficients very similar to my benchmark estimates, albeit less precise. To the extent that unobserved heterogeneity takes this time-invariant form, accounting for it does not alter the conclusions from my wage analysis.

G Implied Effect on Steady-State Unemployment

In Section 8.1, I use my estimated causal effects of Hartz IV on individual job-finding rates to assess the (partial equilibrium) impact of the reform on Germany's unemployment rate. The basic idea is to (i) track transitions between employment, unemployment, and non-participation; (ii) estimate the hazard rates of transition between these states with the effects of Hartz IV turned either on or off; and (iii) compute the steady-state unemployment rate under each scenario. The procedure is as follows.

Constructing a panel dataset. I start by constructing a person × month panel encompassing all individuals who appear in the SIAB data at any point during 1995–2010.

Individuals are observed in these data whenever they are employed, registered with the Federal Employment Agency, or receiving any benefits administered by the agency. To match employment concepts used in household surveys as closely as possible, I code individuals as employed if they hold either a socially insured job or a marginal job; as unemployed if they are registered as unemployed (and are not employed); and as non-participants otherwise.

The resulting sample includes non-participation spells among anyone who is employed, unemployed, or otherwise receiving benefits sometime during this period, but it excludes non-participants who never appear in the SIAB data. Using spell-level data on this sample, I construct a monthly panel by taking snapshots of each individual's labor market status on the 15th day of the month. To match the working-age population, I restrict to person × month observations in which the individual is aged 16–64.

Defining labor market states. After coding each individual in the sample as employed, unemployed, or out of the labor force, I obtain narrower classifications by measuring unemployment duration and the receipt of UI benefits.

To distinguish short-term vs. long-term unemployment, I define unemployment duration as the time elapsed since the start of the unemployment spell. In cases where a worker transitions from unemployment to non-participation and then back to unemployment, I count the interim period of non-participation towards duration in the second unemployment spell. I then code unemployed workers as short-term unemployed in their first 12 months of unemployment and as long-term unemployed thereafter.

To measure UI receipt, I identify individuals who claimed UI *at any point during their non-employment spell*, combining periods of unemployment and non-participation. This definition includes (i) periods of joblessness that precede UI take-up, (ii) periods of UI receipt, and (iii) periods after UI exhaustion or other exits from the benefit rolls.

With these definitions in hand, I assign each person-month to one of seven mutually exclusive labor market states, denoted by θ_{it}:

- Employed ($\theta = E$);
- Short-term unemployed, UI recipient ($\theta = S^B$);
- Short-term unemployed, non-recipient ($\theta = S^O$);
- Long-term unemployed, UI recipient ($\theta = L^B$);
- Long-term unemployed, non-recipient ($\theta = L^O$);
- Non-participant, UI recipient ($\theta = N^B$);
- Non-participant, non-recipient ($\theta = N^O$)

Calculating hazard rates prior to Hartz IV. To obtain a pre–Hartz IV baseline—which can be interpreted as a counterfactual scenario with no reform—I next restrict to observations in 2001–2003, shortly before Hartz IV was passed. Since I have already restricted the sample to individuals observed sometime during 1995–2010, anyone who was present in the data sometime during this longer period is assumed to have been present in Germany over 2001–2003 (and is coded as a non-participant when absent from the data).

For each "origin" state o and "destination" state d, let $N_o \equiv \sum_{i,t} \mathbb{1}\{\theta_{i,t-1} = o\}$ be the number of observations originating in state o, and let $N_{o\to d} \equiv \sum_{i,t} \mathbb{1}\{\theta_{i,t-1} = o \text{ and } \theta_{it} = d\}$ be the number of observed transitions from o to d. Then $\lambda_{o\to d} \equiv \frac{N_{o\to d}}{N_o}$ equals the average monthly transition rate between these states prior to Hartz IV.

Calculating hazard rates under Hartz IV. I assume that Hartz IV increases job-finding among all unemployed individuals who receive UI benefits at any point during their jobless spell. The logic of this assumption is that Hartz IV may boost job-finding rates even before UI take-up (if a worker intends to claim UI); during UI receipt (due to anticipated cuts to long-term benefits); and after UI exhaustion (due to realized cuts to long-term benefits). I conservatively assume that the reform has no effect on job-finding rates among non-participants, regardless of whether they previously received UI.

The benchmark estimates from Section 5.1 imply that Hartz IV increased the hazard rate of job-finding by an average of 12.5 percent within the first 12 months after entry into UI (applicable to the short-term unemployed) and by an average of 7.0 percent from month 13 through month 24 (applicable to the long-term unemployed). I therefore define the post–Hartz IV job-finding rates among unemployed UI recipients as $\lambda^{\text{Hartz}}_{S^B\to E} \equiv 1.125 \cdot \lambda_{S^B\to E}$ and $\lambda^{\text{Hartz}}_{L^B\to E} \equiv 1.070 \cdot \lambda_{L^B\to E}$. To ensure that exit rates from origin state S^B sum to unity, I scale down the hazard rates for the remaining destination states d by setting $\lambda^{\text{Hartz}}_{S^B\to d} \equiv$

$\frac{1-\lambda^{\text{Hartz}}_{S^B\to E}}{1-\lambda_{S^B\to E}} \cdot \lambda_{S^B\to d}$, and likewise for exit rates from the state L^B. I assume that the remaining hazard rates are unchanged under Hartz IV, so that $\lambda^{\text{Hartz}}_{o\to d} \equiv \lambda_{o\to d}$ for those transitions.

Estimating the effect of Hartz IV on the unemployment rate. To gauge the partial equilibrium effect of Hartz IV, I compute steady-state (or "flow-consistent") unemployment rates with Hartz IV turned on or off by using each set of hazard rates to simulate the dynamic system. For the no–Hartz scenario, I start with all workers employed in period 0, use the hazard rates $\lambda_{o\to d}$ to obtain the shares of individuals in each state in period 1, and iterate until convergence. Letting α_s denote the steady-state share of individuals in state s, I compute the pre–Hartz IV steady-state unemployment rate as

$$\overline{u} \equiv \frac{\alpha_{S^B} + \alpha_{S^O} + \alpha_{L^B} + \alpha_{L^O}}{\alpha_E + \alpha_{S^B} + \alpha_{S^O} + \alpha_{L^B} + \alpha_{L^O}} \tag{G.1}$$

I repeat this procedure for the Hartz IV scenario, obtaining post–Hartz IV shares α^{Hartz}_s and a post–Hartz IV steady-state unemployment rate $\overline{u}^{\text{Hartz}}$.

I estimate a steady-state unemployment rate of 11.4 percent prior to (or in the absence of) Hartz IV, somewhat above the official unemployment rate during this period. I find that Hartz IV reduced this rate to 10.7 percent, a decrease of 0.7 p.p.

I also compute the steady-state pre–Hartz IV *long-term* unemployment rate as

$$\overline{u}_L \equiv \frac{\alpha_{L^B} + \alpha_{L^O}}{\alpha_E + \alpha_{S^B} + \alpha_{S^O} + \alpha_{L^B} + \alpha_{L^O}}, \tag{G.2}$$

and similarly for $\overline{u}^{\text{Hartz}}_L$. I find that the long-term unemployment rate fell from 5.5 percent to 5.1 percent, a decline of 0.4 p.p., accounting for over half of the overall decline in unemployment.

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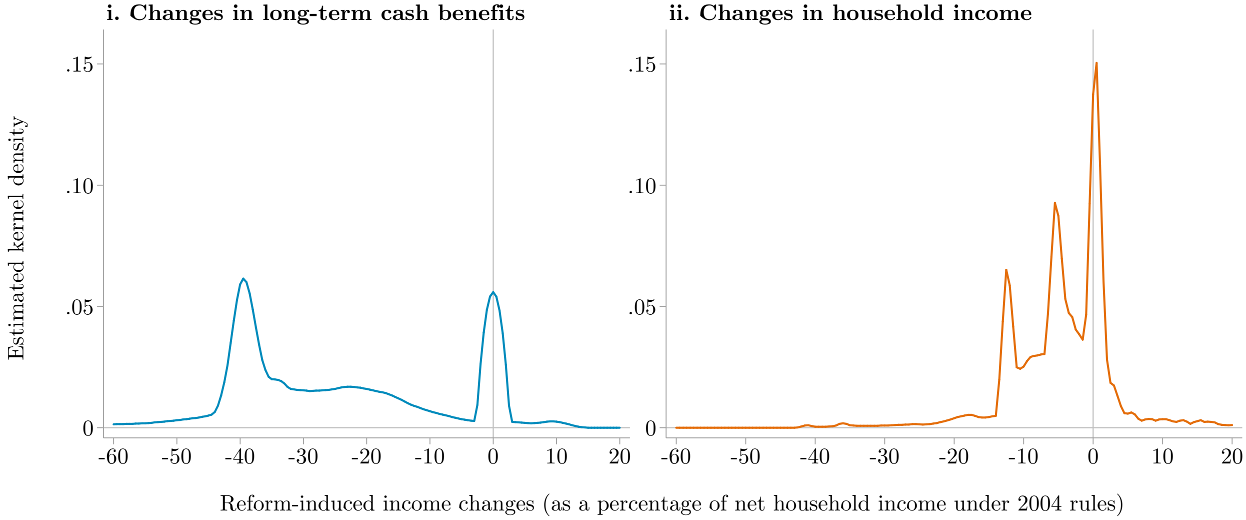
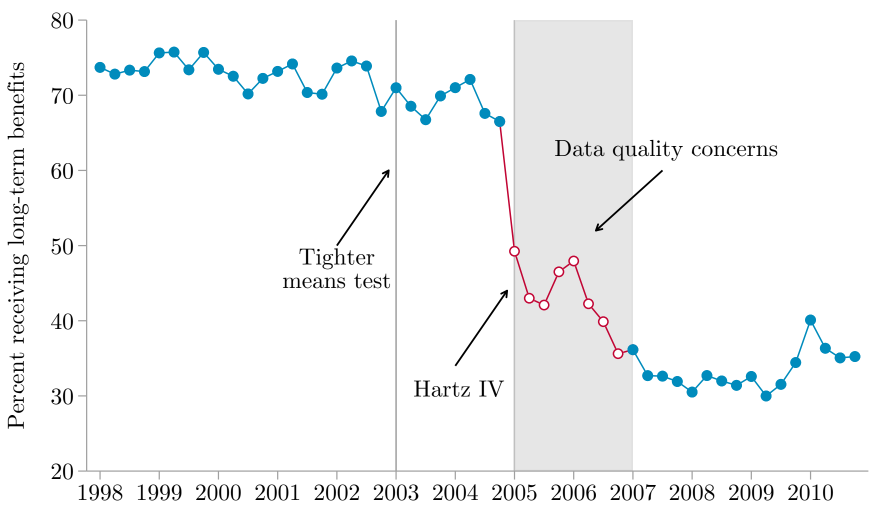
Figures and Tables Referenced Only in the Appendix

Appendix Figure 12: Simulated effects of Hartz IV on long-term benefit levels



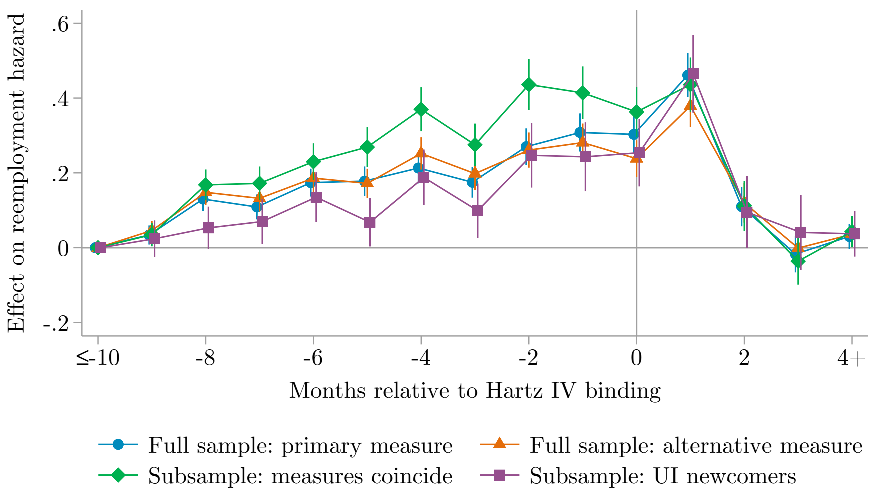
Notes: As detailed in Appendix A, I adapt the OECD Tax-Benefit Model to simulate benefits and income for each claimant in the estimation sample on the basis of observed and imputed characteristics. The left panel shows smoothed simulated changes in long-term cash benefits (unemployment assistance + social assistance) under the 2005 vs. 2004 rules, denominated by net post-exhaustion household income under the 2004 rules. The right panel shows simulated changes in net household income, using the same denominator.

Appendix Figure 13: Percentage of UI exhaustions followed by long-term benefit receipt



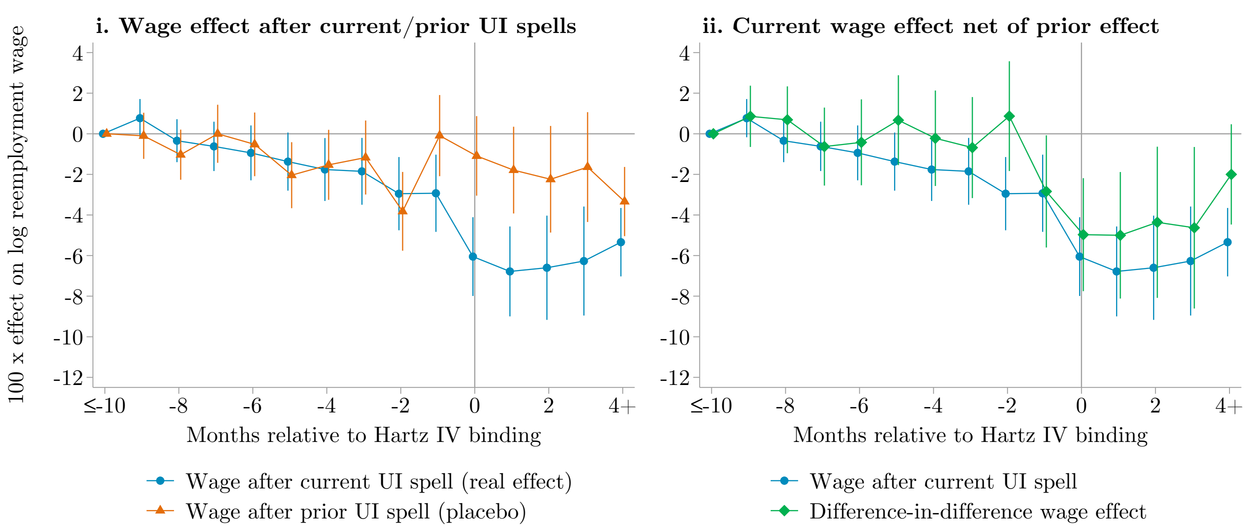
Notes: Percentage of UI claims exhausted in a given quarter that are followed by long-term benefit receipt within 30 days of exhaustion, using a 2 percent sample of prime-age claimants. The asset test for eligibility was tightened under Hartz I in January 2003 and tightened further under Hartz IV in January 2005. The data provider reports concerns about the quality of data on long-term benefit receipt during 2005 and 2006.

Appendix Figure 14: Effects of Hartz IV on the job-finding hazard rate using alternative measures of potential benefit duration



Notes: The first series reproduces my benchmark specification, which uses an "ex post" measure of potential benefit duration (PBD). The second series uses an alternative "ex ante" measure of PBD that relies solely on information observed by the start of a new UI claim. The third series restricts to claimants for whom the two measures coincide. The fourth series uses the ex post measure but restricts to claimants who did not receive UI in the previous 7 years (and for whom PBD is easier to calculate).

Appendix Figure 15: Robustness of wage effects to unobserved heterogeneity



Notes: This figure restricts attention to UI claims for which I observe a *prior* claim by the same worker over 1994–2005. In both panels, the blue series plot the estimated effects of the Hartz IV benefit cuts from my benchmark wage specification (Figure 14) for this restricted subsample. In the left panel, the orange series replaces the outcome variable by the log ratio of post-UI to pre-UI wages in the *prior* UI claim, keeping all explanatory variables coded at their original values. In the right panel, the green series replaces the outcome variable by the difference between the log wage changes in the two UI spells.

Appendix Table 3: Receipt of long-term benefits before and after Hartz IV

	Before Hartz IV (2001–2004)			After Hartz IV (2007–2008)		
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-16.12***	-2.83***	-2.82***	-12.78***	-0.29	-0.19
	(0.35)	(0.57)	(0.57)	(0.75)	(1.21)	(1.19)
East Germany	16.93***	16.29***	16.28***	14.05***	12.91***	12.87***
	(0.34)	(0.34)	(0.34)	(0.87)	(0.87)	(0.86)
Age:						
25–34 years old	–	–	–	–	–	–
35–44 years old	-1.83***	-0.72*	-0.64	-4.14***	-2.40**	-0.24
	(0.40)	(0.40)	(0.40)	(0.95)	(0.96)	(0.95)
45–54 years old	-5.97***	-2.48***	-2.34***	-7.16***	-2.91***	0.24
	(0.45)	(0.45)	(0.46)	(0.92)	(0.96)	(0.96)
Household structure:						
Male × married		-4.32***	-4.40***		-7.71***	-9.47***
		(0.51)	(0.51)		(1.29)	(1.27)
Female × married		-25.91***	-25.93***		-24.32***	-24.98***
		(0.53)	(0.53)		(1.03)	(1.03)
Male × parent		6.50***	6.59***		10.01***	12.04***
		(0.50)	(0.50)		(1.27)	(1.25)
Female × parent		4.58***	4.59***		3.07***	3.46***
		(0.56)	(0.56)		(1.01)	(1.00)
UI benefit level:						
1st tercile	–	–	–	–	–	–
2nd tercile			3.66***			-6.75***
			(0.40)			(0.90)
3rd tercile			-0.12			-17.53***
			(0.42)			(0.88)
Constant	74.43***	72.27***	71.02***	39.46***	36.74***	42.96***
	(0.35)	(0.40)	(0.45)	(0.83)	(0.95)	(1.05)
Number of UI spells	65037	65037	65037	15361	15361	15361

Notes: Estimated coefficients from regressions of long-term benefit receipt on demographic and UI characteristics in a 2 percent sample of UI claimants who exhausted benefits in the indicated years. The outcome variable is an indicator for receiving long-term benefits within 30 days of UI exhaustion. Terciles of UI benefit level are based on benefit level at the time of exhaustion and stratified by sex, East/West residence, marital and parental status, and year of UI exhaustion. The post–Hartz IV sample excludes 2005 and 2006 because the data provider reports concerns about the quality of data on long-term benefit receipt in these years. Robust standard errors are reported in parentheses. *: $p < .10$. **: $p < .05$. ***: $p < .01$.

Appendix Table 4: Alternative measures of potential UI benefit duration

	All claimants (1)	**Veterans** (2)	**Newcomers** (3)
Ex post PBD measure	344.58	321.26	388.40
	(153.95)	(148.82)	(153.85)
Ex ante PBD measure	345.73	316.95	399.82
	(154.74)	(147.02)	(154.40)
Corr(ex post, ex ante)	0.90	0.90	0.89
Ex post PBD = ex ante PBD	0.68	0.62	0.81
Discrepancy $\leq$ 30 days	0.76	0.72	0.84
Number of UI claims	336,634	219,725	116,909

Notes: The "ex post" measure of potential UI benefit duration calculates PBD based on the completed duration of each UI claim, together with a variable indicating the number of unused benefit-days remaining at the end of each spell. The "ex ante" measure calculates PBD based solely on information observed by the start of a new UI claim. I subdivide the sample into UI "veterans"—claimants who have received UI sometime in the 7 years preceding the current claim—and UI "newcomers", who have not. The ex ante measure is likely to perform better for newcomers than for veterans, as complex carry-forward provisions make it difficult to accurately determine UI eligibility for recurrent UI claimants.